PAPERS

ON

MECHANICAL AND PHYSICAL SUBJECTS.

London: C. J. CLAY AND SONS,
CAMBRIDGE UNIVERSITY PRESS WAREHOUSE,
AVE MARIA LANE.
Glasgow: 50, WELLINGTON STREET.

Leipzig: F. A. BROCKHAUS.
New York: THE MACMILLAN COMPANY.
Bombay: E. SEYMOUR HALE.

PAPERS

ON

MECHANICAL AND PHYSICAL SUBJECTS

BY

OSBORNE REYNOLDS, F.R.S., MEM. INST. C.E., LL.D.,
PROFESSOR OF ENGINEERING IN THE OWENS COLLEGE, AND
HONORARY FELLOW OF QUEENS' COLLEGE, CAMBRIDGE.

REPRINTED FROM VARIOUS TRANSACTIONS AND JOURNALS.

VOLUME II

1881—1900

CAMBRIDGE:
AT THE UNIVERSITY PRESS.
1901

Cambridge:
PRINTED BY J. AND C. F. CLAY,
AT THE UNIVERSITY PRESS.

PREFACE TO VOLUME II.

THIS Volume includes the Reprint of my papers on mechanical subjects, following on to those printed in Volume I., from the year 1881 up to date.

At the expressed wishes of the authors this Volume also includes the Reprint of the second parts of two papers, of which I contributed the first parts only.

One of these is the paper "On the Theory of the (Steam-Engine) Indicator and the Errors in Indicator Diagrams." Of this the second part was contributed by Professor A. W. Brightmore, D.Sc.

The other being the paper "On the Mechanical Equivalent of Heat," and of this the second part was contributed by Mr W. H. Moorby, M.Sc.

OSBORNE REYNOLDS.

19, LADYBARN ROAD,
MANCHESTER.

CONTENTS OF VOL. II.

PAGES

41. *On the Fundamental Limits to Speed* . . . . . 1—24

The different limits imposed by the several properties of material—the limits determined by the ratio of strength to heaviness—stresses due to acceleration in the coupling rods of locomotives—the destructive effect of periodic forces synchronising with the natural period of the structure—the extent to which balancing of machines is possible.

42. *On an Elementary Solution of the Dynamical Problem of Isochronous Vibration* . . . . 25—27

43. *The Comparative Resistances and Stresses in the Cases of Oscillation and Rotation with Reference to the Steam-Engine and Dynamo* . . . . 28—50

The friction in the two cases—the dynamics of oscillations controlled by a crank—effect of reservoirs of energy—loss of energy by friction resulting from pressures caused by inertia—application to the steam-engine and dynamo.

44. *An Experimental Investigation of the circumstances which determine whether the Motion of Water shall be direct or sinuous, and of the Law of Resistance in Parallel Channels* . . . . 51—105

Section I.—Introduction.

The failure of the theory of hydrodynamics to explain why the resistance is in some cases proportional to the velocity, and in others to the square of the velocity—direct and sinuous motion—the effect of viscosity—character of motion dependent on dimensional properties—the evidence of these in the equations of motion—experiments by means of coloured bands in glass tubes prove the existence of a critical velocity at which eddying motion begins—two streams in opposite directions—experiments showing the resistance is constant if $v \propto \mu/\rho \,.\, c$—results shown to agree with both Darcy's and Poiseuille's experiments . . . . 51—67

Section II.

Description of the experiments in glass tubes by means of colour bands—relations between critical velocity, size of tube and viscosity—the sudden appearance of the eddies—effect of initial disturbances—effect of size of disturbance on instability . . . . 68—77

Section III.

Experiments to determine critical velocity by measuring the resistance—the apparatus—methods of measuring the discharge and the pressures—effect of temperature—results—general law of resistance brought out by the method of logarithmic homologues . . . . 78—98

PAGES

Section IV.

Application to Darcy's experiments—their reduction by means of logarithmic homologues—effect of temperature when the critical velocity is passed—effect of roughness of surface of pipes . . 99—105

45. *The Transmission of Energy* . . . . 106—131

Directed and undirected energy—the sources of energy—transmission by means of coal and corn—transmission of stored energy in the directed form—distribution by means of compressed water, compressed air, ropes and shafts.

46. *On the Equations of Motion and the Boundary Conditions for Viscous Fluids* . . . . 132—137

The equations of motion shown to be at variance with the boundary conditions—modification of the equations to satisfy the boundary conditions.

47. *On the General Theory of Thermo-Dynamics* . . . 138—152

Joule's law and Carnot's ideal engine—experimental illustration of the second law of thermodynamics by mechanisms working by means of undirected energy—the limits of the steam-engine—possibilities of the gas-engine.

48. *On the Two Manners of Motion of Water* . . . 153—162

The inadequacy of the theory of fluid motion to account for the actual behaviour of fluids—internal motions seen by introducing colour bands—conditions for steady motion—converging channels generally steady, and diverging channels unsteady—parallel streams steady below and unsteady above a certain velocity—effect of viscosity.

49. *On the Theory of the Steam-Engine Indicator and the Errors in Indicator-Diagrams* . . . . . . 163—180

Requirements that the diagram may be exact.—The disturbances on the pencil—(1) disfigurement of the diagram caused by the inertia of the mechanism—(2) the friction of the pencil—the general effect to increase size of diagram.—The disturbances on the drum—(1) inertia of the drum—(2) varying stiffness of the spring—(3) the friction of the drum—shortening of diagram and reduction of mean pressure.

49A. *Experiments on the Steam-Engine Indicator* . . . 181—202

Description of the apparatus—testing of the springs—effect of oscillations of spring—stretching of indicator-cord.

50. *On the Dilatancy of Media composed of Rigid Particles in contact. With Experimental Illustrations* . . 203—216

Any change of shape causes a change of volumes in a granular medium—equal spheres arranged as a pile of shot have a density $\sqrt{2}$ times as much as when arranged in a cubical formation—condition of maximum density very stable—friction tends to increase stability—experiments with sand and shot contained in bags—dilatancy of media a possible explanation of the force of attraction—also of cohesion and chemical combination.

PAGES

51. *Experiments showing Dilatancy, a Property of Granular Material, possibly connected with Gravitation* . . 217—227

52. *On the Theory of Lubrication and its Application to Mr Beauchamp Tower's Experiments, including an Experimental Determination of the Viscosity of Olive Oil* . . . . 228—310

Section I.—Introduction.

Discordance of experimental results—Mr Tower's discovery of the separating film of oil, &c.—the idea of a hydrodynamical theory of lubrication—the equation of lubrication mentioned before Section A of the British Association at Montreal, and subsequently integrated—the comparison of the theoretical results with experimental shows a temperature effect—determination of the variation of viscosity of olive oil brings the theory into complete accordance with experiments, and shows how various circumstances affect the results—the difference in the radii of brass and journal, and the point of nearest approach of brass to journal, and explanation of increased heating on first reversal—the limits of complete lubrication, incomplete lubrication, and necking—the general arrangement of the paper . . . . 228—234

Section II.—The Properties of Lubricants.

Definition of viscosity—the character of viscosity—the two viscosities—experimental determination of the value of μ for olive oil, &c., Figs. 2 and 3, Table I.—the comparative values of μ for different fluids and different units . . . . 234—242

Section III.—General View of the Action of Lubrication.

The case of two nearly parallel surfaces separated by a viscous fluid—the case of revolving cylindrical surface—the effect of a limited supply of lubricating material—the relation between resistance, load, and speed for limited lubrication—the conditions of equilibrium with cylindrical surfaces—the wear and heating of bearings . . . 242—258

Section IV.—The Equations of Hydrodynamics as Applied to Lubrication.

The complete equations for interior of viscous fluid simplified—the boundary conditions—the first integration of the resulting equations, equations of lubrication—the conditions under which further integration has been undertaken . . . . 258—262

Section V.—Cases in which the Equations are Completely Integrated.

Parallel plane surfaces approaching each other, the surfaces having elliptical boundaries—plane surfaces of unlimited length . . . . 262—265

Section VI.—The Integration of the Equations for Cylindrical Surfaces.

General adaptation of the equations—the method of approximate integration—integration of the equations . . . . 266—273

Section VII.—Solution of the Equations for Cylindrical Surfaces.

c and $\sqrt{\frac{a}{R}}$ small compared with unity—further approximation to the solution of the equations for particular values of c, Figs. 18, 19 and 20—$c = {\cdot}5$ the limit to this method of integrating . . . . 273—282

Section VIII.—The Effects of Heat and Elasticity.

μ and a are only to be inferred from experiments—the effect of the load and the velocity to alter a—the effect of speed on the temperature—the formulæ for temperature and friction, and interpretation of constants—the maximum load at any speed . . . . . 282—289

PAGES

Section IX.—Application of the Equations to Mr Tower's Experiments.

References to Mr Tower's reports, Tables I., IX., and XII.—the effect of necking the journal—first approximation to the difference in the radii of the journal and brass No. 1—the rise in temperature of the film, owing to friction—the actual temperature of the film—the variation of a with the load—application of the equations to the circumstances of Mr Tower's experiments, Table IV.—the velocity of maximum carrying power—application of the equations to determine the oil pressure with brass No. 2—conclusions . . . 289—311

53. *On the Flow of Gases* . . . . 311—320

Experiments show that the flow of gas from one vessel into another is independent of the pressures when their ratio is greater than two, whilst according to the theory the flow diminishes and finally ceases as that ratio is increased—this anomaly due to the assumption made in the theory—that the pressure at the orifice is the pressure in the receiving vessel at a distance from the orifice—the assumption avoided by the integration of the fundamental equations of fluid motion.

54. *On Methods of Investigating the Qualities of Lifeboats* . 321—325

By using models made to scale according to the laws of dynamic similarity, experiments could be made in ordinary weather to correspond to the severest storms with full-sized lifeboats.

55. *On certain Laws relating to the Régime of Rivers and Estuaries, and on the possibility of Experiments on a small Scale* . . . . 326—335

The action of water to raise or lower the beds of rivers and estuaries—shown to depend on the character of the motion, and to be independent of the magnitude or velocity of the stream—tidal rivers—experiments on a model of the Mersey Estuary—resemblance of the contours to the charts of the Mersey.

56. *On the Triple-Expansion Engines and Engine-Trials at the Whitworth Engineering Laboratory, Owens College, Manchester* . . . . 336—379

Description of the engines, boilers, and connections—the appliances for measuring the condensing water, and hot-well discharge—arrangement of indicators—the specially designed hydraulic brakes—their advantages over friction brakes—objects and methods of conducting the trials—the checks on the heat and water afforded by the surface condenser—the radiation—method of combining the diagrams to show the proportion of steam condensed at all points in the expansion—the missing quantity and the effect of the steam jackets in reducing it—the relative effect of the jackets in the three cylinders—mean results of the trials.

57. *Report of the Committee appointed to investigate the Action of Waves and Currents on the Beds and Foreshores of Estuaries by means of Working Models* . . . 380—409

Experiments to determine how the distribution of the sand is affected by the horizontal and vertical dimensions of the models, and the tide period—description of the models and the tide generators—method of surveying—conditions of experiments—results—conclusions—plans.

PAGES

58. *Second Report of the Committee appointed to investigate the Action of Waves and Currents on the Beds and Foreshores of Estuaries by means of Working Models* . 410—481

Experiments to ascertain the law of the limits for dynamic similarity—critical values of the criterion of similarity—general distribution of sand in V-shaped estuaries—effects of land water—the automatic tide gauges—description of the experiments—plans.

59. *Third Report of the Committee appointed to investigate the Action of Waves and Currents on the Beds and Foreshores of Estuaries by means of Working Models* . 482—518

V-shaped estuary with a long tidal river—possible condition of instability—tides varying from spring to neap—effect of training walls—effect of groins—general results of the investigation—description of the experiments—plans.

60. *On Two Harmonic Analyzers* . . . . 519—523

An instrument for detecting and identifying periodic vibrations in structures—an appliance for setting up vibrations in a structure so as to find its natural period.

61. *Study of Fluid Motion by means of Coloured Bands* . 524—534

Experiments showing how the internal motions, otherwise invisible, are shown by the use of colour bands—the small resistance to wave motions—internal fluid motion generally a process of mixing—an experiment showing a straight vortex illustrating how internal waves can exist without motion on the outside boundary.

62. *On the Dynamical Theory of Incompressible Viscous Fluids and the Determination of the Criterion* . . . 535—577

Section I.—Introduction.

Stokes' dissipation function and the author's determination of the critical velocity of water—considerations which show that the criterion follows from the equations of motion—basis of the method of analysis—summary of conclusions of the investigation . . . . . . 535—544

Section II.

The mean-motion and heat-motions as distinguished by periods—mean-mean-motion and relative-mean-motion—discriminative cause and action of transformation—two systems of equations—a discriminating equation . . . . 544—563

Section III.

The criterion of the conditions under which relative-mean-motion cannot be maintained in the case of incompressible fluid in uniform symmetrical mean-flow between parallel solid surfaces—expression for the resistance . . . . 563—577

63. *Experiments showing the Boiling of Water in an Open Tube at Ordinary Temperatures* . . . . . . 578—587

Explanation of the hissing noise in the kettle—reduction of pressure in a contracting channel sufficient to cause boiling—conditions necessary as explained by the author's determination of the critical velocity of water.

PAGES

64. *On the Behaviour of the Surface of Separation of Two Liquids of different Densities* . . . . . . 588—590

65. *On Methods of Determining the Dryness of Saturated Steam and the Condition of Steam Gas* . . . 591—600

The conditions of steam in Regnault's experiments—wet steam—wire-drawing calorimeters—theory of the reductions—the erroneous assumption that the specific heat of steam above the temperature of saturation is constant and equal to that of the steam gas in Regnault's experiments—the possibility of obtaining an accurate estimate—means of assuring the final condition, that of steam gas.

66. *On the Method, Appliances and Limits of Error in the direct Determination of the Work Expended in Raising the Temperature of Ice-Cold Water to that of Water Boiling under a pressure of* 29·899 *inches of Ice-Cold Mercury in Manchester* . . . . 601—733

PART I.

The standard of temperature in Joule's determination—description of the experimental steam-engine and other appliances used in this investigation—the brake and the possible errors due to fluctuations of the speed and the turning moment—the cyclic variations of speed—thermometer scales avoided by working between the standard temperatures 32° and 212°—the additional appliances required—conduction and radiation of heat—the standards of length, mass, and temperature—air in the water—the specific heat of the water—complete table of the corrections for all circumstances affecting the accuracy of the results . . . . 601—657

PART II.

Details of the several parts of the apparatus and measuring appliances—the system of conducting the trials—comparison of the thermometers—adjustments of the brake—leakages—details of the different series of trials—the correction . . . . 658—733

67. *On the Slipperiness of Ice* . . . . 734—738

An explanation afforded by the author's theory of lubrication which shows that continuous rectilinear motion is one of the only two cases in which complete lubrication is possible.

41.

ON THE FUNDAMENTAL LIMITS TO SPEED.

I.

[*From* "The Engineer," Oct. 28, 1881.]

AMONG the facts which are so familiar to us as not to command our attention are the limits to the rates at which we can move over the surface of this earth, or, to put it more generally, the limits to the rate at which terrestrial objects can move. Everyone is now familiar with the fact that railway trains do not exceed sixty or seventy miles an hour; that steamboats do not exceed twenty-five miles an hour; carriages on ordinary roads, ten or twelve. The fastest running animals rarely exceed a mile in two minutes, or the fastest bird a mile a minute. That there are circumstances on which these limits depend must be generally recognised; but, while speed is the highest of our mechanical ambitions, how many of those who find themselves confined for nine hours between London and Edinburgh have ever asked themselves, why should there be a limit to speed at all?

In the early days of railroads the question as to the possibility of exceeding the speed of animals was very prominent; and many of the immediate circumstances on which possible speed depends—such as the strength and elasticity of the machine, and the smoothness of the road—have since received due attention. This was a matter of necessity, just as, in attempting to gain a higher standpoint on the side of a hill, account must be taken of the difficulties of the ground immediately above one. But such notice is a very different thing from a general survey of the limit imposed by the height of the hill itself. While we were still in the valley, and the immediate difficulties of ascent were great, our aspirations might well fall short of the top of the hill, which would not then become an object of attention. But having toiled up a great way, and having apparently reached a flat, or nearly flat, plane on which we are wandering without making any considerable ascent, it cannot but be a matter of interest and importance to make a more general exploration, and endeavour to ascertain what is the nature of the country behind and above the clouds which surround us.

The greatest speeds attained have not increased now for many years. It is probable that the run from Holyhead to London is still the fastest journey ever accomplished over so long a distance, although the number of instances in which this speed is approximately reached are now numerous, and continually increasing. With animals there is no great alteration—why should there be? And with machines, locomotives or steamboats, the improvement is that the average speed more nearly reaches the maximum, rather than any extension of the maximum. Noticing this, we cannot avoid the surmise that the obstruction to further advance arises from something more fundamental than mere economy or imperfection of mechanical contrivance. The question as to how far this is the case must admit of an answer if the circumstances can be subjected to a complete theoretical examination. The problem is very complicated, and it may well be doubted whether our knowledge of the circumstances and possibilities of art is sufficient to enable us to arrive at a definite conclusion. But what we may do is to look, in the first instance, for any circumstance which imposes a definite limit to possible speeds, and having investigated the law of this limit, look for other limits, and having examined each separately, endeavour to arrive at the result when they are taken in conjunction.

To begin with, it will be well to try and catch sight of the top of the hill from a distance. Going far away from the complexity of our immediate problem, we may ask whence there can be any limit to possible speeds? Any limitations in the circumstances on which speed depends would cause a limit to speed and, although perhaps not very obvious, consideration will show that speed depends on certain physical and mechanical properties of material, and that these are essentially limited. Thus the strength of material is limited. Some materials are stronger than others, but the strength of the strongest is easily reached, and although improvement in art brings the stronger and more appropriate materials within reach, still by no tittle have we been able to extend the strength of the strongest beyond what it has been, so to speak, fixed by nature. When compared by heaviness, natural tissues are the strongest materials. A silk cord will sustain more than a steel wire of the same weight, and such a wire is the strongest form of any manufactured material. To the limited strength, as compared with the weight of material, then, we may look for a limit to possible speeds; and this is not all. There are other limits—for instance, the limited temperature at which material retains its strength; in fact, the properties and powers of material are essentially limited in all directions, and, inasmuch as speed s on these properties, it must be limited. If we take a somewhat 'iew, the immediate conclusion is that there are at least two distinct of a limit to speed. The first and most obvious of these is that the ıce to motion requires that the moving object should be continually orward by a force, and the maintenance of this force requires additions weight of the moving object, which additions increase the resistance; at a certain speed there will be a balance between the resistance and ce, any increase in the force causing a still greater increase in the ıce.

s may be illustrated by reference to a railway. The resistance of the is the addition necessary to maintain the motion. Taking the best the resistance of an engine at high speeds is about 45 lb. per ton of ;ht. If, then, the locomotive weighs 20 tons it would require a steady 900 lb. to balance its resistance. To maintain this force a certain e of steam must act on the pistons. To keep up this pressure the rs must be filled and emptied every revolution of the driving wheels— ›ry 26·4 ft., or 200 times per mile. To maintain the speed then the must supply steam enough to fill the cylinders 200 times per mile, whatever time the mile is run. Now the power of supplying steam by ler is limited. A boiler of a certain weight cannot be made to supply han a certain amount of steam, and if we know the shortest time :h the boiler will produce 200 cylinders full of steam at the pressure d to move the engine, we know the shortest time in which it could nile, or the limit of speed arising from this source. To increase the the boilers would be to increase the weight and consequent resistance engine, so that the only chance of extending the limit is to increase ım-producing power of the same weight of boiler; and the question r this actual limit has been reached is a question as to whether there nains, after all these years, room for improvement in the best boilers— r, in fact, the steam-producing power of boilers has reached the limit d by the limit to the strength and other properties of material of they may be constructed.

› case of the locomotive has been introduced here merely for the sake ıtrating the fact that, however distant, there is a limit to possible arising from this source. As a matter of fact this limit is not actually l, for, as will be subsequently shown, there are other and inferior which come in; that is when the engine is running without a train, en the train is added, as it must be from an economical point of view, ıe steam-producing power of the boiler does impose an economical ı the speed of the train.

› case of steamboats is somewhat different. With these the resistance increases in a high ratio with the speed, as the square of the speed, so that not only have the cylinders to be filled at a rate proportional to the speed of the boat, but to maintain the requisite force the size of the pistons or the pressure of the steam must increase as the square of the speed; so that instead of being, as with the locomotive, nearly in the simple ratio to the speed, the quantity of steam required in a given time varies as the cube of the speed. Thus, in the case of steamboats, the steam-producing capacity of a certain weight of boiler is the source of the actual as well as the economical limit to the speed. This limit has been reached with the modern steam launch and torpedo boat, in which as much as two-thirds of the whole weight of the ship are given up to the engines and boilers; the highest speeds so attained being about twenty-five miles an hour. The action of this, which may be called the physical limit to speed, may be traced in animals, but the requisite data for its discussion are wanting. The second fundamental source of limits to speed is the strength of the parts, and the forces holding these parts, necessary to withstand the forces to which the motion gives rise. This may be called the dynamical limit to speed.

This source of limit has received less general notice than the preceding. That the motion of machines and animals necessarily gives rise to forces in and between their parts is not perhaps very obvious, on account of its being so well known that motion itself does not give rise to force between the parts of a moving object. But this is only when the motion is rectilinear and uniform. To stop and start a body or to change its direction requires force proportional to the weight of the body and the rate at which the change is made. In order to realise how all possible motions on the earth are limited, it must be noticed that uniform rectilinear motion is impossible. Objects on the earth have to maintain their motion against such resistances as they encounter by the relative and limited motions of their parts; with animals by the motion of their legs, wings, or fins; in machines by the motions of their pistons, cranks, and wheels; and, even apart from this, uniform motion is impossible owing to the impossibility of maintaining a direct course—for instance, a perfectly even road.

The limit to the speed of any complex body, such as an animal, an engine, or even a revolving wheel, will depend primarily on the manner in which the general motion depends on or involves change in the speed or direction of motion of any or all of the parts. For example, in the case of all carriages the limit to the strength of the tires of the wheels would limit the speed if there were no inferior limit. That what is called centrifugal force tends to burst the tires must be universally known; but there is a simplicity about the law of this limit which marks it out as the best illustration of the class of limits which arise from acceleration.

The bursting tension of the tire caused by the revolution of the wheel is the result of the centrifugal force acting on each elementary portion of the tire, and is the same as if the tire were subject to an outward pressure equal to the centrifugal force all over its inner surface. The dynamical problem of estimating the centrifugal tension from the weight, diameter, and speed of revolution of the tire is not difficult, but it will be sufficient here to state the result. The tension per square inch of section of the tire is ·37 multiplied by the weight of a cubic inch of the material and the square of the velocity in feet per second. The limit of speed is that which causes a centrifugal tension equal to the greatest stress the material will safely bear. With iron this is about 15,000 lb. per square inch. A cubic inch of iron weighs ·24 lb., so that the velocity squared is equal to $11 \times 15{,}000$ or 165,000, or, roughly, the velocity equals 400 ft. per second. This, which is 270 miles an hour, is the limit arising from centrifugal force to the safe velocity; for steel tires, the strength of which is about double that of iron, the limit becomes 380 miles an hour. It should be noticed that neither the diameter of the wheel nor the thickness of the tire makes any difference to this limit, which depends solely on the ratio between the strength and heaviness of the material. If we could get a stronger material, then we might extend the limit, but as natural fibres are the only materials stronger than steel, and these do not possess the hardness necessary for tires, there is absolutely no prospect of any extension in this direction.

The velocity of the train is the same as the velocity of the tire, so that the figures given above show the limit to the velocity of the train arising from the centrifugal force on the tire—that is, supposing the tire subject to no forces but those considered. Looked at in this way, the limit appears well away from any speeds already realised. But as the tire is subject to forces arising from its contact with the rail and from the load on the wheel, the margin left for centrifugal force is much less than what has been stated, so that the actual limit, which involves complex considerations, is really much lower.

Wheels have been here considered as affording the simplest example of how changes in the direction, or speed of motion in the parts, of a moving object must cause a limit to the speed at which the object can move, and not because the wheels are the parts which would give way first were the speed to be increased. In the locomotive, as at present constructed, there are parts—the coupling and connecting rods, for instance—which would give way under these accelerations before the tires; and it will be the object in a subsequent article to discuss somewhat fully the limit to speed imposed by these, as well as by other parts of the machinery.

In the case of animals there are no wheels, but the problem does not differ greatly; for the forces required to stop and start the limbs tax the strength of these in much the same way as the strength of the tires is taxed by centrifugal force. So that the conclusion is the same, that the strength, as compared with the heaviness, of the material of the bones and tissues of animals determines a limit to the possible speed; which conclusion is borne out by the fact that the strength, as compared with the heaviness of these materials, is as high, or higher, than that of any other materials—the strength being that required to resist the particular forces which the parts are generally called upon to sustain, *i.e.*, bone to resist crushing, and sinews to resist tension.

Before closing this article, which is intended as an introduction to the more definite discussion of certain particular cases where these limits come in, it should be pointed out that besides the two sources of limits to speed which have been particularly noticed, viz., those which arise from the strength of the material, and those from the limited capacity of producing energy, there are other sources of limits. One of these, of a physical kind, is the inability to get rid of the heat produced at the joints by friction. The heating of bearings, which is a very common source of the actual limit to speed, although it has not apparently received much attention except in a practical way, admits of theoretical consideration as being subject to definite laws.

Another source of the limit to speed, of the greatest practical importance, although more complex than the preceding, is the effect of the moving pieces and the forces between these to cause unsteadiness to the motion of the whole structure. The difficulty of keeping a railway train steady has perhaps as much to do with the actual speed attained as any other cause. In so far as this unsteadiness arises from the unevenness of the road, and the mere disturbing forces caused on the frame by the moving pieces, it belongs to the class of dynamical limits, but it depends on a particular property of matter not involved in other cases of this class of limits. The rocking of a structure depends on the character of its elasticity, and on the period as well as the magnitude of the disturbing forces; and, as a matter of fact, the tendency to vibrate would impose a limit on the speed of most machines, so that it is entitled to a place amongst the sources of limit, and may be called the elastic limit.

So far, then, we see that there are four distinct sources of limits to speed. The limited capacity of producing energy, the limited strength of the material, the limited power of discharging the heat produced by friction, and the elastic limit. In pointing out the general nature of these limits, attention has been directed to objects with powers of locomotion as being more familiar; there are, however, the same sources of limits to the speed of stationary machinery, such as steam-engines and tools.

II.

[*From* "The Engineer," Nov. 18, 1881.]

To obtain an idea of the effect of accelerations, we may take an instance of a moving machine, and supposing its speed to increase, consider which of its parts would give way first. The locomotive seems to afford the best example. Imagine, then, a locomotive to be started down a long incline with the steam fully on; what part of the machine would give way first? In the case of an engine with its wheels coupled, the question may be answered with certainty. The coupling rods would be thrown off. Although perhaps not generally known, this has been shown both theoretically and practically. Anyone with the smallest mechanical insight, observing from a distance a coupled engine in motion, cannot fail to perceive that the rapid up-and-down motion of these rods, which are held only at the ends, must call for great strength to prevent them breaking in the middle. That the strength so called for approaches the actual strength of the rods can, of course, only be ascertained by definite calculation. Six years ago the case of one of these rods was taken as an example, to illustrate to the engineering class at Owens College the effect of accelerations, and the result of the calculation then made was to show that the strength called for when the engine was running at 70 miles an hour was nearer the limit imposed by the actual strength of the material than is usually considered safe in estimating the size of such structures. Thus, instead of 10,000 lb., the stress amounted in this example to 15,000 lb. The fact was surprising enough to arrest attention, and raise a question as to the considerations which had led to the proportions of these rods. On reference to the text-books and manuals it was found that the effects of accelerations had no place in them, so that it would appear that engineers have had no rule to go by but that of experience; or, in other words, that the dimensions of these rods have been arrived at by the process of trial and failure. All these facts considered, the matter seemed one of no small mechanical interest. For apart from the importance of these rods and the desirability of supplying a theoretically derived formula in place of empirical rules, the experience of the fitness of these rods has been so ample, that as soon as we are in a position to calculate the stresses in their material, they furnish a very important test as to the factor of safety for such parts of machinery. Thus, it appears that while a rule has been laid down that a certain stress is the greatest which the iron in any important part of a machine should bear, these very important parts have been unwittingly allowed to bear, and have borne safely, half as much again as that given by the rule. That the stress in these rods may be as great as appeared from theoretical consideration, or, at least, that they are the parts of the engine which first give way when an undue speed is attained, has been confirmed by the records of railway accidents. Shortly after the first investigations were made, a train having on it three similar coupled engines ran away down an incline, the brakes being overpowered, and eye-witnesses described how the first symptom of disaster was the flying off of the coupling rods from one of the engines, those from the others following immediately after. In 1878 attention was called to these facts at a meeting of the Manchester Literary and Philosophical Society, and they excited the interest of Dr Joule, who has kindly sent the author published accounts of several instances of the failure of these rods in cases of high speeds. Amongst these was the following extract from a letter published in the *Manchester Courier*. The accident occurred on the Cheshire line from Manchester to Liverpool, on which the speeds are very high. The author of the letter has clearly used the term connecting rod in the sense of coupling rod. "Shortly after we had passed one of the small stations on the way, and before reaching Warrington, the connecting rod of the engine, or some other material portion of that part of the mechanism, became broken, and flew off with such force as to strike the embankment on the near side, and thence rebound with terrible power into the window of one of the third-class carriages immediately behind, completely smashing in the woodwork, as well as all the glass, to the great danger of one or more passengers within, but who escaped uninjured. I was a passenger on another occasion, on the same journey, when the connecting rod snapped in two, and the two pieces continued to whirl round until the train could be stopped, to the great risk of driving the engine and carriages from the metals. And I have heard it said that accidents of a similar kind have occurred on other occasions."

The theory of these rods has been taught in the engineering classes at Owens College for several years, but its first appearance in print seems to have been in a letter in *The Engineer* of May 27th, 1881, signed "S. R.," dated Manchester, May 11th; and more fully in an article which appeared in *The Engineer*, of Sept. 9th, 1881. Leaving what we may call the swinging forces out of consideration, the coupling rods are designed to withstand certain forces which cannot exceed a definite amount. This amount may be estimated for each particular case. The utmost one rod can be called upon to do is to turn one pair of wheels against the whole friction between the wheels and the rail, which latter may be sanded. In such a case, F, the coefficient of friction, would be about ·3. Let R be the radius of the wheels in inches, L the length of the cranks, P the pressure between the wheel and the rail in pounds; then taking T for the force in pounds, tension or compression, in the rod necessary to cause the pair of wheels to slide when the other rod is in the line of centres,

$$LT = FRP,$$

or

$$T = \frac{FRP}{L}.$$

T may be either tension or compression, but it is the latter that is the most important for the present consideration. If now we take the swinging action into account, we have to add the effect of the vertical force which must act on each point of the rod in order to change its vertical motion. Relatively to the engine each point of the rod will describe a circle exactly similar to that described by either crank pin. In describing its circle each portion of the rod will be subject to centrifugal force. Consider a cubic inch of material of weight w, the centrifugal force of this by the well-known formula is

$$C = 12\frac{wv^2L^2}{gLR^2} = 12\frac{wv^2L}{gR^2}.$$

Where v is the velocity of the engine in feet per second, and $g = 32{\cdot}2$ the acceleration of gravity. The direction of the centrifugal force will be parallel to the line joining the centre of the crank shaft with the centre of the crank pin, and consequently will be vertical and directly across the rod when the cranks are vertically up or down.

We have then a force C, acting upwards or downwards, on each cubic inch of the rod. When the cranks are down this force must be added to the weight of the rod, which will then act in the same direction. Then the effect to break the rod will be the same as if the engine were standing, and the weight of the material of the rod were increased in the ratio

$$\frac{C + w}{w}.$$

So that as regards this force the rod may be considered as a loaded beam. Let the rod be of uniform section of length H, area S, and depth $2y$, also let K be the radius of gyration of the section. Then the load on the rod is $(C + w)\,SH$, and the greatest bending moment in inch-lb. is

$$(C + w)\frac{SH^2}{8} = M,$$

and if f be the greatest stress in the rod, for the resistance to bending we have the well-known formula—

$$M = \frac{K^2Sf}{y}.$$

Comparing these two values of M we may determine f the stress due to centrifugal force in terms of the velocity of the crank pin—

$$f = \frac{C+w}{8}\frac{H^2 y}{K^2}.$$

We have thus two independent forces, to which the material of the rod is liable. The bending moment M and the thrust T, the stress caused by T distributed uniformly over the section would be $\frac{T}{S}$. Therefore the stress due to both these causes is equal to—

$$f + \frac{T}{S},$$

and
$$f + \frac{T}{S} = \frac{c+w}{8}\frac{H^2 y}{K^2} + \frac{T}{S},$$

which formula will give the greatest stress which one of these rods is subject to when the dimensions, speed, and material are known. As an example let us suppose,

$$H = 108 \text{ in.},\quad y = 2\tfrac{1}{4},\quad S = 7{\cdot}87,\quad w = {\cdot}28,\quad L = 8\tfrac{1}{2},$$

$v = 100$ (70 miles an hour nearly),

$R = 39$ in., $P = 26880$ (12 tons), $F = {\cdot}3$.

Substituting these quantities, which correspond to the dimensions of an express passenger engine on the North British Railway described in *The Engineer*, Vol. L., 1878, and we find

$$f = 11357,$$

$$\frac{T}{s} = 4700,$$

so that the greatest compressive stress in the rod when the engine is running at seventy miles an hour is 16,000 lb. per square inch. This stress is applied and reversed from tension to compression every revolution of the wheel, so that the fact that these rods do safely withstand these stresses affords sufficient proof that the material of which they are composed will safely bear a repeated load of 16,000 lb. on the square inch. As they are constructed, however, these rods clearly impose a limit on the possible speed of the engine, and a limit very close to that which is actually attained by passenger engines. There is no necessity, however, that this limit should be so low. The simple bar form which is usually that given to these rods is about the worst shape they could have to resist the centrifugal forces. By making them hollow or with flanges, it would be perfectly easy to extend the limit considerably without adding to the weight of the rods.

The coupling rods are those parts of a locomotive in which the accelerations produce the greatest effect, but all the reciprocating parts of the engine are subjected to similar forces. The connecting rods differ from the coupling rods, in the fact that it is only one end that swings, and hence that the effect of the acceleration varies from nothing at the piston end to the value given by the formula at the crank end. Thus while the coupling rods may be regarded as a beam loaded uniformly, the connecting rods are subject to loads varying uniformly from the piston to the crank end. But the result will be the same, and the liability of the connecting rod to break under its swinging action would impose a limit to the speed were it not for the inferior limit imposed by the coupling rods. Let alone the swinging motion, the mere reciprocation would impose a limit to speed. Thus to stop and start the piston and its attachments requires a force which is given by the same formula $\frac{Wv^2L}{gR^2}$, L now being the length of the crank, and W the weight of the piston and its attachments. At moderate speeds these forces are small compared with the forces produced by the pressures of the steam, but increasing, as they do, as the square of the speed, they soon leave the others behind. By diminishing the lengths of the cranks in proportion to the diameters of the wheels, and the consequent amplitude of reciprocation, the accelerations are proportionally diminished; but then, in order to transmit the same power, the size of the pistons and the dimensions of all the parts must be proportionally increased, and then the heating of the bearings comes in to limit the speed. Thus with high speeds of pistons the forces arising from reciprocation limit the speed, while with low speeds the difficulty of the bearings limits the speed. There is, therefore, a middle course between these two extremes, and it is this medium course to which experience has led, although the determining causes have been but very imperfectly recognised.

So far the accelerations spoken of have been those which result from the regular motion of the internal parts of the engine. But in the case of all carriages there is another class of accelerations, which, although less regular, act a similar part in causing a limit to the speed, and which follow the same laws. These are the vertical accelerations which arise from the inequalities of the road. If the road be uneven—as all roads are, more or less—the wheels, and to some extent the carriages, move up and down according to the inequalities. This up and down motion, although not regular, necessitates up and down accelerating forces, which will be proportional to the square of the velocity of the carriage, so long as no limit comes in to prevent the wheels following the inequalities of the road. The upward acceleration is caused by the pressure of the road on the wheel, and the limit to this is obviously one of strength. So long as neither the road nor the wheel gives way, the motion must ensue. But the downward acceleration can only result from the force of gravitation acting on the wheel, and the pressure exerted by the carriage to keep the wheel down. Where springs are used this pressure will be maintained nearly constant, whatever the acceleration may be; but without springs the greatest acceleration is that of gravity—for the carriage will have to follow the wheel in its vertical motion, and the greatest acceleration is when they are both free to fall.

If W be the weight of the wheel and C the load of the carriage, then, without springs, the greatest acceleration is 32·2, or g; but with efficient springs it is $\frac{C+W}{W}g$. When the speed of the carriage is such as to require an acceleration greater than this in order to keep the wheel in contact with the road, the wheel will bound. This practically limits the speed of carriages without springs on ordinary or paved roads to three or four miles an hour; but with springs there is no difficulty in attaining speeds equal to the highest that horses can maintain.

The use of the level iron rails maintained in their proper position diminishes the vertical motion to such an extent, that there is no difficulty from this cause at the highest speeds attained even at the present day. But the difficulty of maintaining the rails, and particularly the ends of the rails, in their places, is considerable, and one misplaced rail becomes a source of danger, so that it cannot be said that the vertical accelerations exercise no influence on the limit of speed. This action, however, must not be confused with the liability of the train to rock, which, although depending on the unevenness of the road, depends rather on the frequency of the inequalities than on their magnitude; and further, as has already been pointed out, depends on the elastic properties of the train. This rocking will be considered in another article.

III.

[*From* "The Engineer," Dec. 9, 1881.]

ALTHOUGH vibration is one of the greatest and most common difficulties with which engineers have to contend, it is, perhaps, of all mechanical phenomena least understood. It does not appear to have been made the subject of any treatise, or to have a place in works which treat of applied mechanics. This has doubtless arisen from the great apparent diversity in the circumstances under which it occurs. The mechanical principles involved are sufficiently well understood by natural philosophers; but they have not been applied to the practical questions. Such an application is, however, not only possible, but the general circumstances on which vibrations depend may be apprehended without the aid of mathematical symbols. In fact an unconscious apprehension of the principles of vibration is one of the earliest lessons which children learn. The act of swinging in a child's swing requires such a knowledge, and this whether swinging oneself by motion in the swing, or swinging another by pushing the swing. And the same may be said of shaking an apple tree. The act of shaking a tree does not consist simply in exerting a force first in one direction and then in the opposite. One might do this, exerting many times the force necessary, if properly applied, to bring not only the apples but the leaves off the trees, without bringing down a single apple. What is required besides the alternating force is that the alternations should be timed right. This timing of the alternations in the direction of the force we exert comes naturally when we are trying to shake an object; for naturally we follow the object in its motion; indeed it is difficult to avoid doing this. But if, instead of shaking the tree by muscular exertion, we were to arrange a steam-engine to shake it, then we should at once perceive that there was only one particular speed of the engine at which the tree would shake. The general phenomenon, the apprehension of which has been wanting to the understanding of the circumstances on which vibration depends, is that the discovery of which led Hooke to perceive the mechanical law which bears his name—"*ut extenso sic vis*"—and also led him to construct a watch after the present method. This phenomenon is that a fixed object will, when set in motion to a greater or less degree, continue to rock in a particular direction, with a particular, and only with that particular, rate of oscillation. This is no less true of ships, bridges, and parts of machines, than of apple trees, tuning-forks, and the balance-wheels of watches. We say continue to rock; but it is not meant that it will continue for ever, or for any great length of time. The motion will gradually diminish, according to the resistance encountered from the air and the imperfect elasticity of the structure.

The rate at which a structure will rock depends on two circumstances—the stiffness of the attachments by the bending of which the rocking takes place, and the magnitude and distribution of the weight to be rocked. In the case of short, stiff objects, like the prongs of a tuning-fork, the vibrations may amount to hundreds per second; whereas in the case of trees, ships, bridges, or steam-engines, they are often as low as two or three per second, or even one in two or three seconds.

The period in which a body will continue to rock in any manner may be called its period of free vibration for that manner of rocking; and having recognised the general existence of such periods of free vibration, a general view of the circumstances under which dangerous vibrations are likely to occur is not difficult. Were it not for the decadence of the free vibration when once set up, owing to such causes as have been already mentioned, then it is obvious that if to the swing already attained a small addition were made, the increased swing would continue, and by continually adding fresh swings, however small, the swing must eventually increase until some limit was reached. Thus, one child swinging another, if there were no retardation, would, if it continued to impart a push, however slight, each time the swing passed, eventually send the swing completely round. As it is, however, owing to the retardation arising from the resistance of the air and the stiffness of the ropes, the work done by the swinger only just balances the energy lost, and so only maintains the speed; the greater the speed the greater the work spent in retardation, and hence the greater the exertion on the part of the swinger necessary to maintain it. Now, the theory of all steady vibration is the same; whatever may be its nature, there must always be something to act the part of the swinger, and by well-timed acceleration make good the necessary loss. In order that the extent of vibration may be constant, the added velocity must be exactly what is lost; if it be too great the amplitude will increase, or if too small, diminish. There are several things which may thus act the part of the swinger—any reciprocating or revolving weight, any periodic force, such as may arise from the intermittent pressure of steam on the piston, or a periodic motion, such as is caused by the wheels of a carriage running over the setts on the street or the sleepers on a railway; in fact, any periodic disturbance. But as a matter of fact, such disturbances have always a fixed period, and as the body will only oscillate in a fixed period, it is only in the case when the period of the disturbance exactly fits the period of vibration that this vibration can be steady, and this rarely or never happens. What really happens is that the period of disturbance approximates more or less to the period of vibration, and in order to understand the theory of vibrations under consideration it is necessary to consider how a difference in the periods influences the vibration. The swing will enable us to do this. Suppose the period of the swing to be two seconds, and suppose that the swinger pulls a rope every 2·05 seconds; the first pull will set the swing in motion a little; in the second swing the pull will come a little late, but still before the forward motion has ceased, which will be ·5 second from the start. The second pull will therefore accelerate the motion, and so will the eight succeeding pulls. After this, however, the pull will come on the backward motion and exercise a retarding effect, and by the time ten such pulls have been given the retarding effect will have just balanced the previous accelerating effect, and all motion will have ceased. We see, then, that the result would be waves of vibration, ten effective pulls, and as much motion as these would impart, and then ten retarding pulls, destroying the motion. The number of effective pulls clearly depends on the approximation of the period of the pulls to that of the swing. If this had been only ·01 second different, then we should have had fifty effective pulls and a corresponding motion. The magnitude of the motion attained will thus depend on two things—the magnitude of the disturbing force or pull compared with the weight on the swing, and the number of effective pulls of which the discrepancy of the periods admits, which number will be the whole period divided by four times the difference of the two periods. This result, which is obvious in the case of the swing, is equally true for all classes of vibration. When the period of a disturbing or swinging force differs from the natural period of swing, the result will be batches of oscillations increasing from nothing till they reach a magnitude depending on the magnitude of the disturbing force, and the ratio of the natural period to the difference in the two periods, the number of swings before the maximum is reached being equal to one-fourth this ratio, which will also be the number while the motion is diminishing. It thus appears that if the periods approximate, a comparatively small disturbing force must produce a considerable swinging, while if the difference in the periods is large, then the amount of motion will be confined to that produced by the action of the single disturbance. In the latter case the vibration is called a forced vibration. Of course, when large disturbing forces are allowed, forced vibrations may become important, but this seldom occurs, as large disturbing forces may generally be avoided. Small disturbing forces, however, are almost always present where there is periodic motion, and though the forced vibrations which would result are unimportant, when these, owing to the near coincidence of their period with that of force vibration accumulate, motion of almost any extent may ensue. It is this near coincidence of the period of the disturbance or free vibrations with the period of free vibration which is the condition of danger from vibrations, and the possibility of avoiding the danger lies in the possibility of avoiding this approximate coincidence. This may be attempted in two ways, one by adjusting the period of disturbance, the other by constructing the structure so as to adjust the period of free vibration wide of that of the disturbance. The first of these methods is seldom applicable, for the period of disturbance is generally determined by the speed of revolution of some part of the machinery, and which speed must vary between nothing and the highest which the limit arising from vibration or some other cause will allow. It is, therefore, to the construction of the structure that we must look, in order to prevent the period of disturbance from reaching that of free vibration. The period of free vibration in any structure may, and generally will, be different for different directions of rocking, even when the structure rocks as a whole on its supports; and when the structure consists of many parts with more or less elastic connections, all of these parts may have different periods of rocking. Thus each branch of a tree if shaken separately would swing in a different period from the tree as a whole, and each apple in a different period from the branch; so that if we attempt to shake the stem in a wrong period for the whole tree, we shall probably succeed in shaking some branch.

Without going too deeply into the mechanics of the subject, we may look on a structure or a part of a structure as a solid mass on elastic supports; and then there will be in all six independent ways in which it can vibrate or swing, all of which may have different periods. There will be three linear motions—for instance, up and down, north and south, east and west, or in whatever directions these may be, they must be at right angles to each other; and three circular or rotary vibrations about these axes at right angles. Owing to a want of pliancy in the supports perceptible rocking is seldom possible in all these directions. For instance, if we fix a hammer with a long shaft in a vice, pinching the bottom of the shaft with the head upwards, then the head may oscillate in two ways. If the broadest way of the handle be east and west, then the head if set swinging would swing in one period east and west, another north and south, but owing to the rigidity of the shaft there could be no perceptible motion up and down; also the head might have a rotary motion about a vertical axis by twisting the shaft, but the shaft would not allow of the head having any perceptible rotary motion about any horizontal axis. In saying that these are the only three directions of oscillation, it is not meant that the body cannot be set off oscillating in other directions, but that it will not continue to oscillate in other directions if started. In the case of the hammer, the head might be set swinging south-east, but it would then change its manner of swing until it moved in a circle, such a motion being equivalent to two motions in conjunction, one north and south, the other west and east, which would have different periods, and so the corresponding phases would change.

These distinct periods, which are easily conceived in the case of the hammer, will exist more or less in all structures. In the locomotive, for instance, the boiler is capable of rocking on its springs, with a lifting up-and-down motion, or with a rolling motion from side to side, or with what is called a bucking motion, one end rising and the other falling; these three motions will have different periods. And to avoid oscillations in these directions it is necessary that these periods should be such as not nearly to coincide with that of the machinery when this is moving fast. But it is not only the rocking of the engine as a whole that has to be considered; every part of the engine will be capable of free periodic motion, and should the period of any forced vibrations rise into coincidence with any of these periods, the part to which it belongs will be in danger. Such a coincidence is only to be avoided when all the free periods are smaller than the period of any disturbing force at the fastest speed of the engine, for since as the engine acquires motion, the period of disturbance gradually diminishes; this must come into coincidence with any period of free vibration which may be greater than that of the period of disturbance when at its smallest. The periods of vibration of any structure may be diminished by increasing its stiffness, or the stiffness of its supports or attachments, but there is a limit to the stiffness possible, so that the structure may fulfil its functions. For instance, the springs of the locomotive have to allow the wheels to adapt themselves to the inequalities of the road, and if they are too stiff they will fail to do this.

In this way it is seen that there must be a limit to the possible smallness of the period of free vibration of the structure and its parts, and hence to the speed of the structure, on which the smallness of the period of disturbance will depend. The stiffness of structures has for the most part been determined by experience, and any further extensions of speeds will require increased stiffness, and this throughout the structure; for in any complex structure, such as a locomotive or a railway carriage, there are so many parts of which the free periods are small—the floor, roof, the sides, seats, and partitions—that as the period of disturbance becomes smaller, nothing but a general stiffening of the entire structure will prevent destructive vibrations. Doubtless the parts may be made stiffer than they are—and this without materially increasing their weight—which would call in other limits to speed. Much has been done of late years, imperfectly as the theory has been understood. This is very apparent when we compare the smoothness of the motion of one of the present northern express trains with what it was some few years ago. The carriages are, however, only stiffened up to the normal speeds, any excess of speed becoming apparent by the tremour or vibration which ensues; and even at the normal speeds there is room for further improvement, in the accomplishment of which careful attention to the foregoing considerations should be of the greatest use.

IV.

[*From* "The Engineer," *March* 17, 1882.]

THE inertia of the moving parts of a machine besides calling for strength in the parts themselves, as, for instance, in the tires of wheels, often calls for restraining forces in the supports to prevent the moving parts changing their position. Such forces exerted on the frame when they exist will, like those in the moving parts themselves, increase in the ratio of the square of the speed, and hence the possible speed would in such cases be limited by the strength of the attachments or supports of the frame. As a matter of fact, the disturbing forces on the frame constitute one of the commonest difficulties in the way of attaining high speeds, and demand the most careful consideration at the hands of engineers. These forces cannot, like the forces in the moving parts themselves, be considered as fundamental, since, except for the complications involved, it is always possible so to arrange the moving pieces of a machine that their inertia shall cause no resultant disturbance on the whole frame and its supports, the forces being confined to the moving pieces and those portions of the frame which connect them. To accomplish this counterweights have to be employed, and in some cases it would be necessary to add additional moving pieces, the only function of which would be to oppose the inertia of the parts which are necessary for the primary purpose of the machine. When this is done the machine is said to be perfectly balanced. There are, however, many considerations relating to the balancing of machines which have nothing to do with a complete balance, for, as will be presently explained, such a balance is often impracticable.

The general theory of a complete balance involves two conditions: (1) in order that there may be no force to move the frame in any particular direction, or that there may be no tendency to move the centre of gravity of the frame; (2) that there may be no tendency to turn the frame round about its centre of gravity. The condition (1) may be simply expressed. The moving weights must be so arranged that, however the several weights may move, the centre of gravity of the whole system of moving pieces must not change its position during the motion. The condition (2) may also be simply expressed in the language of theoretical mechanics. It is that the moving weights must at no time have any aggregate moment of acceleration about any axis through the centre of gravity. To those who are not familiar with mathematical language, this second condition as thus expressed may not be very intelligible, nor is it easy to express the complete condition in more general language; but as the practical examples are for the most part very simple, it will be sufficient to explain the condition as applied to one of these examples. Suppose the moving parts consist of two equal weights. Then the first condition involves that the accelerations on these weights shall be equal and in opposite directions, *i.e.*, if the acceleration on the one is north, that on the other must be south. But this first condition does not require that the centres of gravity of the two weights shall be opposite one another in the direction of acceleration; this, however, constitutes the second condition. For instance, in the case of a crank shaft in uniform rotation, the centre of gravity of the shaft itself, lying in the axis, will not move; but the centre of gravity of the crank revolving round the shaft will be subject to continual acceleration, directed from the axis. An equal weight fixed at an equal distance from the axis, and on the opposite side to the crank, will suffice to satisfy the first condition, however far along the shaft it may be from the crank; but to satisfy the second condition, the centre of gravity of the counterweight and of the crank must be in a line perpendicular to the axis of the shaft; and since the connecting rod occupies the space opposite the crank, it is in general impossible to balance a crank with a single weight, two weights having to be used, placed so that the centre of gravity of the whole mass on one side of the crank shaft shall be opposite to the centre of gravity of the mass on the other.

The moving parts of machines consist in general of revolving pieces, such as crank shafts, and oscillating pieces, such as pistons and connecting rods, the motion of which is derived from, or governed by, a revolving crank.

In the case of the revolving pieces, a complete balance may always be effected in each piece by the addition of counterweights on the piece itself. Thus, as far as a crank shaft in a locomotive is concerned, apart from the connecting rods, pistons and other moving parts attached to it, the addition of suitable weights on the driving wheels will satisfy both conditions and prevent any disturbance on the frame arising from the revolution of the crank shaft. Oscillating pieces, however, cannot be balanced in so simple a manner. They require a weight or weights of which the centre of gravity is in the line of oscillation, and oscillating in exactly the reverse manner. Now, the manner of oscillation of, say, a piston depends not only on the motion of the crank, but also on the length of the connecting rod, the varying obliquity of which, when the connecting rod is short, will produce an important effect. The only way, therefore, in which a connecting rod and piston can be completely balanced is by oscillating weights connected with cranks on the crank shaft, by connecting rods of such length that their obliquity is always the same as that of the connecting rod which drives the piston. In this way, however, a complete balance may be effected. That it is rarely or never done is owing to the complexity and increased friction attending such an arrangement, which renders it in other ways a greater evil than the disturbances on the frame which it prevents. Practically, then, it comes to this—that revolving pieces may be completely balanced; but, as regards oscillating pieces, the balance cannot be made complete.

In default of a complete balance, there remains the question as to the desirability of an imperfect balance, or what may be better described as the introduction of other forces, so as to modify the resultant force on the frame. The practical possibility of such modifications is limited by considerations of complexity and friction to the addition of certain weights to the crank shaft, which introduce forces in one direction equal to those which they balance in the direction at right angles. But for the effect of the obliquity of the connecting rod, the force arising from the acceleration of the piston in the direction of its motion will at all times be the same as the component in that direction of the centrifugal force of an equal weight revolving with the crank and having its centre of gravity in the axis of the crank pin. The centrifugal force of the revolving weight, however, would not be confined to the direction of oscillation, so that if such a weight be used to balance the piston, it will introduce an equal force at right angles to the direction of oscillation. Thus if weights be added to the driving wheels of a locomotive of such magnitude as to balance not only the weights of the cranks, but also weights equal to the connecting rods and pistons, having their centres of gravity in the crank pins, the only horizontal forces will be those which arise from the effect of the obliquity of the connecting rods on the motion, while vertical forces will have been introduced nearly equal to what the horizontal forces arising from the pistons and connecting rods would have been. The effect of smaller balance weights is to leave more of the horizontal forces unbalanced, and introduce less vertical force. Such is a sketch of what may be called the practical possibilities of balancing machines, which, like a steam engine, involve oscillating pieces.

There will, therefore, always be disturbances in the frame, unless they are prevented by the strength of the supports, but it is possible to so arrange counterweights as to mitigate these forces in one direction by introducing equal forces in a direction at right angles. The problem as to how far it is desirable to do this, is that which the practical engineer has to solve, and which, owing to a multiplicity of considerations, can in reality only be solved by experiment. There are, however, several leading considerations, a general apprehension of which should much facilitate the task.

When there is nothing to limit the firmness and stiffness that can be given to the frame and its supports in the directions in which the forces which arise from the inertia of the moving parts tend to move it, there is but little inconvenience arising from these forces. Thus in a stationary steam engine founded in the earth steadiness may be obtained by weight and solidity of foundations, almost the only drawback being the expense entailed and the space occupied.

But when an engine has to be carried by a floor or on any structure more or less elastic, then the case is different, and it becomes a question of the greatest importance in which direction disturbing forces will produce the least and in which the most harmful effect, it being desirable as far as possible to balance the forces in the latter direction at the expense of forces in other directions.

It is not, however, simply a question of stiffness, as it may happen from various reasons that equal forces caused by the revolution of the engine might do more harm, and under certain circumstances even cause greater disturbance in that direction in which the supports are stiffest. The consideration of the circumstances on which vibrations depend, as described in the preceding article, at once shows that the directions in which the greatest disturbance of the frame is likely to result from forces caused by the revolution of the engine, are those directions in which the period of free vibration of the frame on its supports nearly corresponds to the period of revolution of the engine. And where there is any direction in which such a near coincidence occurs, it is an absolute necessity that in this direction the balance should be as nearly perfect as possible. It is often impossible to ascertain beforehand in which direction such a coincidence may be expected, but its existence at once declares itself upon the engine being put to work. Indeed, wherever an engine or revolving machinery causes a visible swinging vibration, it is in consequence of such a coincidence of periods, and the oscillations will be found to occur in batches, the magnitude of the oscillations and the number in each batch increasing as the speed of the engine approaches some particular value. This may be seen in many cranes. In such structures the period of oscillation depends upon the load suspended, and hence it will often be seen that while the engine which works the crane will run quite steadily when the crane is unloaded, when loaded, decided oscillations are set up; or it may be just the other way, and oscillations occur when there is no load, while the structure is steady when the load is on. In almost all such cases the oscillations might be prevented by counterweights so placed as to alter the direction in which the forces occur.

Such oscillations are, as has been said, to be feared chiefly in cases where the frame of the engine is carried on elastic supports. All engines supported on springs—as portable or traction engines—are liable to them, as are also marine engines, owing to the elasticity of the ship. And in these cases it is only in avoiding such oscillations that counterweighting has to be studied. In some cases, however, notably that of the locomotive, it is not only in causing such oscillations as ensue when the directions and periods of free vibration and of the unbalanced forces coincide, that such forces are harmful. In the locomotive, although the frame of the engine rests on elastic supports, namely, the springs, yet the revolving piece—the crank shaft with the driving wheels, whence alone can arise vertical unbalanced forces—rests on the rails, which afford a very rigid support in a downward direction, and to prevent upward motion there is the axle-box with the weight of the locomotive upon it. Unbalanced weights on the crank shaft cannot therefore cause in a vertical direction such oscillations as have just been considered; but they give rise to other evils. If the centrifugal force is sufficient it will lift the axle-box against the pressure of the spring, causing the wheel to leave the rail on to which it will return with a blow, causing what is known as hammering; while short of this a want of vertical balance will cause the wheel to run with varying pressure or tread upon the rail, which will cause the wheel to wear out of the round, even if the pressure be nowhere sufficiently relieved to allow the wheels to slip. Of these evils hammering must be avoided, *i.e.*, the speed of the engine must not reach the point at which this begins, and the load and speed should not be so great as to cause slipping. But the wear arising from unequal tread is not so serious an evil but that it may be faced as an alternative for other evils. A certain limited want of balance in the vertical direction is thus permissible.

In a horizontal direction the character of the support of the engine and crank shaft is altogether different. In this direction the crank shaft is held to the frame of the engine by the axle-boxes, but the frame of the engine resting on the wheels has no backward and forward support at all, except such as is derived from the elastic drawing apparatus which connects it with the train. While as regards twisting about a vertical axis which causes the engine to run with a sinuous motion, the only support is that derived from the comparatively loose fit of the flanges of the wheels between the rails. Thus any want of balance in a horizontal direction causes the engine to move forward with an uneven motion or with a sinuous motion on the rails. The last of these evils is the worst, but they are both bad according to their degree.

As we have seen, the motions of the pistons and connecting rods introduce horizontal forces such as will produce one or other, or generally both, these evils. These horizontal forces can only be diminished by counterweights on the driving wheels, which introduce vertical forces equal in magnitude to those which they balance. It is a question, therefore, between two evils—unsteady horizontal motion or unequal tread. Experience has shown that, up to a certain point, the latter evil is the least, and that it is better, at least in part, to balance the horizontal forces. As to the exact degree in which this should be done practice differs. Nor is there sufficient data on which to lay down a general rule; but the circumstances which in each case should determine the balance weights are to be inferred from the foregoing considerations. The limit to the counterweights lies in the inequalities which they cause in the pressure of the driving wheels on the rails; and hence the permissible magnitude of these inequalities is what should be ascertained in order to determine the balance weights. Or, in other words, what is wanted to be known is the greatest proportion to the gross load on the driving wheels that the vertical component of the centrifugal force may be practically allowed to bear, and the counterweight might then be designed so as to produce this force when the engine is running at its normal speed; unless, indeed—as would never happen—such weight was more than sufficient to balance the horizontal forces.

This method of arriving at the best counterweight is the only logical one. The usual custom appears to be to balance a certain proportion of the horizontal forces. This, however, is not logical, since but for the vertical effect of the counterweights, the more perfectly the horizontal forces are balanced the better, and there is no fixed ratio between the horizontal forces and the load on the driving wheels which determines the allowable magnitude of the vertical forces. The common rules, too, as to the distribution of the balance weights, are apt to be faulty, for by these rules the distribution of counterweights is to be such as would completely balance weights centered in the crank pins bearing a certain proportion to the oscillating weights. So that not only does the imperfection of the horizontal balance produce irregularities in the forward motion of the engine, but it also produces a twisting or sinuous motion. Now, whatever proportion of the horizontal weights may be balanced, the counterweights may be so placed on the wheels as entirely to prevent the twisting or sinuous motion. The rule, therefore, as far as it is possible to state it, should be to use the largest counterweights which the load on the driving wheels will allow, and to distribute it so as to balance all tendency to turn the crank shaft about a vertical axis.

In the case of coupled engines, the balance weights should obviously be equally distributed between the wheels, so that the inequality of wear may also be distributed. In these engines it is a common custom to make the coupling rods and the crank pins which carry them act the part of counterweights for the pistons and driving cranks, by placing the coupling crank pins on the opposite side of the shaft to the driving cranks. This effects a considerable reduction in the actual counterweight, but this appears to be the only point gained, while in order to reduce the forces and friction on the journals, the coupling rod crank should be on the same side of the shafts as the driving cranks, so that the forces transmitted to the coupling rods may not be transmitted through the journals and thus cause increased friction on the bearings.

The disturbing effect of the inertia of the oscillating parts forced itself into notice very early in the days of the locomotive, and immediate good was found to result from the use of counterweights, which, by increasing the steadiness, allowed higher speeds to be attained. It does not appear, however, that any systematic attempts to determine the best arrangements of the balance weights have been recorded. Experiments have been made from which certain conclusions have been drawn, but the subject has not received the treatment which its importance deserves. This is doubtless because the investigation is one which involves the long-continued control, in certain respects, of locomotive engines, while those who have had this control have not been able to devote unclouded attention to this subject.

If a railway company would engage the assistance of one of the highly-qualified young engineers to be found at the present time, giving him the control of the balance weights and a sufficient number of locomotives, and power to watch the results, both as regards steadiness and wear, for a considerable period, not only would they be amply repaid, but they would earn the gratitude of all locomotive engineers, and, indeed, of the travelling public.

42.

ON AN ELEMENTARY SOLUTION OF THE DYNAMICAL PROBLEM OF ISOCHRONOUS VIBRATION.

[*From the Twenty-second Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester," *Session* 1882–83.]

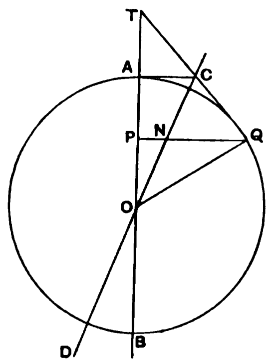
(*Read* November 14, 1882.)

WHEN a heavy body is free to move in one direction, subject only to a force which is proportional to the distance the body has moved from some neutral position, and tends to return the body to that position, the body will, if set in motion, vibrate about the neutral position in a period which is independent of the magnitude of the motion.

The deduction of this theorem from the laws of motion, although well known, is generally accomplished by the solution of a differential equation. In some text books this is avoided by comparing the law of force on the vibrating body with that of a component of the centrifugal force on a revolving body; this method involves no mathematical difficulties, but it is indirect and hides rather than removes the dynamical difficulties. My own experience has shown me that the mathematical difficulty or obscurity of these methods stand very much in the way of those who are commencing the study of practical mechanics, in which vibration and oscillation play a part of fundamental importance. It was with a view of meeting the requirements of such students that I sought for a method involving only Elementary Mathematics, in which the solution depended directly on the principle of the conservation of energy. Having succeeded in finding such a method, which, although it bears a superficial resemblance to the method of the text books already mentioned, so far as I am aware, has not hitherto been published, it seems that it may be useful to publish it.

The method is to show that the vibrating body will at all times be opposite, in a direction perpendicular to its path, to a body revolving uniformly in a circle, having a diameter equal to the amplitude of oscillation, with a velocity equal to the greatest velocity of the vibrating body.

Let O be the neutral position of the body considered as a point, AOB the path described during oscillation, let p be the force on the body when at a unit distance from O, so that as the force varies uniformly with the distance, px will be the force at the distance $OP = x$.



Take PN perpendicular to OP and make PN on some scale equal px, then N will lie on a straight line COD, and the area OPN will represent the work U done against the force in moving from O to to P and

$$U = \frac{PN \times OP}{2} = \frac{px^2}{2} \quad \text{.........(1).}$$

Let W be the weight and v the velocity of the body, then by the conservation of energy

$$\frac{W}{2g} v^2 + U = \frac{Wv_0^2}{2g} = E \quad \text{.........................(2),}$$

where E is the energy of the system and v_0 the velocity at O, or substituting from equation (1)

$$\frac{Wv_0^2}{2g} = \frac{Wv^2}{2g} + \frac{px^2}{2} \quad \text{.............................(3);}$$

but when P is at A or B, $v = O$; put $OA = a$, then

$$pa^2 = \frac{Wv_0^2}{g} \quad \text{.................................(4),}$$

and equation (3) becomes

$$\frac{W}{g} v^2 = p(a^2 - x^2) \text{.............................(5).}$$

Describe a circle about O as centre with a radius a, and let PN produced meet this circle in Q, and let QT the tangent at Q meet AB in T, then the triangle QTP will be similar to OQP and

$$\frac{PT}{TQ} = \frac{PQ}{OQ} \quad \text{................................(6).}$$

Now suppose a point at Q moving with a velocity u such that it keeps opposite to P.

Then the component of this velocity parallel to AB is

$$u\frac{PT}{TQ}=u\frac{PQ}{OQ},$$

and this is v since Q remains opposite to P.

Therefore substituting in equation (5)

$$\frac{W}{g}u^2\frac{PQ^2}{OQ^2}=p(a^2-x^2).$$

Then since $PQ^2=OQ^2-OP^2=a^2-x^2$ we have

$$pa^2=\frac{W}{g}u^2 \quad\text{..............................(7).}$$

Therefore Q moves on the circle with a velocity

$$u=a\sqrt{\frac{gp}{W}},$$

which is constant. Or the motion of the vibrating body will be such that it always keeps opposite in a direction perpendicular to its path to a body revolving in a circle of diameter equal to the amplitude and with the greatest velocity of the vibrating body.

This completely defines the motion of the vibrating body for starting from A the arc described by the vibrating body after time t is $a\sqrt{\frac{pg}{W}}\,.\,t$ and the vibrating body will be opposite.

The time of a complete oscillation will be the time taken to complete a revolution. If t is the time of oscillation

$$\sqrt{\frac{pg}{W}}\,t=2\pi.$$

Therefore the time of oscillation is given by

$$t=2\pi\sqrt{\frac{W}{pg}} \quad\text{..............................(8).}$$

43.

THE COMPARATIVE RESISTANCES AND STRESSES IN THE CASES OF OSCILLATION AND ROTATION WITH REFERENCE TO THE STEAM ENGINE AND DYNAMO.

I.

[*From* "The Engineer," *January* 5, 1883.]

1. THE two principal motions which are given to the parts of machines are uniform rotation and oscillation. These motions are both possible, and are both capable of performing mechanical operations; and the question as to why one or the other should be used gives rise to some interesting points. In some cases, as in that of the lathe, the general purpose of the machine renders one or other of these kinds of motion essential; but this is not so often the case as at first sight appears, for, if we consider, there are few operations performed by machines which cannot be performed in some way or another by animals, and continuous rotation is unknown in the animal mechanics. Nature has worked entirely by oscillation, so that the use of continuous rotation in machinery must be because, for some reason, it is preferred to oscillation. As to the reason for this preference, animal mechanics does not help us, for the constitutions of animals require a certain amount of continuity in the material throughout the entire animal, and this is inconsistent with continuous rotation. In machinery, however, this reason for the choice of reciprocation is altogether absent, and it has to compete with rotation on its merits in other respects.

2. The respects in which the motions of reciprocation and rotation may be compared are numerous, and sometimes complex; amongst the principal are adaptability to the operation, simplicity of construction, and friction.

3. The first two of these respects are those in which the relative merits of the two classes of motion are most obvious, and accordingly we may

expect to find that the choice of one or other class of motion generally turns on their relative adaptability to a particular operation, and the simplicity of construction of the mechanism involved. It may happen that in both these respects the same motion is to be preferred: but in many very important cases it seems that as regards choice of motion, adaptability to the operation is at variance with simplicity of construction: then there is rivalry between the two classes of motion, and the choice is not easy.

Thus we find that, although one or other class of motion has firmly established itself for certain purposes, there are a vast number of cases in which there has been and still is a contest more or less close. Illustrations are not far to seek. We find reciprocating and rotary pumps and blowing machines, reciprocating pressure engines and revolving wheels or turbines for obtaining power from water, reciprocating and rotary saws. We might say oscillating and rotary propellers, but the rotary motion seems to have established itself for steam boats, although the oscillating oar holds the advantage for manual labour. Numerous other instances might be given, but it will be sufficient to give two, and to these attention will be chiefly directed. The first is the steam engine, and the second the dynamo—electric machine and electric motor.

In the steam engine, although reciprocation has the best of it, the battle has never been given up. This is a case in which simplicity of construction is apparently, at all events, at variance with adaptability to the operation. In some cases, as in pumping engines, the operation involves or admits of reciprocation, and, as is well known, it was to such operations only that the steam engine was confined for about a hundred years after its invention. For these purposes it would naturally seem that the reciprocating motion was most applicable. But so little applicable to move revolving machinery did it appear, that when, after the lapse of a century, Watt improved the engine and saw the importance of applying it to revolving machinery, he kept his improvements waiting for something like ten years while he was attempting to find a revolving substitute for the reciprocating piston. At last he gave up the quest, and found in the crank, or his bastard form of it, a means of applying the reciprocating engine to purposes requiring revolution. But although abandoned by Watt, the quest has been and is still being followed by others. The apparently obvious advantage of a revolving engine, and the apparent simplicity of the problem, offer so tempting a field for invention, that probably nine out of ten of those who commence practical mechanics engage in it until they find how thoroughly others have been over the ground before them. So the reciprocating engine holds its own in the long practical test. This may be said to be on account of its simplicity of construction, and the adaptability of the reciprocating piston to the operation of taking the work out of the steam: still nothing approaching a satisfactory theoretical or scientific explanation of its advantage has been given. Thus the advantage of the reciprocating over the rotary steam engine stands almost entirely as an empirical fact, without explanation, and somewhat in opposition to what has been thought probable from scientific consideration.

On the other hand if we turn to the dynamo-electric machine, we see that the case is reversed. If there is an operation for which reciprocating motion appears to be adapted, it is the conversion of mechanical energy into electric currents, particularly into alternating currents, such as are best adapted for the electric light. In this operation there is something approaching to a necessity for continuity in the material, such as that which determines the motion in animal mechanics to be that of oscillation. Reciprocating motion would allow of continuity of material, whereas, in the case of continuous revolution, continuity in the conductors is only imperfectly secured by causing the stationary position of the conductor to press against the moving portion. Again, the *modus operandi* is to cause soft iron alternately to approach and recede from magnets, or to cause coils of the conducting wire to move so that the lines of magnetic force alternately pass inside and outside the coil.

The telephone acts by a reciprocating dynamo and motor, and its efficiency is such as to show how perfectly the motion of reciprocation is adapted for these purposes. Experience in the construction of the dynamo may as yet be called small: but while the records of the "Patent Journal" show that some of the most successful electricians have started with a belief in the adaptability of vibration, all the numerous successful machines have been rotary. It is probable that some reason for this has occurred to those most deeply engaged in the subject, but I am not aware that any has been publicly expressed: so that we may say that the advantage of rotary motion in the dynamo is an empirical fact, and is somewhat opposite to what might have been expected. It would seem, however, that this paradox is not so obscure as that of the advantages of the reciprocating engine; and it is not improbable that the explanation of the less difficult paradox may throw some light on that which has so long remained unsolved. In the case of the dynamo the considerations are much narrowed down, and hence the ground for advantage must be more distinct.

4. A careful study of the kinetics of the problem shows that there is one important respect not specifically dealt with in the treatises on the theory of machines, in which, as it would occur in the dynamo, reciprocation must be at a great disadvantage as compared with rotation. This respect is the third mentioned in § (2). Careful consideration shows that in the dynamo reciprocation must be at a great disadvantage as regards friction. This may

not appear to be unnatural, although the data and methods for investigating the friction of reciprocation, as compared with rotation, have not been formulated, there is a general impression that the balance would be against oscillation. Indeed, it is probable that this impression is one of the reasons which has led to the persistent attempts to produce a rotary steam engine. But such an indefinite impression entirely fails to explain why rotary motion should have an advantage under the circumstances of the dynamo which it has not under those of the steam engine, or why reciprocation should be at a disadvantage in the dynamo-electric machine when it is not in the dynamo of the telephone. Under these circumstances it appeared desirable to attempt a more definite study of the friction of reciprocation as applied to circumstances such as exist in the dynamo. This brings out facts which must be of great importance in the theory of machines, and which are altogether in the direction of explaining the foregoing riddles.

It appears that the amount of friction which has to be overcome in maintaining the motion of reciprocation of a particular piece of a machine controlled as by a crank, is not, as in the case of rotation, a quantity depending merely on the weight, manner of support, and motion of the reciprocating piece, but depends essentially on the forces which the reciprocating part is transmitting during its motion: and in general diminishes as those forces increase up to a certain point, when it vanishes. To take an illustration—in an ordinary steam engine doing full work it can be shown that the friction resulting purely from the motion being reciprocating is zero: but if the load be taken off the engine and the governors act so as to control the speed, the friction due to reciprocation will rise, and will reach a maximum when the engine is doing no work except driving itself.

The same would be to a certain extent the case in a crank-driven reciprocating dynamo. When moving unexcited, *i.e.* with the circuit open and doing no work, the resistance from the friction entailed by the reciprocating motion would be a maximum. When, by closing the circuit, resistance was thrown on to the machine, the work spent in friction from reciprocation would diminish, but it could not altogether vanish. In order that it might vanish altogether, the resistances encountered towards the end of the stroke must bear a certain relation to the weight and velocity, or more correctly, to the energy of motion of the reciprocating part, and this relation cannot be reached under the circumstances of the practical dynamo, in which the energy of motion of the reciprocating piece bears a much greater proportion to the work done than in the steam engine, and in which the resistances fall off at the end of the stroke. Thus while in the steam engine the lightness of the piston compared to the pressure which the steam exerts upon it at the commencement of the stroke, allows of its being driven at convenient speeds without entailing—when doing work—any extra friction from the reciprocation: in the dynamo, owing to the smallness of the resistance at the ends of the stroke compared with the weight of the reciprocating piece and the high speed required to develop the power, the friction entailed by reciprocation would be large.

In this comparison both machines are supposed to be controlled by the crank. The friction under such circumstances is not at all the same as when the reciprocating piece is controlled in other ways, as by a spring. In the telephone the motion is controlled by a spring, so that the same argument does not apply here. There are, however, certain limits to such a method of control, which it is not unimportant to consider. In order to render intelligible the reasoning relating to these points, it will be necessary to enter somewhat upon the kinetics of reciprocation, and this will form the subject of my next article.

II.

[*From* "The Engineer," *January* 19, 1883.]

5. THE object of the present article is the consideration of certain dynamical problems presented by the oscillating pieces of machines. In former articles under the head "Limits to Speed," it has been shown that the resistance called forth by the inertia of the revolving and oscillating parts of machines must, as the speed increases, reach a point beyond which the strength of material will not allow them to go. In this respect there is but little difference between revolving and oscillating pieces. But, as regards friction, or the work necessary to overcome friction, it will appear that oscillating pieces stand in a very different position to rotary pieces.

In applying the principles of mechanics to machines it is customary to treat separately the kinematical, or purely geometrical considerations, leaving all forces out of account. In this respect, *i.e.*, as regards the geometry of their motion, mechanisms, such as the crank and piston, which involve oscillating pieces, have received their due share of attention. But considerations relating to the forces in such mechanism have not received very systematic treatment. These considerations belong to two different classes, those which do not and those which do depend on the inertia of the moving parts. The first of these, although applied to moving bodies, are strictly statical, relating solely to the resolution and balance of forces; and it is this class which has received most attention. The considerations relating to the inertia of the parts have been much neglected. They constitute what, a few years ago, would have been called the dynamics of machinery, but what is now better expressed as the kinetics of machinery. In some few instances, as in the case of the fly-wheel of the steam-engine, inertia is necessary to the action of the machine; but with the majority of moving pieces the inertia only plays an incidental part in the action of the machine, or, in other words, the machine would get on better if these parts could be made of matter without inertia, and hence it has been very much the custom to leave inertia out of consideration.

This omission to consider the effect of inertia has been one of the main causes of the much complained of discrepancy between theory and practice, and it is to such considerations that we must look for explanations of the practical selection of one form of mechanism from amongst several, which, so far as kinematics show, appear to be equally applicable, as for instance, the reciprocating piston as against all forms of rotary engines. For some purposes the requisite motion is so slow that the inertia and energy of motion of the moving parts, and quantities depending on these, are so small as to be of no account, and then kinetic considerations are of no importance in determining the fitness of the mechanism; but whenever it is a question of attaining the highest possible speed, such considerations assume the first importance.

6. *The kinetics of oscillating pieces.*—If treated completely by integrating the equations of motion this would be a very difficult, if at all a possible, subject; only one case, that in which the motion is harmonic, has received much, if any, attention. And this case may be dealt with by the aid of elementary mathematics. By the laws of work and energy, however, the kinetics of oscillation are tractable, and the results so obtained are sufficient for the present purpose. The following notation will be used unless otherwise stated:—v is velocity in feet per second; W weight in pounds; E energy in foot-pounds; $g = 32$, acceleration of gravity. When a heavy body is subject to reciprocating motion its velocity will vary from some maximum value, v_0, to zero, so that E, the energy of motion is given by

$$E = \frac{Wv^2}{2g} \quad \text{....................................(1)}.$$

E will be greatest when v^2 is a maximum, and at its least when $v = o$, $E = o$; so that the body must lose and gain E_0 foot-pounds of energy of motion twice in each complete oscillation. In the case of the pendulum the energy of motion is transformed into energy of elevation, or when the velocity is zero the mass of the pendulum is $\frac{v^2}{2g}$ feet higher than when the velocity is v. But in other cases, as when a piston is controlled by a crank, the energy of motion is transferred to and from the vibrating body, or, in other words, the body must perform and receive work to the extent of $\frac{Wv_0^2}{2g}$ on each reversal of its motion.

7. It will be well to express this graphically. Let AOB be the path of the oscillating body, and suppose it to move from A to B, and to have

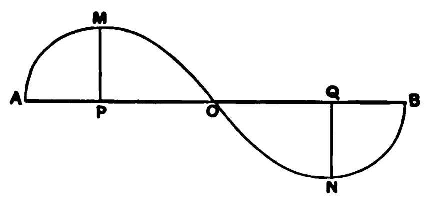


Fig. 1.

its greatest energy of motion E_0 at O. Then, since it starts from rest at A, before reaching O, it must have been subject to the action of forces which will do E_0 foot-pounds of work. These forces might, if their magnitude were known at each point P of the path, be represented as in the diagram of the steam indicator by distances PM perpendicular to AB. If so represented, the ends M of these distances would lie on some line AMO, which, with AO, would form the diagram of inertia, or of the force to balance inertia from A to O. The area of this diagram would represent the work done on the body, and would therefore represent E_0, the energy of motion at O. In the same way, since the body comes to rest at B, the body must have encountered resistance or opposing force against which it does E_0 foot-pounds of work in moving from O to B. This is represented by a diagram ONB, the area of which represents E_0 foot-pounds of work, and is therefore equal to the area OMA. Since the resistance from O to B is in the opposite direction to the force from A to O, N will be on the opposite side of AB to M. When the motion takes place from A to B the area AMO represents work done on the moving body to cause energy of motion, and the area ONB represents work done by the moving body to get rid of its energy of motion. Therefore, ONB would be negative work done on the body. When the motion is from B to A the area BNO represents work done on the body, and OMA is negative. Taking v_0 for the velocity at O and v for the velocity at any other point P, and supposing PM to represent the force to the scale p lbs. to a foot. Then, areas being in square feet,

$$p \times \text{area } AMO = p \times \text{area } ONB = \frac{Wv_0^2}{2g} \quad \ldots\ldots\ldots\ldots\ldots(2),$$

and $p \times$ area OPM represents the work from O to P or P to O. Then, by the equation of the conservation of energy we have

$$\frac{Wv^2}{2g} = p \times AMP \quad\text{..............................(3)},$$

$$\frac{Wv_0^2}{2g} = p \,.\, OPM + \frac{Wv^2}{2g} \quad\text{...........................(4)}.$$

In cases of reciprocation it is not easy to find either the force $p \,.\, PM$, or the velocity v at every point of the path, but one or other of these is always a direct circumstance of the motion. Thus, if the body move under the action of a spring, the stiffness of the spring determines the force $p \,.\, PM$, which is thus independent of every other condition. Or if the body be moved by a crank and connecting rod, as in the steam-engine, the velocity at each point is a kinematical consequence of the velocity of the crank. In every case, therefore, either $p \,.\, PM$ or v is a direct consequence of the circumstance of motion. Now, whichever of these may be the direct consequence, the other is a consequence of the equation of energy, or if we know the one as a direct consequence, we can find the other by the equation of energy.

8. *Oscillation controlled by a crank.*—In this case AB will be the diameter of the crank circle. Describe a circle with AB as diameter; then, neglecting the effect of the obliquity of the connecting rod, the position R of

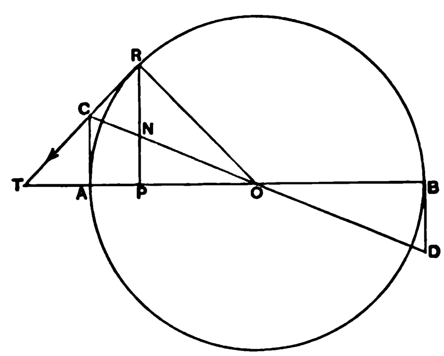


Fig. 2.

the crank on its circle, corresponding to the position P of the piston, is given by producing PN to meet the circle in R. Let RT be the tangent to the circle at R, then if u is the velocity of the crank pin at R, the velocity of P is

$$u \,.\, \frac{TP}{TR} = u \,.\, \frac{PR}{OR} \quad\text{..............................(5)},$$

whatever may be the position of P. If u is constant all round, when P is at O we have from (5)

$$v_0 = u \quad\text{.................................(6)},$$

and for every other point

$$v = u\frac{PR}{OR} \quad\text{...............................(7)}.$$

Substituting the equation of energy (4)

$$\frac{W}{2g}u^2 = p\,.\,OPM + \frac{W}{2g}\left(\frac{PR}{OR}\right)^2 . \, u^2,$$

or,

$$OPM = \frac{W}{2gp}u^2\frac{OR^2 - PR^2}{OR^2}$$

$$= \frac{W}{2gp}u^2\frac{OP^2}{OR^2} \quad\text{...............................(8)};$$

take

$$PN = \frac{W}{gp}\,.\,\frac{u^2 OP}{OR^2} \quad\text{...............................(9)},$$

then

$$\frac{PN \times OP}{2} = OPM \quad\text{..........................(10)}.$$

Join ON and produce it to meet perpendiculars through A and B in C and D. Then N must lie on the line CD for all positions of P, since by (9) PN is proportional to OP. Therefore by (10) the area OPM is equal to the area of the triangle OPN. Therefore M coincides with N on CD, and the force $p\,.\,PN$ is completely expressed. Put $OR = a$, then by (9)

$$\frac{PN}{OP} = \frac{W}{gp}\frac{u^2}{a^2} \quad\text{...............................(11)},$$

or writing e for $\frac{PN}{OP}$,

$$PN = e\,.\,OP \quad\text{...............................(12)}.$$

So that W, u, a, being known, we have e and PN for each value of OP.

9. *Oscillation controlled by a spring.*—The spring gives the force $p\,.\,PM$; take the usual case in which this force $p\,.\,PM$ is proportioned to OP; let

$$p\,.\,PM = peOP \quad\text{...............................(13)}.$$

Then as before M is on the line CD. And it is obvious that, the diagram of forces being the same as before, the relation between the force and velocity will be the same; but as, in the case already considered, the force is controlled by the motion, and in this case the motion is controlled by the force, it is well to make the two proofs independent.

Let R be a point moving on the circle so as always to be opposite to P, then, as before, we have

$$u\frac{PQ}{OQ}=v \text{ ....(14).}$$

And from the equation of energy (4)

$$\frac{W}{2g}\left\{v_0^2-u^2\left(\frac{PR}{OR}\right)^2\right\}=p.\left(\frac{OP.PN}{2}\right)=e.p.\frac{OP^2}{2}\text{ ....(15).}$$

When P is at A,

$$\frac{W}{2g}v_0^2=ep\frac{OR^2}{2}.$$

Therefore,
$$\frac{W}{2g}u^2\frac{PR^2}{OR^2}=e.p.\left(\frac{OR^2-OP^2}{2}\right)=e.p.\frac{PR^2}{2},$$

$$u^2=\frac{epg}{W}a^2=v_0^2\text{ ....(16).}$$

Equation (16) shows that u is constant all round the circle, so that in the case of a spring controlled weight the motion is such that P is always opposite a point R revolving uniformly with a velocity V_0. Thus the motion of P is completely defined.

The two cases which have been completely considered are cases of harmonic motion which may and have been dealt with by other methods. The method just given, as a matter of course, leads in these cases to the same results as other methods, but it has the advantage of being applicable to obtain certain results when neither the law of motion nor the law of force is completely defined, and, what is its chief advantage as regards the theory of machines, is that the kinetic forces are represented by a diagram, which may be at once combined with the diagram such as that of steam pressure, representing the forces acting on the oscillating pieces; and hence a complete diagram of transmitted forces obtained. The advantages of this will appear in the sequel; but first there are some other general points to be dealt with.

10. *Vibration and reciprocation.*—The two classes of oscillating motion typified by the two cases considered—namely, that controlled by the crank and that controlled by the spring, are, as regards the circumstances on which they depend, essentially different; and although custom is not uniform in the matter, it is well to distinguish them by different names. The class represented by the crank may be well called a motion of reciprocation, as the body is constrained to move backwards and forwards exactly along the same path and through the same distance, whatever may be the speed. Whereas in the case of a vibrating body, although it moves backwards and forwards along the same path, the distance depends on the speed. In the former

case, that of reciprocation, the only effect of increasing the speed of motion is to increase the rate of oscillation, whereas the effect of increasing the speed of motion in the case of vibration is primarily to increase the length of the path, the effect on the rate of oscillation depending on the law of stiffness of the spring, which in the case of a normal spring is such that the rate of oscillation is constant.

If a weight of 1 lb. be held by a spring which requires 1·2 lb. to deflect it 1 ft., it would vibrate in a period of one second, and through a distance depending on the initial disturbance. A weight of 1 lb. controlled by a uniformly revolving crank would vibrate in the period of revolution of the crank and through a distance of twice the length of the crank. If the crank revolve in a period of one second, and the spring be disturbed to move through twice the length of the crank, the two motions become identical, and the energy of motion is the same in both cases.

The next question is, what becomes of this energy of motion? and this will form the subject of the next article.

III.

[*From* "The Engineer," *February* 2, 1883.]

11. *The transmission of energy.*—In the last article the kinetics of vibrations were treated, so far as the oscillating body was concerned. This is only one side of the subject. To maintain oscillation there must be some action on the body from the outside. Without refining too much, we may say that the energy of motion must be imparted to and taken from the oscillating body twice every revolution by the action of other bodies. What becomes of the energy after it leaves the vibrating body, and whence comes the fresh supply, depend on the circumstances which maintain the motion. These may be divided into two principal classes.

12. (1) It may be that the whole or part of the work done by the body in stopping is done against the resistance of friction. As much of the energy as is thus spent will be transformed into heat and lost, and to maintain the motion a fresh supply must be drawn from some external source. (2) It may be that the work done by the body in stopping is done upon some body susceptible of energy, which stores the energy as it receives it, and then, when the motion of the body is reversed, returns the greater part of it to the reciprocating body again, thus diminishing the draught to be made upon the fresh supply. The return can never be complete, as there will always be some frictional resistance to motion. Probably the most complete return

is made in the case of the balance-wheel of the watch, which does its work in stopping against the hair-spring and receives this energy again so nearly in full that the fresh supply added by the escapement bears a very small proportion to the whole. When the oscillation is controlled by a crank, the work of giving the energy of motion is done by the crank, and the crank again receives the work done by the body in stopping. If the crank is connected with a fly-wheel this wheel will absorb and give out energy by a variation of its velocity, and thus the energy of motion is transferred backward and forward between the fly-wheel and the oscillating body, just as in the former case it was transferred between the oscillating body and the spring. In both cases there are certain losses inherent on the transmission, and these losses constitute the disadvantage, as regards friction, of oscillation compared with rotation. They will be different in different cases. Before, however, proceeding to consider these, it will be well to consider shortly the various means of storing and re-storing energy.

13. *Reservoirs of energy.*—The storing and re-storing of energy is generally accomplished by the variation in the motion of some body, as of the fly-wheel, by the elastic deformation of some body, such as a spring, or by the pressure of a gas; but it may be accomplished by the raising of a weight or by magnetic or electric actions. Whichever of these means is used, there is a material reservoir which must have sufficient capacity under the particular circumstance to contain the energy of motion. The capacities of such reservoirs will depend on various circumstances; but one factor will, in all cases, be the amount of material: (1) If the reservoir is the motion of matter, then its capacity will depend on the circumstances of motion; but if u_0 and u_1 are the velocities of the reservoir when charged and discharged, the capacity is

$$\frac{W(u_0^2 - u_1^2)}{2g}.$$

(2) If the reservoir be a spring, then the capacity will depend on the state of stress and elasticity of the material; but if f be the stress in pounds on the square inch, and E is the modulus of elasticity, the capacity is

$$V\frac{\bar{f}^2}{2E} \quad\text{.................................. (17),}$$

V being the volume of the material of the spring in cubic inches, f the stress in pounds per square inch, $\bar{f}^2$ the mean value of f^2 throughout the spring, E the modulus of elasticity. Where the amount of energy to be stored is large, the weight and size of the reservoir are often matters of the first importance. These depend solely on the weight and velocity of the oscillating pieces, and these being known, the weight of the reservoir, if it is a fly-wheel or a spring, can easily be found.

14. The case of springs is the only one that need be considered in this respect. In this it will appear that the storage power of steel, or any material, is so small that the size of the reservoir becomes prohibitory for any but very small mechanisms. In a well formed spring $\bar{f}^2$ will be $\frac{1}{2}$ or $\frac{1}{3}$ of the square of the greatest stress caused in the spring, according as the spring is spiral or beam. Taking the case of the beam and assuming the greatest stress 20,000 lb. and $e = 40,000,000$, then the energy must be less than $\frac{V}{7 \cdot 2}$ where V is the volume of steel in cubic inches. Now, if we have a vibrating body making n oscillations per minute, the energy of motion by the previous article is

$$E_0 = \left(\frac{\pi n}{30}\right)^2 \frac{W}{2g} a^2 = \cdot 00017\, W a^2 n^2 \ldots\ldots\ldots\ldots (18),$$

approximately. If, then, we take such an oscillating body as the piston of a locomotive, let $W = 300$, $a = 1$, and $n = 100$.

The energy is 510 foot-pounds, and the volume of steel required to store this would be 3672 cubic inches, nearly 1000 lb., or about $\frac{1}{2}$ ton of steel would be required for a spring sufficient to store and re-store the energy of motion of each piston and rods of a small locomotive when at full speed. This sufficiently shows why oscillating pieces on a large scale cannot be controlled by springs.

15. If air or steam be used instead of steel, then the weight required is small, and need not be considered, although the size of cylinder for its storage is important. In most steam-engines steam is more or less used for this purpose; but this will be closely considered later on.

There is, however, a physical point with regard to the use of elastic reservoirs, which is important.

16. *Changes of temperature in reservoirs of energy.*—All bodies which expand by heat have their temperature increased by compression and diminished by expansion.

With such rigid bodies as steel, this change of temperature is small for possible distortions, but in the case of gases and steam it is very large. If air be compressed to half its volume instantaneously, its temperature rises to 172° Fah.

This change of temperature plays an important part in the loss of energy in transmission which we now come to consider.

17. *Losses of energy in transmission in the case of a steel spring.*—The loss of energy in transmission to and from the vibrating body will be

very small, for the spring may be united with the vibrating body and the supports, so that there is no motion or friction at the joints, and thus the whole loss is in the spring. Even steel may not be perfectly elastic; but the chief loss, which is also very small, is due to the change of temperature. The spring is heated during compression, and cooled during extension, and then, before the restitution takes place, conduction and radiation bring the temperature to equilibrium again, so that the force of restitution is less than that of distortion; but this loss is small, as is shown by the time a spring will continue to vibrate.

18. *The loss in transmitting energy to steam or gas confined in a cylinder.* —In this case the loss from variation of temperature is considerable, but not easy to estimate, and besides this there is the friction and leakage of the piston. When the compression is carried to several atmospheres, as in the steam-engine, these losses cannot be less than from 15 to 25 per cent. during each transmission. If this were not so a piston in a closed cylinder of air would oscillate when disturbed, but as a fact it does little more than spring back to its initial position. Such a loss as this is fatal to the use of steam or air as a means of maintaining oscillation, except in cases, as in the steam-engine, where the use of steam is rendered desirable for other reasons. Before going into these, however, there remains to be considered the friction in the important case of the crank.

19. *The loss of energy in transmission in the case of the crank-controlled oscillation.*—This is the principal means by which oscillating pieces are controlled in machinery, and it is this kind of reciprocation that competes with revolution. Both motions, reciprocation and oscillation, entail certain loss of energy by friction, and it is important to distinguish between those losses that are common to both and those which are peculiar to reciprocation. Now the losses which are common arise mainly from the action of gravity, and the forces of the operation performed—as, in the case of revolution, the tension of the belt or the pressure of the teeth—to cause friction. The losses peculiar to reciprocation are those which arise from the friction caused by the forces due to the inertia of the reciprocating piece. The simplest case will alone be here considered, and the forces which arise from gravity will be left out of account as common to both reciprocation and rotation. As a simple case we may suppose a crank and fly-wheel on a shaft, the radius of which is r_1, r_2 being the radius of the crank pin, and a the length of the crank, a foot being the unit. The reciprocating piece is supposed to be connected to the crank by a long light connecting rod, so that the whole weight w lies in the reciprocating piece, and the pressure on the guides is so small that it may be neglected.

The forces which arise from the inertia of the reciprocating piece will be transmitted through the crank pin to the bearings. These forces will give rise to friction on the crank pin and the bearings. The forces will be different at different parts of the revolution. Let C be the mean over the whole revolution and f the coefficient of friction at the bearings, then the work (L) spent in overcoming this friction during one revolution is given by the well-known formula

$$L = 2\pi r f C \qquad (19).$$

Or, taking into account that this force acts both on the crank pin and bearings,

$$L = 2\pi f C(r_1 + r_2) \qquad (20).$$

To find C we have in Fig. 2 the value of pPM for each position of the crank, and to find the mean we have only to divide the crank circle into any number of equal parts, and find the corresponding positions of the reciprocating piece as $P_1P_2\ldots$ in Fig. 3; find the corresponding values of $P_1M_1P_2M_2$,

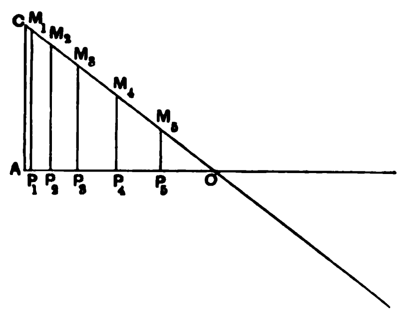


Fig. 3.

and take the mean. This method may be employed whatever may be the shape of the curve ACM_2M_4O. In this case it is well known that

$$C = \frac{2}{\pi} \cdot p \cdot AC$$

$$= \frac{2}{\pi} \cdot \frac{v_0^2}{a} \frac{W}{g}$$

$$= \frac{4}{\pi} \frac{E_0}{a} \qquad (21).$$

Where, as before, E_0 is the energy of motion, C is exactly $\frac{2}{\pi}$ times the centrifugal force of a weight revolving on an arm a with velocity v_0. The loss per revolution then becomes

$$L = 8f(r_1 + r_2)\frac{E_0}{a} \qquad (22).$$

This formula gives the loss in any actual case where r_1 and r_2 are known.

The values of r_1 and r_2 will be determined to meet the forces which fall on the crank pin and crank shaft. If we assume that the forces arising from inertia are paramount, then, since the maximum value of these is

$$\frac{Wv_0^2}{ag} = \frac{2E_0}{a},$$

we shall have

$$\left.\begin{aligned} r_1 &= B_1\sqrt{\frac{2E_0}{a}} \\ r_2 &= B_2\sqrt{\frac{2E_0}{a}} \end{aligned}\right\} \quad \text{.............................(23)},$$

where B_1 and B_2 are constants, which, according to the practice in steam-engines, may be taken to be $\cdot 001$; therefore

$$L = \cdot 008 \,.\, f \,.\, \left(\frac{W}{ga}\right)^{\frac{3}{2}} v_0^3 \quad \text{..........................(24)},$$

which gives the loss on the supposition that the machine is designed to stand the reciprocating forces only.

The importance of this value of L would be in proportion to the work done by the machine per revolution, and it is easy to see that since L increases as the cube of the speed, it may be very small at speeds of, say, 100 revolutions per minute, and yet become so large as to be prohibitory at 300 or 400 revolutions per minute.

In the steam-engines as they exist, these reciprocating forces are not large enough to affect the size of r_1, r_2, which are larger than they would be as in (23), and yet the loss as given by (22) is insignificantly small. In a reciprocating dynamo, in order to obtain anything like the same duty per weight of material as the present revolving dynamo gives, the weights and speeds of their oscillating pieces would have to bear nearly the same relation to the weights and speeds of the engines which drive them as do the armatures of the present dynamos. This means increasing the number of revolutions, as compared with the engines, by a quantity of the order 10, or increasing L in the ratio 1000. The further consideration of these matters will be undertaken in the next article.

IV.

[*From* "The Engineer," *February* 16, 1883.]

20. *Application to the dynamo and steam-engine.*—In order to arrive at a just estimate of the friction caused by the inertia of reciprocation in practical cases, it is necessary to consider the forces which arise from inertia in conjunction with the working force—the force required to accelerate the piston in conjunction with the pressure of steam.

21. *The resultant of inertia and the working forces on a reciprocating piece.*—When, as is generally the case, the reciprocating piece is subject to forces besides those which act between it and the crank, the pressure on the crank will be the resultant of this force and the force necessary to balance the inertia.

The working force may be represented, as in the case of the steam-engine, by a diagram. Let $p \cdot PM'$ be the working force at the point P. Consider the motion from A to B, and let M' be on the upper side of AB when the force is in the direction of AB, and on the lower side when the force is in

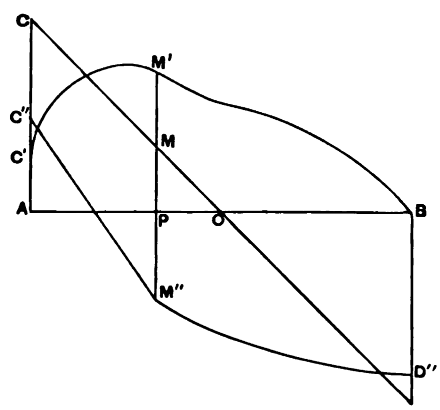


Fig. 4.

the direction BA. That is, PM' is drawn on the same side as PM representing the forces to overcome the inertia. The pressure on the crank will therefore be

$$p(PM' - PM) = p \cdot M'M \text{........................}(25);$$

make $PM'' = MM'$, noticing that M'' will be above AB when M is above M', and *vice versâ*. In this way a line $AC''M''D''B$ may be drawn, the distance of which from AB shows the pressure on the crank; and then the mean pressure may be found as before, substituting PM'' for PM. As far as the friction is concerned this will be independent of the direction of the pressure on the crank, so that in finding the mean PM'' must be taken always of the same sign.

There are two cases of special interest. First let AC' be greater than AC, then the forces acting from A to B will be altogether in the direction AB. Take two points P and Q (Fig. 5) on the opposite sides of O, and at

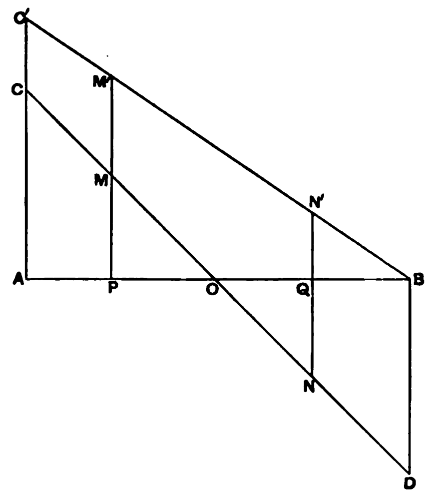


Fig. 5.

equal distances from it. Then if the two triangles OAC and BAC' are similar, and therefore $C'M'B$ parallel to CD,

$$PM' + QN' = MM' + NN \quad \text{......................(26).}$$

Therefore the mean of the pressure at these two points on the crank will be unaltered by the inertia, and as at these points the crank is making equal angles with AB, in opposite directions, the mean pressure on the crank will be identically the same as would arise from the working forces, or, in other words, the inertia of the reciprocating piece will cause no extra friction; and this, as will be shown, is practically the case in the steam-engine. Second, let the inertia be paramount, *i.e.*, AC greater than AC', and let the acting forces be symmetrical about O, as shown by the curve $AJM'N'B$ in Fig. 6.

Let the curve CO cut the curve $AM'B$ in J; draw JI perpendicular to AB and take $OK = OI$. Then as before if P lies between I and K

$$PM' + QN' = MM' + NN' \quad \text{......................(27).}$$

So that between I and K the mean pressures on the crank will be the same as if caused by the working forces PM' only.

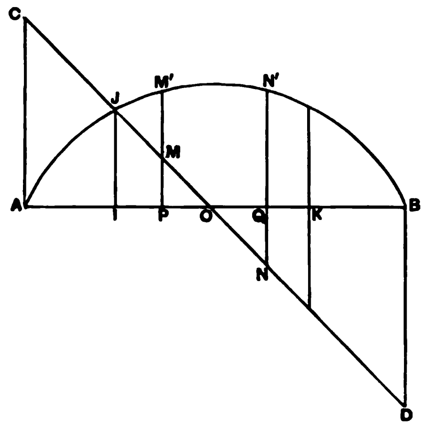


Fig. 6.

When P lies between A and I, then

$$PM + QN = MM' + NN' \quad \text{........................(28)},$$

or the mean pressure will be the same as if only the forces of inertia acted; and this, as will be shown, is the case of the dynamo machine.

22. *Application to the dynamo machine.*—Since there has been no experience with oscillating dynamo machines, the formula obtained in the last article can only be applied to an assumed case. The revolving dynamo machine may be made to furnish the data for an oscillating dynamo machine; a, the length of the crank, may be taken equal to the mean radius of the armature, W equal the weight of the armature, and the time of an oscillation the same as the time of a revolution.

Taking a particular dynamo driven by 6-horse power, it appears that

$$\left.\begin{aligned} W &= 200 \text{ lb.} \\ a &= \cdot 3 \\ n &= 1000 \\ f &= \cdot 05 \end{aligned}\right\} \quad \text{..............................(29)}.$$

So that $v_0 = 30$ approximately;

$$L = \cdot 008 f \left(\frac{W}{ga}\right)^{\frac{3}{2}} v_0^3 \quad \text{...........................(30)}.$$

This gives for L 1000 foot-pounds, in round numbers. This is the loss per revolution. Per minute the loss would be about 1,000,000 foot-pounds, or 30-horse power. So that the loss due to the friction arising from the inertia of the reciprocating armature would be five times greater than the work done in creating a current. Put this way, even supposing the assumed data admit of considerable modification, it is clear that the friction arising from reciprocation is prohibitory in the case of a dynamo machine. But before adopting this view, it is well to see how far this loss might be modified by the work which the dynamo machine was doing. 6-horse power, with a stroke of ·6 and 1000 revolutions per minute, would be equivalent to a uniform resistance on the vibrating body of 165 lb. The force $\dfrac{Wv^2}{ag} = 20{,}000$.

So that if in the diagram $AC = 1''$, and $p.AC$ represent 20,000 lb., $p = 20{,}000$; and if $pAC' = 160$ lb., $AC' = {\cdot}008''$. This is too small to be drawn to scale; but if drawn, the line JI in Fig. 6 would be ·008 AC and OI would be ·008 AO. Therefore the amount of work represented by the diagram $ILLK$ would be $\cdot 008 \times 160$, or 1·3 foot-pounds, and this may be neglected. And for the rest of the diagram, as shown in Section 21, the mean pressure on the crank pin will be the same as if the forces of inertia were alone to be considered. In this case, therefore, where the forces of inertia are paramount, the friction is determined almost entirely by the forces of inertia, the working forces neither adding to nor subtracting from the friction.

It thus appears that there is no chance for the reciprocating dynamo machine, driven by a crank, and it will appear equally clear that there is no chance for a reciprocating dynamo machine driven direct from the piston of a steam-engine, for in this case the energy of motion, which, as in the last example, is 3000 foot-pounds, would have to be stored by cushioning steam, that is to say, 3000 foot-pounds would have to be transmitted to and from the steam twice each revolution; the entire transmission therefore would be 12,000 foot-pounds. Now, taking the smallest estimate of loss in this transmission, namely, 15 per cent., we have a loss of 1800 foot-pounds per revolution, nearly double as great as with the crank.

If we substitute a steel spring for the cushioning, then the weight of steel, which, estimated as before, would be 6 tons, is prohibitory.

Thus in every case we have amply sufficient reasons for the non-applicability of reciprocation to the dynamo machine.

These results are sufficiently striking in themselves, but they become still more so when compared with the corresponding results for the steam-engine.

23. *Application to the steam-engine.*—For the sake of comparison the circumstances of the engine may be taken similar to those of the dynamo machine just considered. Thus, the weight of piston and reciprocating parts

is taken at 200 lb., and the length of crank, ·3. This would only give a 7 in. stroke, which is somewhat out of proportion, considering that 200 lb. would correspond with a piston some 15 in. in diameter, that is, considering the shortness of the stroke. This will be a convenient size to assume for the piston, taking the initial pressure of steam 120 lb. on the square inch; since this over a 15 in. piston is 21,120 lb., which is just about the same as the

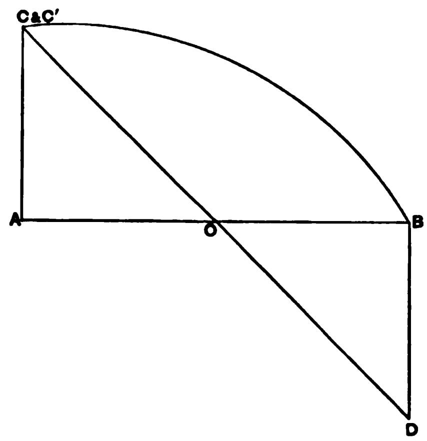


Fig. 7.

force of inertia, and in the diagram $AC = AC'$, or C and C' coincide, *i.e.*—if, for the sake of comparison, the number of revolutions is taken the same as before $n = 1000$.

Since W, n, and a are the same as before, the crank pin will be subject to the same pressures, on account of inertia; and since we may assume, from expansion, the pressure of steam to fall towards B, the greatest pressures on the crank pin will not exceed the greatest forces of inertia; therefore $r_1 r_2$ may be taken to have the same value as before. And considering only the force of inertia, we should find as before—

$$L = 1000 \text{ foot-pounds},$$

$$n\,L = 1{,}000{,}000,$$

or the loss would be at the rate of 30-horse power. But even supposing this loss to take place, it bears a very different comparison to the work done by the steam-engine from what it did to the dynamo machine. With an initial pressure of 120, the steam being used as in the locomotive, the mean pressure would be, say, 70 lb.; this would give the work per stroke 15,000 foot-pounds, so that the loss would only be one-fifteenth, or between 6 and 7 per cent., instead of 500 per cent. in the case of the dynamo machine.

As a matter of fact, however, there would be no such loss in the engine when doing its full work. This appears on compounding the diagrams of inertia and working pressure as in Fig. 5, Art. 21, for since AC' is not greater than AC,

$$PM' + QN' = MM' + M'N \quad\text{.......................(31)},$$

throughout the diagram, or the mean pressure on the crank taken all round is not affected by the inertia of the piston; and hence whatever loss the friction arising from the pressure may cause, it will be due entirely to the acting pressure of steam, and so long as this remains unaltered, the loss per revolution will be the same at all speeds up to 1000 revolutions. Considering that the speed of piston here taken 1800 ft. per minute, and the number of revolutions, 1000, are well outside all practical values, this example shows that in whatever other ways the forces arising from the inertia of reciprocation act to limit the speed of the steam-engine, they need not affect the friction of the engine, either directly or indirectly by requiring larger bearings, even should the speed of the steam-engines reach values five or six times greater than the present values. Thus, although, as we have seen in the case of the dynamo machine, there are circumstances in which the friction arising from the inertia of the reciprocating force is so large compared with the acting forces as to be prohibitory to oscillating motion, yet in the case of the steam-engine these forces give rise to no loss whatever, and do not place the reciprocating engine at a disadvantage as compared with the rotary engine.

It seems, then, that we have a good reason for the general impression in favour of rotary motion as compared with reciprocating motion, and also a good reason why the impression is erroneous as applied to the steam-engine.

Before closing these articles, it may be well to refer shortly to cushioning, or compression, as used in reference to the steam-engine.

24. *Cushioning.*—The useful purposes attributed to this are these:—

(1) Cushioning is supposed to save steam by filling the passages to the ports and other necessary clearance, so that this has not to be filled with fresh steam which does no work in filling them.

(2) Cushioning is often supposed by relieving the crank from the duty of stopping the piston, and so by diminishing the pressure on the crank pin and bearings, to diminish the friction.

(3) Cushioning is found by experience to be necessary in the case of all high-speed engines, to prevent a sudden shock attending the admission of steam.

Now, the last of these advantages is a matter of experience, and is alone sufficient to warrant a certain amount of cushioning. If, when running at its greatest speed, an engine knocks or bumps in its bearings, it is a sign that it is insufficiently cushioned. This admits of theoretical explanation. If cushioned, as the piston approaches the end of its stroke A it will be stopping itself driving the crank, the force arising from inertia being at its greatest. Thus the force will have a tendency to close all the joints between the piston and the bearings in the direction BA, opening them in the direction AB. On the admission of the steam, owing to the small clearance to be filled, the pressure suddenly rises to a greater value than the force of inertia, and the piston is, as it were, shot back by the pressure of the steam and the elasticity of the engine against the force of its inertia. The joints thus close towards B with a bump. This bump could not have occurred had not the reversal of the direction of the combined force and inertia been sudden when the joints were open towards A. By cushioning, the pressure of the steam which balances the inertia rises gradually, so that the joints which are at first open towards A close gradually.

As regards the first two advantages, the first of these must be regarded as hypothetical, or rather, as theoretical, and the second as imaginary.

The steam with which the clearance is filled is not all gain. This is well known. The work done in compression has to be deducted from the work done by the forward pressure of the steam, or the power of the engine will be diminished by the power spent in compression, while the entire friction and the losses by condensation remain the same. As these losses appear to be something like 40 per cent. of the theoretical power of the steam as used in the engine, there cannot be much margin for gain of steam. The advantage may be a little one way or the other, but it is not worth mentioning.

The second assumed advantage of cushioning, namely, the diminution of the mean pressure on the engine, vanishes when it is perceived that it is the working pressure of the inertia that is diminished. This assumption amounts to nothing more or less than assuming that the moving energy of the piston might be more efficiently stored and restored by compressing steam than it is by the crank. It has, however, been shown that the crank performs this work in the steam-engine with no loss, whereas in compressing steam there will probably be a loss of from 15 to 25 per cent. of the energy stored. This is the loss which has been shown to balance the gain in steam in (1). In respect of (2), therefore, the cushioning is a disadvantage. That this has not been practically perceived is because, as long as cushioning is only carried to the extent of filling the necessary clearance, then the loss and the gain, as in (1), are nearly balanced, as has already been shown.

The conclusion is, therefore, that cushioning should not be carried further than is sufficient to prevent bumping.

44.

AN EXPERIMENTAL INVESTIGATION OF THE CIRCUMSTANCES WHICH DETERMINE WHETHER THE MOTION OF WATER SHALL BE DIRECT OR SINUOUS, AND OF THE LAW OF RESISTANCE IN PARALLEL CHANNELS.

[*From* "The Philosophical Transactions of the Royal Society," 1883.]

(*Received and Read* March 15, 1883.)

SECTION I.

Introductory.

1. *Objects and results of the investigation.*—The results of this investigation have both a practical and philosophical aspect.

In their practical aspect they relate to the law of resistance to the motion of water in pipes, which appears in a new form, the law for all velocities and all diameters being represented by an equation of two terms.

In their philosophical aspects these results relate to the fundamental principles of fluid motion; inasmuch as they afford for the case of pipes a definite verification of two principles, which are—that the general character of the motion of fluids in contact with solid surfaces depends on the relation between a physical constant of the fluid, and the product of the linear dimensions of the space occupied by the fluid, and the velocity.

The results as viewed in their philosophical aspect were the primary object of the investigation.

As regards the practical aspects of the results it is not necessary to say anything by way of introduction; but in order to render the philosophical scope and purpose of the investigation intelligible it is necessary to describe shortly the line of reasoning which determined the order of investigation.

2. *The leading features of the motion of actual fluids.* Although in most ways the exact manner in which water moves is difficult to perceive and still more difficult to define, as are also the forces attending such motion, certain general features both of the forces and motions stand prominently forth, as if to invite or to defy theoretical treatment.

The relations between the resistance encountered by, and the velocity of, a solid body moving steadily through a fluid in which it is completely immersed, or of water moving through a tube, present themselves mostly in one or other of two simple forms. The resistance is generally proportional to the square of the velocity, and when this is not the case it takes a simpler form and is proportional to the velocity.

Again, the internal motion of water assumes one or other of two broadly distinguishable forms—either the elements of the fluid follow one another along lines of motion which lead in the most direct manner to their destination, or they eddy about in sinuous paths the most indirect possible.

The transparency or the uniform opacity of most fluids renders it impossible to see the internal motion, so that, broadly distinct as are the two classes (direct and sinuous) of motion, their existence would not have been perceived were it not that the surface of water, where otherwise undisturbed, indicates the nature of the motion beneath. A clear surface of moving water has two appearances, the one like that of plate glass, in which objects are reflected without distortion, the other like that of sheet glass, in which the reflected objects appear crumpled up and grimacing. These two characters of surface correspond to the two characters of motion. This may be shown by adding a few streaks of highly coloured water to the clear moving water. Then although the coloured streaks may at first be irregular, they will, if there are no eddies, soon be drawn out into even colour bands; whereas if there are eddies they will be curled and whirled about in the manner so familiar with smoke.

3. *Connexion between the leading features of fluid motion.* These leading features of fluid motion are well known and are supposed to be more or less connected, but it does not appear that hitherto any very determined efforts have been made to trace a definite connexion between

them, or to trace the characteristics of the circumstances under which they are generally presented. Certain circumstances have been definitely associated with the particular laws of force. Resistance, as the square of the velocity, is associated with motion in tubes of more than capillary dimensions, and with the motion of bodies through the water at more than insensibly small velocities, while resistance as the velocity is associated with capillary tubes and small velocities.

The equations of hydrodynamics, although they are applicable to direct motion, *i.e.*, without eddies, and show that then the resistance is as the velocity, have hitherto thrown no light on the circumstances on which such motion depends. And although of late years these equations have been applied to the theory of the eddy, they have not been in the least applied to the motion of water which is a mass of eddies, *i.e.*, in sinuous motion, nor have they yielded a clue to the cause of resistance varying as the square of the velocity. Thus, while as applied to waves and the motion of water in capillary tubes the theoretical results agree with the experimental, the theory of hydrodynamics has so far failed to afford the slightest hint why it should explain these phenomena, and signally fail to explain the law of resistance encountered by large bodies moving at sensibly high velocities through water, or that of water in sensibly large pipes.

This accidental fitness of the theory to explain certain phenomena while entirely failing to explain others, affords strong presumption that there are some fundamental principles of fluid motion of which due account has not been taken in the theory. And several years ago it seemed to me that a careful examination as to the connexion between these four leading features, together with the circumstances on which they severally depend, was the most likely means of finding the clue to the principles overlooked.

4. *Space and velocity.* The definite association of resistance as the square of the velocity with sensibly large tubes and high velocities, and of resistance as the velocity with capillary tubes and slow velocities, seemed to be evidence of the very general and important influence of some properties of fluids not recognised in the theory of hydrodynamics.

As there is no such thing as absolute space or absolute time recognised in mechanical philosophy, to suppose that the character of motion of fluids in any way depended on absolute size or absolute velocity, would be to suppose such motion without the pale of the laws of motion. If then fluids in their motions are subject to these laws, what appears to be the dependence of the character of the motion on the absolute size of the tube, and on the absolute velocity of the immersed body, must in reality be a dependence on the size of the tube as compared with the size of some other object, and on the velocity of the body as compared with some other velocity. What is the standard object, and what the standard velocity which come into comparison with the size of the tube and the velocity of an immersed body, are questions to which the answers were not obvious. Answers, however, were found in the discovery of a circumstance on which sinuous motion depends.

5. *The effect of viscosity on the character of fluid motion.* The small evidence which clear water shows as to the existences of internal eddies, not less than the difficulty of estimating the viscous nature of the fluid, appears to have hitherto obscured the very important circumstance that the more viscous a fluid is, the less prone is it to eddying or sinuous motion. To express this definitely—if μ is the viscosity and ρ the density of the fluid—for water μ/ρ diminishes rapidly as the temperature rises, thus at 5° C. μ/ρ is double what it is at 45° C. What I observed was that the tendency of water to eddy becomes much greater as the temperature rises.

Hence connecting the change in the law of resistance with the birth and development of eddies, this discovery limited further search for the standard distance and standard velocity to the physical properties of the fluid. To follow the line of this search would be to enter upon a molecular theory of liquids, and this is beyond my present purpose. It is sufficient here to notice the well-known fact that

$$\frac{\mu}{\rho} \text{ or } \mu'$$

is a quantity of the nature of the product of a distance and a velocity.

It is always difficult to trace the dependence of one idea on another. But it may be noticed that no idea of dimensional properties, as indicated by the dependence of the character of motion on the size of the tube and the velocity of the fluid, occurred to me until after the completion of my investigation on the transpiration of gases, in which was established the dependence of the law of transpiration on the relation between the size of the channel and the mean range of the gaseous molecules.

6. *Evidence of dimensional properties in the equations of motion.* The equations of motion had been subjected to such close scrutiny, particularly by Professor Stokes, that there was small chance of discovering anything new or faulty in them. It seemed to me possible, however,

that they might contain evidence which had been overlooked, of the dependence of the character of motion on a relation between the dimensional properties and the external circumstances of motion. Such evidence, not only of a connexion but of a definite connexion, was found, and this without integration.

If the motion be supposed to depend on a single velocity parameter U, say the mean velocity along a tube, and on a single linear parameter c, say the radius of the tube; then having in the usual manner eliminated the pressure from the equations, the accelerations are expressed in terms of two distinct types. In one of which

$$\frac{U^2}{c^3}$$

is a factor, and in the other

$$\frac{\mu U}{\rho c^4}$$

is a factor. So that the relative values of these terms vary respectively as U and

$$\frac{\mu}{c\rho}.$$

This is a definite relation of the exact kind for which I was in search. Of course without integration the equations only gave the relation without showing at all in what way the motion might depend upon it.

It seemed, however, to be certain, if the eddies were due to one particular cause, that integration would show the birth of eddies to depend on some definite value of

$$\frac{c\rho U}{\mu}.$$

7. *The cause of eddies.* There appeared to be two possible causes for the change of direct motion into sinuous. These are best discussed in the language of hydrodynamics, but as the results of this investigation relate to both these causes, which, although the distinction is subtle, are fundamentally distinct and lead to distinct results, it is necessary that they should be indicated.

The general cause of the change from steady to eddying motion was in 1843 pointed out by Professor Stokes, as being that under certain circumstances the steady motion becomes unstable, so that an indefinitely small disturbance may lead to a change to sinuous motion. But the causes above referred to are of this kind, and yet they are distinct, the distinction lying in the part taken in the instability by viscosity.

If we imagine a fluid free from viscosity and absolutely free to glide over solid surfaces, then comparing such a fluid with a viscous fluid in exactly the same motion—

(1) The frictionless fluid might be instable and the viscous fluid stable. Under these circumstances the cause of eddies is the instability as a perfect fluid, the effect of viscosity being in the direction of stability.

(2) The frictionless fluid might be stable and the viscous fluid unstable, under which circumstances the cause of instability would be the viscosity.

It was clear to me that the conclusions I had drawn from the equations of motion immediately related only to the first cause; nor could I then perceive any possible way in which instability could result from viscosity. All the same I felt a certain amount of uncertainty in assuming the first cause of instability to be general. This uncertainty was the result of various considerations, but particularly from my having observed that eddies apparently come on in very different ways, according to a very definite circumstance of motion, which may be illustrated.

When in a channel the water is all moving in the same direction, the velocity being greatest in the middle and diminishing to zero at the sides, as indicated by the curve in Fig. 1, eddies showed themselves reluctantly and

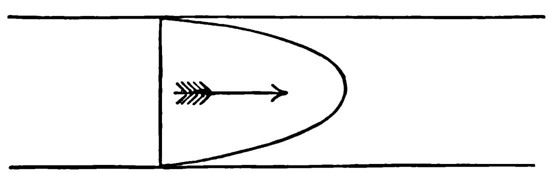


Fig. 1.

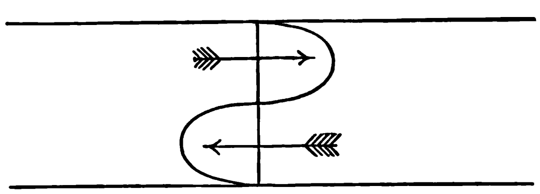


Fig. 2.

irregularly; whereas when the water on one side of the channel was moving in the opposite direction to that on the other, as shown by the curve in Fig. 2, eddies appeared in the middle regularly and readily.

8. *Methods of investigation.*—There appeared to be two ways of proceeding—the one theoretical, the other practical.

The theoretical method involved the integration of the equations for unsteady motion in a way that had not been accomplished and which, considering the general intractability of the equations, was not promising.

The practical method was to test the relation between U, μ/ρ, and c; this, owing to the simple and definite form of the law, seemed to offer, at all events in the first place, a far more promising field of research.

The law of motion in a straight smooth tube offered the simplest possible circumstances and the most crucial test.

The existing experimental knowledge of the resistance of water in tubes, although very extensive, was in one important respect incomplete. The previous experiments might be divided into two classes: (1) those made under circumstances in which the law of resistance was as the square of the velocity, and (2) those made under circumstances in which the resistance varied as the velocity. There had not apparently been any attempt made to determine the exact circumstances under which the change of law took place.

Again, although it had been definitely pointed out that eddies would explain resistance as the square of the velocity, it did not appear that any definite experimental evidence of the existence of eddies in parallel tubes had been obtained, and much less was there any evidence as to whether the birth of eddies was simultaneous with the change in the law of resistance.

These open points may be best expressed in the form of queries to which the answers anticipated were in the affirmative.

(1) What was the exact relation between the diameters of the pipes and the velocities of the water at which the law of resistance changed?

Was it at a certain value of

$$cU?$$

(2) Did this change depend on the temperature, *i.e.*, the viscosity of water? Was it at a certain value of

$$\rho \frac{U}{\mu}?$$

(3) Were there eddies in parallel tubes?

(4) Did steady motion hold up to a critical value and then eddies come in?

(5) Did the eddies come in at a certain value of

$$\frac{\rho c U}{\mu}?$$

(6) Did the eddies first make their appearance as small and then increase gradually with the velocity, or did they come in suddenly?

The bearing of the last query may not be obvious; but, as will appear in the sequel, its importance was such that, in spite of satisfactory answers to all the other queries, a negative answer to this, in respect of one particular class of motions, led me to the reconsideration of the supposed cause of instability.

The queries, as they are put, suggest two methods of experimenting:—

(1) Measuring the resistances and velocities of different diameters, and with different temperatures of water.

(2) Visual observation as to the appearance of eddies during the flow of water along tubes or open channels.

Both these methods have been adopted, but, as the questions relating to eddies had been the least studied, the second method was the first adopted.

9. *Experiments by visual observation.*—The most important of these experiments related to water moving in one direction along glass tubes. Besides this, however, experiments on fluids flowing in opposite directions in the same tube were made, also a third class of experiments, which related to motion in a flat channel of indefinite breadth.

These last-mentioned experiments resulted from an incidental observation during some experiments made in 1876 as to the effect of oil to prevent wind waves. As the result of this observation had no small influence in directing the course of this investigation, it may be well to describe it first.

10. *Eddies caused by the wind beneath the oiled surface of water.*—A few drops of oil on the windward side of a pond during a stiff breeze, having spread over the pond and completely calmed the surface as regards waves, the sheet of oil, if it may be so called, was observed to drift before the wind, and it was then particularly noticed that while close to, and for a considerable distance from the windward edge, the surface presented the appearance of plate glass; further from the edge the surface presented that irregular wavering appearance which has already been likened to that of sheet glass, which appearance was at the time noted as showing the existence of eddies beneath the surface.

Subsequent observation confirmed this first view. At a sufficient distance from the windward edge of an oil-calmed surface there are always eddies beneath the surface even when the wind is light. But the distance from the edge increases rapidly as the force of the wind diminishes, so that at a limited distance (10 or 20 feet) the eddies will come and go with the wind.

Without oil I was unable to perceive any indication of eddies. At first I thought that the waves might prevent their appearance even if they were there, but by careful observation I convinced myself that they were not there. It is not necessary to discuss these results here, although, as will appear, they have a very important bearing on the cause of instability.

11. *Experiments by means of colour bands in glass tubes.*—These were undertaken early in 1880; the final experiments were made on three tubes, Nos. 1, 2, and 3. The diameters of these were nearly 1 inch, $\frac{1}{2}$ inch, and $\frac{1}{4}$ inch. They were all about 4 feet 6 inches long, and fitted with trumpet mouthpieces, so that the water might enter without disturbance.

The water was drawn through the tubes out of a large glass tank, in which the tubes were immersed, arrangements being made so that a streak or streaks of highly coloured water entered the tubes with the clear water.

The general results were as follows:—

(1) When the velocities were sufficiently low, the streak of colour extended in a beautiful straight line through the tube, Fig. 3.

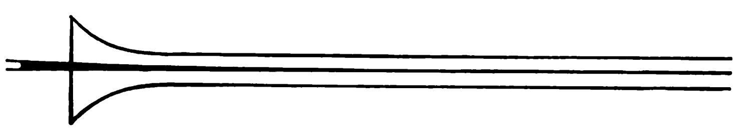


Fig. 3.

(2) If the water in the tank had not quite settled to rest, at sufficiently low velocities, the streak would shift about the tube, but there was no appearance of sinuosity.

(3) As the velocity was increased by small stages, at some point in the tube, always at a considerable distance from the trumpet or intake, the

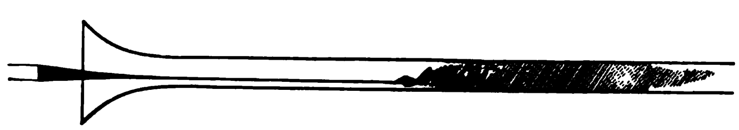


Fig. 4.

colour band would all at once mix up with the surrounding water, and fill the rest of the tube with a mass of coloured water, as in Fig. 4.

Any increase in the velocity caused the point of break down to approach the trumpet, but with no velocities that were tried did it reach this.

On viewing the tube by the light of an electric spark, the mass of colour resolved itself into a mass of more or less distinct curls, showing eddies, as in Fig. 5.

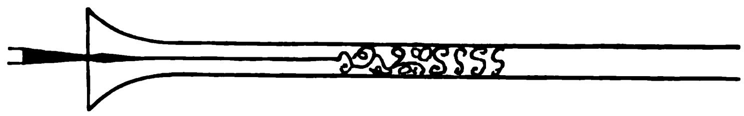


Fig. 5.

The experiments thus seemed to settle questions 3 and 4 in the affirmative, the existence of eddies and a critical velocity.

They also settled in the negative question 6, as to the eddies coming in gradually after the critical velocity was reached.

In order to obtain an answer to question 5, as to the law of the critical velocity, the diameters of the tubes were carefully measured, also the temperature of the water, and the rate of discharge.

(4) It was then found that, with water at a constant temperature, and the tank as still as could by any means be brought about, the critical velocities at which the eddies showed themselves were almost exactly in the inverse ratio of the diameters of the tubes.

(5) That in all the tubes the critical velocity diminished as the temperature increased, the range being from 5° C. to 22° C.; and the law of this diminution, so far as could be determined, was in accordance with Poiseuille's experiments. Taking T to express degrees centigrade, then by Poiseuille's experiments,

$$\frac{\mu}{\rho} \propto P = (1 + 0{\cdot}0336T + 0{\cdot}00221T^2)^{-1}.$$

Taking a metre as the unit, U_s the critical velocity, and D the diameter of the tube, the law of the critical point is completely expressed by the formula

$$U_s = \frac{1}{B_s}\frac{P}{D}$$

where

$$B_s = 43{\cdot}79$$

$$\log B_s = 1{\cdot}64139.$$

This is a complete answer to question 5.

During the experiments many things were noticed which cannot be mentioned here, but two circumstances should be mentioned as emphasising the negative answer to question 6. In the first place, the critical velocity was much higher than had been expected in pipes of such magnitude, resistance varying as the square of the velocity had been found at very much smaller velocities than those at which the eddies appeared when the water in the tank was steady; and in the second place, it was observed that the critical velocity was very sensitive to disturbance in the water before entering the tubes; and it was only by the greatest care as to the uniformity of the temperature of the tank and the stillness of the water that consistent results were obtained. This showed that the steady motion was unstable for large disturbances long before the critical velocity was reached, a fact which agreed with the full-blown manner in which the eddies appeared.

12. *Experiments with two streams in opposite directions in the same tube.*—A glass tube, 5 feet long and 1·2 inch in diameter, having its ends slightly bent up, as shown in Fig. 6, was half filled with bisulphide of carbon,

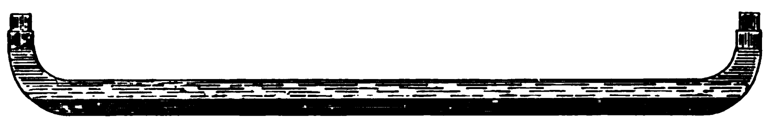


Fig. 6.

and then filled up with water and both ends corked. The bisulphide was chosen as being a limpid liquid but little heavier than water and completely insoluble, the surface between the two liquids being clearly distinguishable. When the tube was placed in a horizontal direction, the weight of the bisulphide caused it to spread along the lower half of the tube, and the surface of separation of the two liquids extended along the axis of the tube. On one end of the tube being slightly raised the water would flow to the upper end and the bisulphide fall to the lower, causing opposite currents along the upper and lower halves of the tube, while in the middle of the tube the level of the surface of separation remained unaltered.

The particular purpose of this investigation was to ascertain whether there was a critical velocity at which waves or sinuosities would show themselves in the surface of separation.

It proved a very pretty experiment and completely answered its purpose.

When one end was raised quickly by a definite amount, the opposite velocities of the two liquids, which were greatest in the middle of the tube, attained a certain maximum value, depending on the inclination of the tube. When this was small no signs of eddies or sinuosities showed themselves; but, at a certain definite inclination, waves (nearly stationary) showed themselves, presenting all the appearance of wind waves. These waves first made their appearance as very small waves of equal lengths, the length being comparable to the diameter of the tube.

Fig. 7.

When by increasing the rise the velocities of flow were increased, the waves kept the same length but became higher, and when the rise was sufficient the waves would curl and break, the one fluid winding itself into the other in regular eddies.

Whatever might be the cause, a skin formed slowly between the bisulphide and the water, and this skin produced similar effects to that of oil and water; the results mentioned are those which were obtained before the skin showed itself. When the skin first came on regular waves ceased to form, and in their place the surface was disturbed, as if by irregular eddies, above and below, just as in the case of the oiled surface of water.

The experiment was not adapted to afford a definite measure of the velocities at which the various phenomena occurred; but it was obvious that the critical velocity at which the waves first appeared was many times smaller than the critical velocity in a tube of the same size when the motion was in one direction only. It was also clear that the critical velocity was nearly, if not quite, independent of any existing disturbance in the liquids; so that this experiment shows—

(1) That there is a critical velocity in the case of opposite flow at which direct motion becomes unstable.

(2) That the instability came on gradually and did not depend on the magnitude of the disturbances, or in other words, that for this class of motion question 6 must be answered in the affirmative.

It thus appeared that there was some difference in the cause of instability in the two motions.

13. *Further study of the equations of motion.*—Having now definite data to guide me, I was anxious to obtain a fuller explanation of these results from the equations of motion. I still saw only one way open to account for the instability, namely, by assuming the instability of a frictionless fluid to be general.

Having found a method of integrating the equations for frictionless fluid as far as to show whether any particular form of steady motion is stable for a small disturbance, I applied this method to the case of parallel flow in a frictionless fluid. The result, which I obtained at once, was that flow in one direction was stable, flow in opposite directions unstable. This was not what I was looking for, and I spent much time in trying to find a way out of it, but whatever objections my method of integration may be open to, I could make nothing less of it.

It was not until the end of 1882 that I abandoned further attempts with a frictionless fluid, and attempted by the same method the integration of a viscous fluid. The change was in consequence of a discovery that in previously considering the effect of viscosity I had omitted to take fully into account the boundary conditions which resulted from the friction between the fluid and the solid boundary.

On taking these boundary conditions into account, it appeared that although the tendency of internal viscosity of the fluid is to render direct or steady motion stable, yet owing to the boundary condition resulting from the friction at the solid surface, the motion of the fluid, irrespective of viscosity, would be unstable. Of course this cannot be rendered intelligible without going into the mathematics. But what I want to point out is that this instability, as shown by the integration of the equations of motion, depends on exactly the same relation,

$$U \propto \frac{\mu}{c\rho},$$

as that previously found.

This explained all the practical anomalies and particularly the absence of eddies below a pure surface of water exposed to the wind. For in this case the surface being free, the boundary condition was absent, whereas the film of oil, by its tangential stiffness, introduced this condition; this circumstance alone seemed a sufficient verification of the theoretical conclusion.

But there was also the sudden way in which eddies came into existence in the experiments with the colour band, and the effect of disturbances to lower the critical velocity. These were also explained, for as long as the motion was steady, the instability depended upon the boundary action alone, but once eddies were introduced, the stability would be broken down.

It thus appeared that the meaning of the experimental results had been ascertained, and the relation between the four leading features and the circumstances on which they depend traced for the case of water in parallel flow.

But as it appeared that the critical velocity in the case of motion in one direction, did not depend on the cause of instability, with a view to which it was investigated, it followed that there must be another critical velocity, which would be the velocity at which previously existing eddies would die out, and the motion become steady as the water proceeded along the tube. This conclusion has been verified.

14. *Results of experiments in the law of resistance in tubes.*—The existence of the critical velocity described in the previous article, could only be tested by allowing water in a high state of disturbance to enter a tube, and after flowing a sufficient distance for the eddies to die out, if they were going to die out, to test the motion.

As it seemed impossible to apply the method of colour bands, the test applied was that of the law of resistance as indicated in questions (1) and (2) in § 8. The result was very happy.

Two straight lead pipes No. 4 and No. 5, each 16 feet long and having diameters of a quarter and a half inch respectively, were used. The water was allowed to flow through rather more than 10 feet before coming to the first gauge hole, the second gauge hole being 5 feet further along the pipe.

The results were very definite, and are partly shown in Fig. 8, and more fully in diagram 1, page 90.

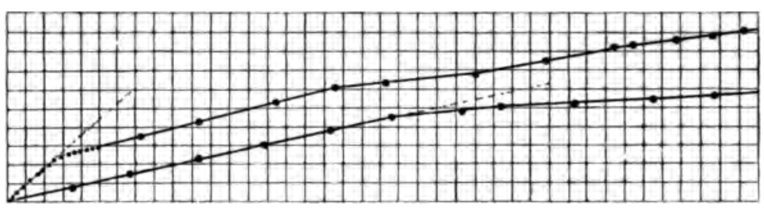


Fig. 8.

(1) At the lower velocities the pressure was proportional to the velocity, and the velocities at which a deviation from the law first occurred were in exact inverse ratio of the diameters of the pipes.

(2) Up to these critical velocities the discharge from the pipes agreed exactly with those given by Poiseuille's formula for capillary tubes.

(3) For some little distance after passing the critical velocity, no very simple relations appeared to hold between the pressures and velocities. But by the time the velocity reached 1·2 (critical velocity) the relation became again simple. The pressure did not vary as the square of the velocity, but as 1·722 power of the velocity; this law held in both tubes and through velocities ranging from 1 to 20, where it showed no signs of breaking down.

(4) The most striking result was that not only at the critical velocity,

but throughout the entire motion, the laws of resistance exactly corresponded for velocities in the ratio of

$$\frac{\mu}{\rho c}.$$

This last result was brought out in the most striking manner on reducing the results by the graphic method of logarithmic homologues as described in my paper on Thermal Transpiration.* Calling the resistance per unit of length as measured in the weight of cubic units of water i, and the velocity v, log i is taken for abscissa, and log v for ordinate, and the curve plotted.

In this way the experimental results for each tube are represented as a curve; these curves, which are shown as far as the small scale will admit in Fig. 9, present exactly the same shape, and only differ in position.

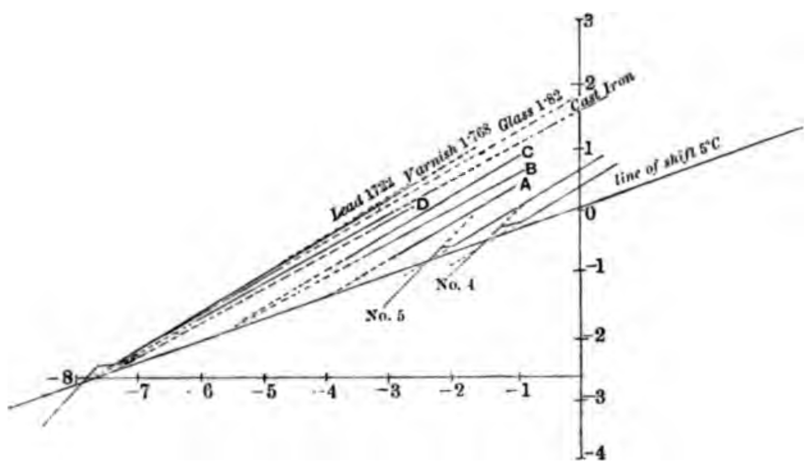


Fig. 9.

Pipe.	Diameter. m.
No. 4, Lead	0·00615
„ 5, „	0·0127
A, Glass	0·0496
B, Cast-iron	0·188
D, „	0·5
C, Varnish	0·196.

Either of the curves may be brought into exact coincidence with the other by a rectangular shift, and the horizontal shifts are given by the difference of the logarithms of

$$\frac{D^3}{\mu^2}$$

* *Phil. Trans.* 1879, Part II. p. 40.

for the two tubes, the vertical shifts being the difference of the logarithms of

$$\frac{D}{\mu}.$$

The temperatures at which the experiment had been made were nearly the same, but not quite, so that the effect of the variations of μ showed themselves.

15. *Comparison with Darcy's experiments.*—The definiteness of these results, their agreement with Poiseuille's law, and the new form which they more than indicated for the law of resistance above the critical velocities, led me to compare them with the well-known experiments of Darcy on pipes ranging from 0·014 to 0·5 metre in diameter.

Taking no notice of the empirical laws by which Darcy had endeavoured to represent his results, I had the logarithmic homologues drawn from his published experiments. If my law was general then these logarithmic curves, together with mine, should all shift into coincidence, if each were shifted horizontally through

$$\frac{D^3}{P^2},$$

and vertically through

$$\frac{D}{P}.$$

In calculating these shifts there were some doubtful points. Darcy's pipes were not uniform between the gauge points, the sections varying as much as 20 per cent., and the temperature was only casually given. These matters rendered a close agreement unlikely. It was rather a question of seeing if there was any systematic disagreement. When the curves came to be shifted the agreement was remarkable. In only one respect was there any systematic disagreement, and this only raised another point; it was only in the slopes of the higher portions of the curves. In both my tubes the slopes were as 1·722 to 1; in Darcy's they varied according to the nature of the material, from the lead pipes, which were the same as mine, to 1·92 to 1 with the cast-iron.

This seems to show that the nature of the surface of the pipe has an effect on the law of resistance above the critical velocity.

16. *The critical velocities.*—All the experiments agreed in giving

$$v_c = \frac{1}{278}\frac{P}{D}$$

as the critical velocity, to which corresponds as the critical slope of pressure

$$i_c = \frac{1}{47700000}\frac{P^2}{D^3},$$

the units being metres and degrees centigrade. It will be observed that this value is much less than the critical velocity at which steady motion broke down; the ratio being 43·7 to 278.

17. *The general law of resistance.*—The logarithmic homologues all consist of two straight branches, the lower branch inclined at 45 degrees and the upper one at n horizontal to 1 vertical. Except for the small distance beyond the critical velocity these branches constitute the curves. These two branches meet in a point on the curve at a definite distance below the critical pressure, so that, ignoring the small portion of the curve above the point before it again coincides with the upper branch, the logarithmic homologue gives for the law of resistance for all pipes and all velocities

$$A\frac{D^3}{\rho^2}i = \left(B\frac{D}{\rho}v\right)^n,$$

where n has the value unity as long as either number is below unity, and then takes the value of the slope n to 1 for the particular surface of the pipe.

If the units are metres and degrees centigrade,

$$A = 67{,}700{,}000,$$

$$B = 396,$$

$$P = (1 + 0{\cdot}0336\,T + 0{\cdot}000221\,T^2)^{-1}.$$

This equation then, excluding the region immediately about the critical velocity, gives the law of resistance in Poiseuille's tubes, those of the present investigation and Darcy's, the range of diameters being

from 0·000013 (Poiseuille, 1845)
to 0·5 (Darcy, 1857),

and the range of velocities

$$\left.\begin{matrix}\text{from } 0{\cdot}0026 \\ \text{to } 7{\cdot}00\end{matrix}\right\}\text{ metres per sec., 1883.}$$

This algebraical formula shows that the experiments entirely accord with the theoretical conclusions.

The empirical constants are A, B, P, and n; the first three relate solely to the dimensional properties of the fluid summed up in the viscosity, and it seems probable that the last relates to the properties of the surface of the pipe.

Much of the success of the experiments is due to the care and skill of Mr Foster, of Owens College, who has constructed the apparatus and assisted me in making the experiments.

SECTION II.

Experiments in glass tubes by means of colour bands.

18. In commencing these experiments it was impossible to form any very definite idea of the velocity at which eddies might make their appearance with a particular tube. The experiments of Poiseuille showed that the law of resistance varying as the velocity broke down in a pipe of say 0·6 millim. diameter; and the experiments of Darcy showed this law did not hold in a half-inch pipe with a velocity of 6 inches per second.

These considerations, together with the comparative ease with which experiments on a small scale can be made, led me to commence with the smallest tube in which I could hope to perceive what was going on with the naked eye, expecting confidently that eddies would make their appearance at an easily attained velocity.

19. *The first apparatus.*—This consisted of a tube about ¼ inch or 6 millims. in diameter. This was bent into the siphon form having one straight limb about 2 feet long and the other about 5 feet (Fig. 10).

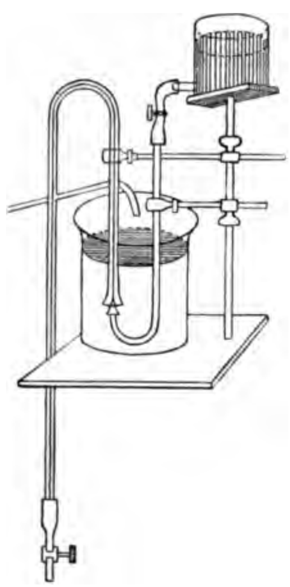


Fig. 10.

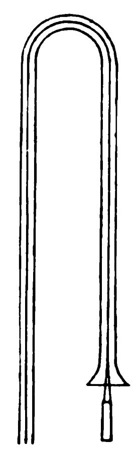


Fig. 10′.

The end of the shorter limb was expanded to a bell mouth, while the nger end was provided with an indiarubber extension on which was a screw ip.

The bell-mouthed limb was held vertically in the middle of a beaker, ith the mouth several inches from the bottom as shown in Figs. 10 and 10′.

A colour tube about 6 millims. in diameter, also of siphon form, was laced as shown in the figure, with the open end of the shorter limb just nder the bell mouth, the longer limb communicating through a controlling ip with a reservoir of highly coloured water placed at a sufficient height. . supply-pipe was led into the beaker for the purpose of filling it; but not ith the idea of maintaining it full, as it seemed probable that the inflowing ater would create too much disturbance, experience having shown how nportant perfect internal rest is to successful experiments with coloured ater.

20. *The first experiment.*—The vessels and the siphons having been filled nd allowed to stand for some hours so as to allow all internal motion to ease, the colour clip was opened so as to allow the colour to emerge slowly elow the bell (Fig. 11).

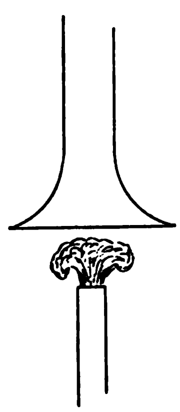


Fig. 11.

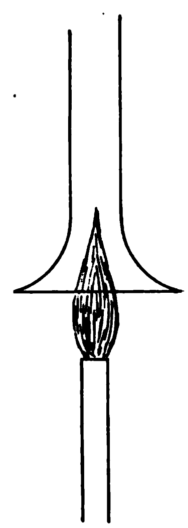


Fig. 12.

Then the clip on the running pipe was opened very gradually. The ater was drawn in at the bell mouth, and the coloured water entered, at rst taking the form of a candle flame (Fig. 12), which continually elongated ntil it became a very fine streak, contracting immediately on leaving the olour-tube, and extending all along the tube from the bell mouth to the utlet (Fig. 10). On further opening the regulating clip so as to increase the velocity of flow, the supply of colour remaining unaltered, the only effect was to diminish the thickness of the colour band. This was again increased by increasing the supply of colour, and so on until the velocity was the greatest that circumstances would allow—until the clip was fully open. Still the colour band was perfectly clear and definite throughout the tube. It was apparent that if there were to be eddies it must be at a higher velocity. To obtain this about 2 feet more were added to the longer leg of the siphon, which brought it down to the floor.

On trying the experiment with this addition, the colour band was still clear and undisturbed.

So that for want of power to obtain greater velocity this experiment failed to show eddies.

When the supply pipe which filled the beaker was kept running during the experiment, it kept the water in the beaker in a certain state of disturbance. The effect of this disturbance was to disturb the colour band in the tube, but it was extremely difficult to say whether this was due to the wavering of the colour band or to genuine eddies.

21. *The final apparatus.*—This was on a much larger scale than the first. A straight tube, nearly 5 feet long and about an inch in diameter, was selected from a large number as being the most nearly uniform, the variation of the diameter being less than 1-32nd of an inch.

The ends of this tube were ground off plane, and on the end which appeared slightly the larger was fitted a trumpet mouth of varnished wood, great care being taken to make the surface of the wood continuous with that of the glass (Fig. 13).

The other end of the glass pipe was connected by means of an indiarubber washer with an iron pipe nearly 2 inches in diameter.

The iron pipe passed horizontally through the end of a tank, 6 feet long, 18 inches broad and 18 inches deep, and then bent through a quadrant so that it became vertical, and reached 7 feet below the glass tube. It then terminated in a large cock, having, when open, a clear way of nearly a square inch.

This cock was controlled by a long lever reaching up to the level of the tank. The tank was raised upon trestles about 7 feet above the floor, and on each side of it, at 4 feet from the ground, was a platform for the observers. The glass tube thus extended in a horizontal direction along the middle of the tank, and the trumpet mouth was something less than a foot from the end. Through this end, just opposite the trumpet, was a straight colour tube three-quarters of an inch in diameter, and this tube was connected, by means of an indiarubber tube with a clip upon it, with a reservoir of colour, which for good reasons subsequently took the form of a common water bottle.

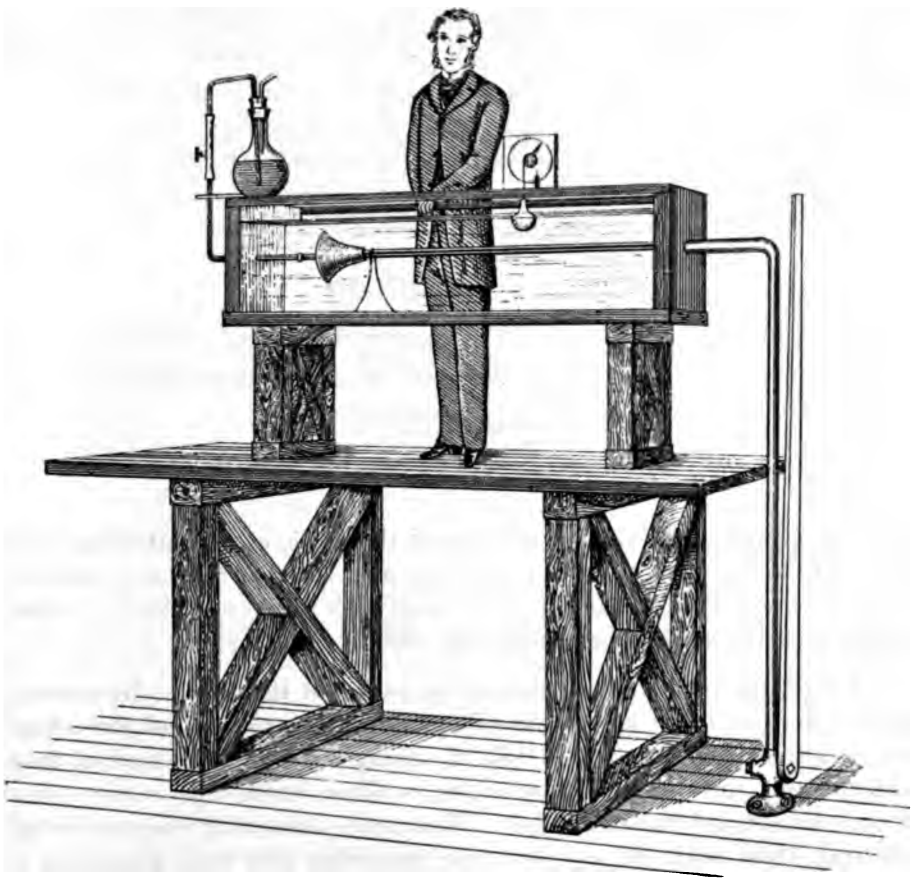


Fig. 13.

With a view to determining the velocity of flow, an instrument was fitted for showing the changes of level of the water in the tank to the 100th of an inch (Fig. 14). Thermometers were hung at various levels in the tank.

22. *The final experiments.*—The first experiment with this apparatus was made on 22nd February, 1880.

By means of a hose the tank was filled from the water main, and having been allowed to stand for several hours, from 10 A.M. to 2 P.M., it was then found that the water had a temperature of 46° F. at the bottom of the tank, and 47° F. at the top. The experiment was then commenced in the same manner as in the first trials. The colour was allowed to flow very slowly, and the cock slightly opened. The colour band established itself much as before, and remained beautifully steady as the velocity was increased until,

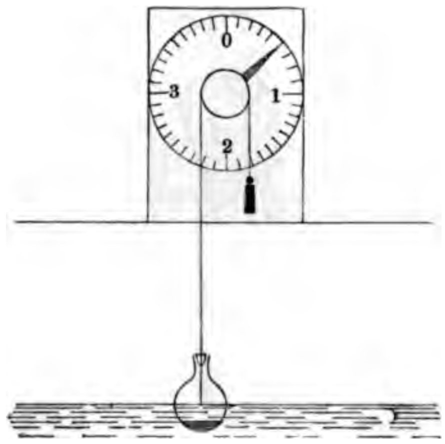


Fig. 14.

all at once, on a slight further opening of the valve, at a point about two feet from the iron pipe, the colour band appeared to expand and mix with the water so as to fill the remainder of the pipe with a coloured cloud, of what appeared at first sight to be of a uniform tint (Fig. 4, p. 59).

Closer inspection, however, showed the nature of this cloud. By moving the eye so as to follow the motion of the water, the expansion of the colour band resolved itself into a well-defined waving motion of the band, at first without other disturbance, but after two or three waves came a succession of well-defined and distinct eddies. These were sufficiently recognisable by following them with the eye, but more distinctly seen by a flash from a spark, when they appeared as in Fig. 5, p. 60.

The first time these were seen the velocity of the water was such that the tank fell 1 inch in 1 minute, which gave a velocity of $0^{m}{\cdot}627$, or 2 feet per second. On slightly closing the valve the eddies disappeared, and the straight colour band established itself.

Having thus proved the existence of eddies, and that they came into existence at a certain definite velocity, attention was directed to the relations between this critical velocity, the size of the tube, and the viscosity.

Two more tubes (2 and 3) were prepared similar in length and mounting to the first, but having diameters of about one-half and one-quarter inch respectively.

In the meantime an attempt was made to ascertain the effect of viscosity y using water at different temperatures. The temperature of the water om the main was about 45°, the temperature of the room about 54°; to btain a still higher temperature, the tank was heated to 70° by a jet of ;eam. Then taking, as nearly as we could tell, similar disturbances, the xperiments which are numbered 1 and 2 in Table I., page 74, were made.

To compare these for the viscosity, Poiseuille's experiments were available, ut to prevent any accidental peculiarity of the water being overlooked, xperiments after the same manner as Poiseuille's were made with the water ı the tank. The results of these however agreed so exactly with those of 'oiseuille that the comparative effect of viscosity was taken from Poiseuille's ›rmula

$$P^{-1} = 1 + 0{\cdot}03368T + 0{\cdot}000221T^2,$$

here $P \propto \mu$ with the temperature and T is temperature centigrade.

The relative values of P at 47° and 70° Fah. are as

$$1{\cdot}3936 \text{ to } 1,$$

hile the relative critical velocities at these temperatures were as

$$1{\cdot}45 \text{ to } 1,$$

hich agreement is very close considering the nature of the experiments.

But whatever might have been the cause of the previous anomalies, ıese were greatly augmented in the heated tank. After being heated ıe tank had been allowed to stand for an hour or two, in order to become ;eady. On opening the valve it was thought that the eddies presented a ifferent appearance from those in the colder water, and the thought at once ıggested itself that this was due to some source of initial disturbance. everal sources of such disturbance suggested themselves—the temperature f the tank was 11° C. above that of the room, and the cooling arising from ıe top and sides of the tank must cause circulation in the tank. A few ;reaks of colour added to the water soon showed that such a circulation xisted, although it was very slow. Another source of possible disturbance 'as the difference in the temperature at the top and bottom of the tank, his had been as much as 5°.

In order to get rid of these sources of disturbance it was necessary to ave the tank at the same temperature as the room, about 54° or 55°. 'hen it was found by several trials that the eddies came on at a fall of bout 1 inch in 64 seconds, which, taking the viscosity into account, was igher than in the previous case, and this was taken to indicate that there 'as less disturbance in the water.

As it was difficult to alter the temperatures of the building so as to obtain xperiments under like conditions at a higher temperature, and it appeared that the same object would be accomplished by cooling the water to its maximum density, 40°, this plan was adopted and answered well, ice being used to cool the water.

Experiments were then made with three tubes 1, 2, 3, at temperatures of about 51° and 40°. All are given in Table I.

Table I.

Experiments with Colour Bands—Critical Velocities at which Steady Motion breaks down.

Pipe No, 1, glass.—Diameter 0·0268 metre; log diameter $\bar{2}$·42828.
„ No. 2, „ „ 0·01527 „ „ $\bar{2}$·18400.
„ No. 3, „ „ 0·007886 „ „ $\bar{3}$·89783.

Discharge, cub. metre = ·021237; log = $\bar{2}$·32709.

Date, 1880	Reference number	Pipe	Temperature, centigrade	Time of discharge	Velocity, metres	log time	$-\log P$	$\log V$	$\log B_s$
1 March	1	No. 1	8·3	60	0·6270	1·77815	0·11242	$\bar{1}$·79729	1·66200
3 „	2	„	21	87	0·4325	1·93959	0·25654	$\bar{1}$·63593	1·67930
25 „	3	„	15	70	0·5374	1·84500	0·19198	$\bar{1}$·73035	1·64936
21 April	4	„	12	60	0·6270	1·77815	0·15712	$\bar{1}$·79729	1·61730
„	5	„	13	64	0·5878	1·80618	0·16882	$\bar{1}$·76926	1·64464
„	6	„	13	67	0·5614	1·82617	0·16882	$\bar{1}$·74927	1·65363
„	7	„	13	64	0·5878	1·80618	0·16882	$\bar{1}$·76926	1·64464
„	8	„	5	54	0·6967	1·73239	0·06963	$\bar{1}$·84305	1·65898
„	9	„	5	52	0·7235	1·71600	0·06963	$\bar{1}$·85940	1·64269
22 „	10	„	10	62	0·6068	1·79239	0·13319	$\bar{1}$·78305	1·65546
„	11	„	11	64	0·5870	1·80613	0·14525	$\bar{1}$·76931	1·65716
25 March	12	No. 2	22	155	0·7476	2·19033	0·26710	$\bar{1}$·87367	1·67523
23 April	13	„	11	110	1·052	2·04139	0·14525	0·02261	1·64814
„	14	„	11	108	1·072	2·03342	0·14525	0·03058	1·64017
„	15	„	4	83	1·396	1·91907	0·05621	0·14493	1·61486
„	16	„	4	83	1·396	1·91907	0·05621	0·14493	1·61486
„	17	„	4	83	1·396	1·91907	0·05621	0·14493	1·61486
„	18	„	6	86	1·348	1·93449	0·08278	0·12951	1·59371
„	19	„	6	85	1·362	1·92941	0·08278	0·13459	1·59863
24 „	20	No. 3	11	220	1·967	2·34242	0·14525	0·29392	1·66300
„	21	„	10·5	224	1·932	2·35024	0·13920	0·28610	1·67687
„	22	„	11	218	1·982	2·33845	0·14525	0·29789	1·65903
„	23	„	11	116	2·004	2·33445	0·14525	0·30189	1·65503
25 „	24	„	4	164	2·637	2·21484	0·05621	0·42150	1·62446
„	25	„	4	172	2·517	2·23552	0·05621	0·40082	1·64514
„	26	„	6	176	2·460	2·24551	0·08278	0·39083	1·62856
„	27	„	6	176	2·460	2·24551	0·08278	0·39083	1·62856
„	28	„	6	174	2·488	2·24054	0·08278	0·39580	1·62359
„	29	„	6	177	2·446	2·24791	0·08278	0·38837	1·63102

This gives the mean value for $\log B_s$ 1·64139; and $B_s = 43·79$.

In reducing the results the unit taken has been the metre and the temperature is given in degrees centigrade.

The diameters of the three tubes were found by filling them with water.

The time measured was the time in which the tank fell 1 inch, which in cubic metres is given by

$$Q = \cdot 021237.$$

In the table the logarithms of P, v, and B_s are given, as well as the natural numbers for the sake of reference.

The velocities v have been obtained by the formula

$$v = \frac{4Q}{\pi D^2},$$

B_s being obtained from the formula

$$B_s = \frac{P}{vD}.$$

The final value of B_s is obtained from the mean value of the logarithm of B_s.

23. *The results.*—The values of $\log B_s$ show a considerable amount of regularity, and prove, I think conclusively, not only the existence of a critical velocity at which eddies come in, but that it is proportional to the viscosity and inversely proportional to the diameter of the tube.

The fact, however, that this relation has only been obtained by the utmost care to reduce the internal disturbances in the water to a minimum must not be lost sight of.

The fact that the steady motion breaks down suddenly shows that the fluid is in a state of instability for disturbances of the magnitude which cause it to break down. But the fact that in some conditions it will break down for a large disturbance, while it is stable for a smaller disturbance shows that there is a certain residual stability so long as the disturbances do not exceed a given amount.

The only idea that I had formed before commencing the experiments was that at some critical velocity the motion must become unstable, so that any disturbance from perfectly steady motion would result in eddies.

I had not been able to form any idea as to any particular form of disturbance being necessary. But experience having shown the impossibility of obtaining absolutely steady motion, I had not doubted but that appearance of eddies would be almost simultaneous with the condition of instability.

I had not, therefore, considered the disturbances except to try and diminish them as much as possible. I had expected to see the eddies make their appearance as the velocity increased, at first in a slow or feeble manner, indicating that the water was but slightly unstable. And it was a matter of surprise to me to see the sudden force with which the eddies sprang into existence, showing a highly unstable condition to have existed at the time the steady motion broke down.

This at once suggested the idea that the condition might be one of instability for disturbance of a certain magnitude and stable for smaller disturbances.

In order to test this, an open coil of wire, as in Fig. 15, was placed in the tube so as to create a definite disturbance.

Fig. 15.

Eddies now showed themselves at a velocity of less than half the previous critical velocity, and these eddies broke up the colour band, but it was difficult to say whether the motion was really unstable or whether the eddies were the result of the initial disturbance, for the colour band having once broken up and become mixed with the water, it was impossible to say whether the motion did not tend to become steady again later on in the tube.

Subsequent observation however tended to show that the critical value of the velocity depended to some extent on the initial steadiness of the water. One phenomenon in particular was very marked.

Where there was any considerable disturbance in the water of the tank and the cock was opened very gradually, the state of disturbance would first show itself by the wavering about of the colour band in the tube; sometimes it would be driven against the glass and would spread out, and all without a symptom of eddies. Then, as the velocity increased but was still comparatively small, eddies, and often very regular eddies, would show themselves along the latter part of the tube. On further opening the cock these eddies would disappear and the colour band would become fixed and steady right through the tube, which condition it would maintain until the velocity reached its normal critical value, and then the eddies would appear suddenly as before.

Another phenomenon very marked in the smaller tubes, was the intermittent character of the disturbance. The disturbance would suddenly come on through a certain length of the tube and pass away and then come on again, giving the appearance of flashes, and these flashes would often commence successively at one point in the pipe. The appearance when the flashes succeeded each other rapidly was as shown in Fig. 16.

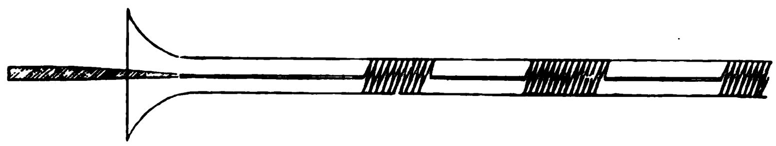


Fig. 16.

This condition of flashing was quite as marked when the water in the tank was very steady, as when somewhat disturbed.

Under no circumstances would the disturbance occur nearer to the trumpet than about 30 diameters in any of the pipes, and the flashes generally, but not always, commenced at about this distance.

In the smaller tubes generally, and with the larger tube in the case of the ice-cold water at 40°, the first evidence of instability was an occasional flash beginning at the usual place and passing out as a disturbed patch two or three inches long. As the velocity was further increased these flashes became more frequent until the disturbance became general.

I did not see a way to any very crucial test as to whether the steady motion became unstable for a large disturbance before it did so for a small one; but the general impression left on my mind was that it did in some way—as though disturbances in the tank, or arising from irregularities in the tube, were necessary to the existence of a state of instability.

But whatever these peculiarities may mean as to the way in which eddies present themselves, the broad fact of there being a critical value for the velocity at which the steady motion becomes unstable, which critical value is proportional to

$$\frac{\mu}{\rho c},$$

where c is the diameter of the pipe and μ/ρ the viscosity by the density, is abundantly established. And cylindrical glass pipes for approximately steady water have for the critical value

$$v = \frac{P}{B_s D},$$

where in metres $B_s = 43{\cdot}79$ about.

SECTION III.

Experiments to determine the critical velocity by means of resistance in the pipes.

24. Although at first sight such experiments may appear to be simple enough, yet when one began to consider actual ways and means, so many uncertainties and difficulties presented themselves, that the necessary courage for undertaking them was only acquired after two years' further study of the hydrodynamical aspect of the subject, by the light thrown upon it by the previous experiment with the colour bands. This has been already explained in Art. 13. Those experiments had shown definitely that there was a critical value of the velocity at which eddies began, if the water were approximately steady when drawn into the tube, but they had also shown definitely, that at such critical velocity, the water in the tube was in a highly unstable condition; any considerable disturbance in the water causing the break down to occur at velocities much below the highest that could be attained when the water was at its steadiest; suggesting that if there were a critical velocity at which, for any disturbance whatever, the water became stable, this velocity was much less than that at which it would become unstable for infinitely small disturbances; or, in other words, suggesting that there were two critical values for the velocity in the tube, the one at which steady motion changed into eddies, the other at which eddies changed into steady motion.

Although the law for the critical value of the velocity had been suggested by the equations of motion, it was, as already explained, only at the beginning of this year that I succeeded in dealing with these equations so as to obtain any theoretical explanation of the dual criteria; but having at last found this, it became clear to me that, if in a tube of sufficient length the water were at first admitted in a high state of disturbance, then as the water proceeded along the tube, the disturbance would settle down into a steady condition, which condition would be one of eddies or steady motion, according to whether the velocity was above or below what may be called the real critical value.

The necessity of initial disturbance precluded the method of colour bands, so that the only method left was to measure the resistance at the latter portion of the tube in conjunction with the discharge.

The necessary condition was somewhat difficult to obtain. The change in the law of resistance could only be ascertained by a series of experiments which had to be carried out under similar conditions as regards temperature, kind of water, and condition of the pipe; and in order that the experiments might be satisfactory, it seemed necessary that the range of velocities should extend far on each side of the critical velocity. In order to best ensure these conditions, it was resolved to draw the water direct from the Manchester main, using the pressure in the main for forcing the water through the pipes. The experiments were conducted in the workshop in Owens College, which offered considerable facilities owing to arrangements for supplying and measuring the water used in experimental turbines.

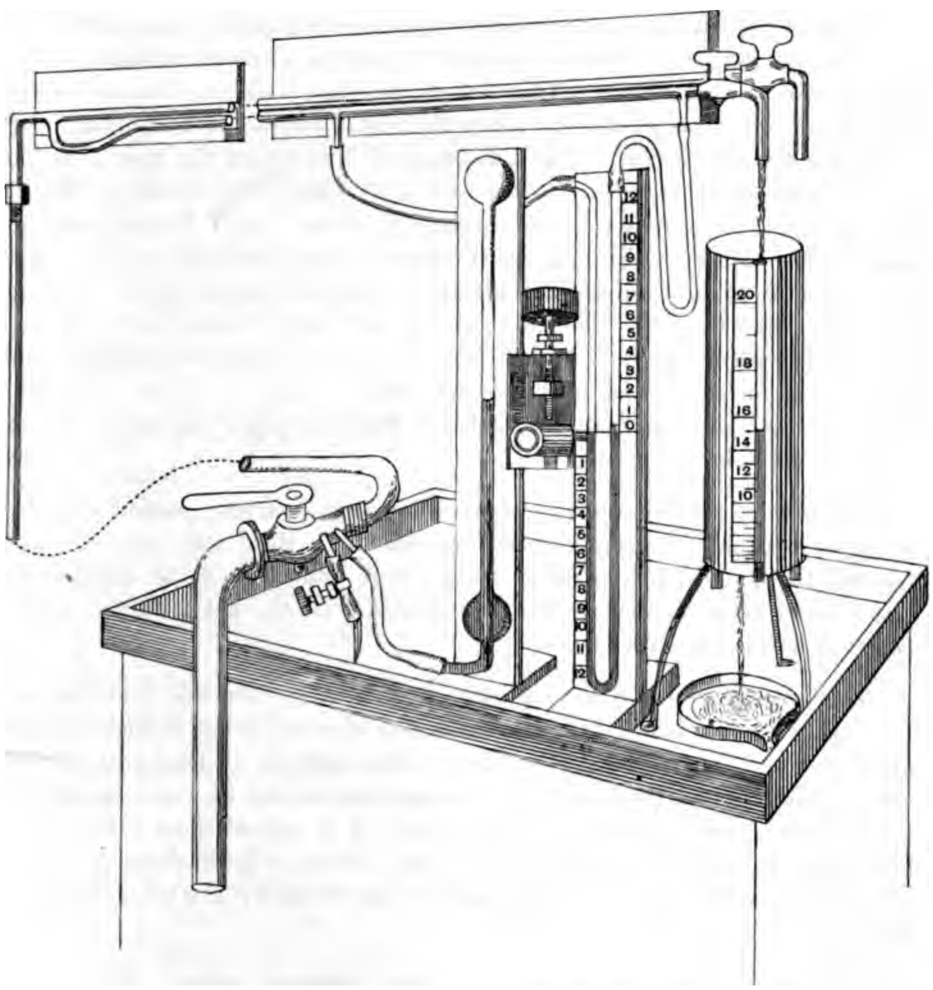


Fig. 17.

25. The apparatus is shown in Fig. 17.

As the critical value under consideration would be considerably that found for the change for steady motion into eddies, a diameter of half an inch (12 millims.) was chosen for the larger pipe, and one quai an inch for the smaller, such pipes being the smallest used in the pre experiments.

The pipes (4 and 5) were ordinary lead gas, or water pipes. These, which, owing to their construction, are very uniform in diameter, and when new, present a bright metal surface inside, seemed well adapted for the purpose.

Pipes 4 (which was a quarter-inch pipe) and 5 (which was a half-inch) were 16 feet long, straightened by laying them in a trough formed by two inch boards at right angles. This trough was then fixed so that one side of the trough was vertical and the other horizontal, forming a horizontal ledge on which the pipes could rest at a distance of 7 feet from the floor; on the outflow ends of the pipes cocks were fitted to control the discharge, and at the inlet end the pipes were connected, by means of a T branch, with an indiarubber hose from the main; this connexion was purposely made in such a manner as to necessitate considerable disturbance in the water entering the pipes from the hose. The hose was connected, by means of a quarter-inch cock, with a four-inch branch from the main. With this arrangement the pressure on the inlet to the pipes was under control of the cock from the main, and at the same time the discharge from the pipes was under control from the cocks on their ends.

This double control was necessary owing to the varying pressure in the main, and after a few preliminary experiments a third and more delicate control, together with a pressure gauge, were added, so as to enable the observer to keep the pressure in the hose, *i.e.*, on the inlets to the pipes, constant during the experiments.

This arrangement was accomplished by two short branches between the hose and the control cock from the main, one of these being furnished with an indiarubber mouthpiece with a screw clip upon it, so that part of the water which passed the cock might be allowed to run to waste, the other branch being connected with the lower end of a vertical glass tube, about 6 millims. in diameter and 30 inches long, having a bulb about 2 inches diameter near its lower extremity, and being closed by a similar bulb at its top.

This arrangement served as a delicate pressure gauge. The water entering at the lower end forced the air from the lower bulb into the upper, causing a pressure of about 30 inches of mercury. Any further rise increased this pressure by forcing the air in the tubes into the upper bulb, and by the weight of water in the tube. During an experiment the screw clip was continually adjusted, so as to keep the level of the water in the glass tube between the bulbs constant.

26. *The resistance gauges.*—Only the last 5 feet of the tube was used for measuring the resistance, the first 10 or 11 feet being allowed for the acquirement of a regular condition of flow.

It was a matter of guessing that 10 feet would be sufficient for this, but since, compared with the diameter, this length was double as great for the smaller tube, it was expected that any insufficiency would show itself in a greater irregularity of the results obtained with the larger tube, and as no such irregularity was noticed it appears to have been sufficient.

At distances of 5 feet near the ends of the pipe, two holes of about 1 millim. were pierced into each of the pipes for the purpose of gauging the pressures at these points of the pipes. As owing to the rapid motion of the water in the pipes past these holes, any burr or roughness caused in the inside of the pipe in piercing these holes would be apt to cause a disturbance in the pressure, it was very important that this should be avoided. This at first seemed difficult, as owing to the distance—5 feet—of one of the holes from the end of pipes of such small diameter, the removal of a burr, which would be certain to ensue on drilling the holes from the outside, was difficult. This was overcome by the simple expedient suggested by Mr Foster of drilling holes completely through the pipes and then plugging the side on which the drill entered. Trials were made, and it was found that the burr thus caused was very slight.

Before drilling the holes short tubes had been soldered to the pipes, so that the holes communicated with these tubes; these tubes were then connected with the limbs of a siphon gauge by indiarubber pipes.

These gauges were about 30 inches long; two were used, the one containing mercury, the other bisulphide of carbon.

These gauges were constructed by bending a piece of glass tube into a U form, so that the two limbs were parallel and at about one inch apart.

Glass tubes are seldom quite uniform in diameter, and there was a difference in the size of the limbs of both gauges, the difference being considerable in the case of the bisulphide of carbon.

The tubes were fixed to stands with carefully graduated scales behind them, so that the height of the mercury or carbon in each limb could be read. It had been anticipated that readings taken in this way would be sufficient. But it turned out to be desirable to read variations of level of the smallness of $\frac{1}{1000}$th of an inch or $\frac{1}{40}$th of a millimetre.

A species of cathetometer was used. This had been constructed for my experiments on Thermal Transpiration, and would read the position of the division surface of two fluids to $\frac{1}{10000}$th inch (page 258, Vol. I.).

The water was carefully brought into direct connexion with the fluid in the gauge, the indiarubber connexions facilitating the removal of all air.

27. *Means adopted in measuring the discharge.*—For many reasons it was very desirable to measure the rate of discharge in as short a time as possible.

For this purpose a species of orifice or weir gauge was constructed, consisting of a vertical tin cylinder two feet deep, having a flat bottom, being open at the top, with a diaphragm consisting of many thicknesses of fine wire gauze about two inches from the bottom; a tube connected the bottom with a vertical glass tube, the height of water in which showed the pressure of water on the bottom of the gauze; behind this tube was a scale divided so that the divisions were as the square roots of the height. Through the thin tin bottom were drilled six holes, one an eighth of an inch diameter, one a quarter of an inch, and four of half an inch.

These holes were closed by corks so that any one or any combination could be used.

The combinations used were:

Gauge No. 1. The $\frac{1}{8}$ inch hole alone.
No. 2. The $\frac{1}{4}$ inch hole alone.
No. 3. A $\frac{1}{2}$ inch hole alone.
No. 4. Two $\frac{1}{2}$ inch holes.
No. 5. Four $\frac{1}{2}$ inch holes.

According to experience, the velocity with which water flows from a still vessel through a round hole in a thin horizontal plate is very nearly proportional to the area of the hole and the square root of the pressure, so that with any particular hole the relative quantities of water discharged would be read off at the variable height gauge. The accuracy of the gauge, as well as the absolute values of the readings, was checked by comparing the readings on the gauge with the time taken to fill vessels of known capacity. In this way coefficients for each one of the combinations 1, 2, 3, 4, 5 were obtained as follows:—

TABLE II.

No. of Gauge	Readings on Gauge	Time	Quantity	Coefficient	Logarithmic coefficient
		Seconds	c.c.		
Gauge No. 1	19·55	61	1160	·966	$\bar{1}$·985
ib.	—	59	1160		
No. 2	5·3	54	1160	4·055	·608
ib.	15·3 full	—	*A*	4·055	—
No. 3	15	360	*A*	16·220	1·210
No. 4	15	178	*A*	32·440	1·511
No. 5	15	90	*A*	64·880	1·812

From this table it will be seen that the absolute values of the coefficients were obtained from experiments on the gauges No. 1 and No. 2, the coefficients for the gauges 3, 4, and 5 being determined by comparison of the times taken to fill a vessel of unknown capacity, which stands in the Table as *A*. The relative value of these coefficients came out sensibly proportional to the squares of the diameters of the apertures.

For the smaller velocities it was found that the gauge No. 1 was too large, and in order not to delay the experiment in progress, two glass flasks were used: these are distinguished as flasks (1) and (2); their capacities, as subsequently determined with care, were 303 and 1160 c.c. The discharge as measured by the times taken to fill these flasks are reduced to c.c. per second by dividing the capacities of the flasks by the times.

28. The method of carrying out the experiments was generally as follows:—My assistant, Mr Foster, had charge of the supply of water from the main, keeping the water in the pressure gauge at a fixed level.

The tap at the end of the tube to be experimented upon being closed, the zero reading of the gauge was carefully marked, and the micrometer adjusted so that the spider line was on the division of water and fluid in the left-hand limb of the gauge. The screw was then turned through one entire revolution, which lowered the spider line one-fiftieth of an inch; the tap at the end of the pipe was then adjusted until the fluid in the gauge came down to the spider line; having found that it was steady there, the discharge was measured.

This having been done, the spider line was lowered by another complete revolution of the screw, the tap again adjusted, and so on, for about 20 readings, which meant about half an inch difference in the gauge. Then the readings were taken for every five turns of the screw until the limit of the range, about 2 inches, was reached. After this, readings were taken by simple observation of the scale attached to the gauge. In taking these readings the best plan was to read the position of the mercury or carbon in both limbs of the gauge, but this was not always done, some of the readings entered in the notes referred to one or other limb of the gauge, care having been taken to indicate which.

In the Tables III., IV., and V. of results appended, the noted readings are given and the letters r, l, and b signify whether the reading was on the right or left limb, or the sum of the readings on both limbs.

The readings marked l and r are reduced by the correction for the difference in the size of the limbs as well as the coefficient for the particular fluid in the gauge.

Thus it was found with the mercury tube that when the left limb had moved through 39 divisions on the scale the right had moved through 41, so that to obtain the sum of these readings, the readings on the left, or those marked l, had to be multiplied by 2·05, and those on the right by 1·95.

With the bisulphide of carbon gauge, 11 divisions on the left caused 9 on the right, so that the correction for the reading on the left was 1·8 and on the right 2·2.

29. *Comparison of the pressure gauges.*—The pressures as marked by the gauges were reduced to the same standard by comparing the gauges; thus ·25 of the left limb of the mercury corresponded with 24 inches on both limbs of the bisulphide. Therefore to reduce the readings of the bisulphide of carbon to the same scale as those of the mercury they were multiplied by

$$\frac{\cdot 25 \times 20\cdot 5}{24} = 0\cdot 0213.$$

This brought the readings of pressure to the same standard, *i.e.*, $\frac{1}{1000}$th of an inch of mercury, but these were further reduced by the factor 0·00032 to bring them to metres of water.

As it was convenient for the sake of comparison to obtain the differences of pressure per unit length of the pipe, the pressures in metres of water have been divided by 1·524, the length in metres between the gauge holes, and these reductions are included in the tables of results in the column headed i.

From the discharges, as measured by the various gauges, reduced to cubic centimetres, the mean velocity of the water was found by dividing by the area of the section of the pipe.

30. *Sections and diameters of the pipes.*—The areas were obtained by carefully measuring the diameters by means of fitting brass plugs into the pipes, and then measuring the plugs. In this way the diameters were found to be—

Diameter, No. 4 pipe, ·242 inch, 6·15 millims.
„ No. 5 pipe, ·498 inch, 12·7 millims.

These gave the areas of the sections—

Section, No. 4 pipe, 29·7 square millims.
„ No. 5 pipe, 125 square millims.

The discharge in cubic centimetres, divided by the area of section in square millimetres, gave the mean velocity in metres per second, as given in the Tables III., IV., and V.

The logarithms of i and v are given for the sake of comparison.

31. *The temperature.*—The chief reason why the water from the main had been used, was from the necessity of having constant temperature throughout the experiments, and my previous experience of the great constancy of the temperature of the water in the mains, even over a period of some weeks.

At the commencement of the experiments the temperature of the water when flowing freely was found to be 5° C. or 41° F., and it remained the same throughout the experiments. Nevertheless, a fact which had been overlooked caused the temperature in the pipes to vary somewhat and in a manner somewhat difficult to determine.

This fact, which was not discovered until after the experiments had been reduced, was that the temperature of the workshop being above that of the main, the water would be warmed in flowing through the pipes to an extent depending on its flow. The possibility of this had not been altogether overlooked, and an early observation was made to see if any such warming occurred, but as it was found to be less than half a degree no further notice was taken until on reducing the results it was found that the velocities obtained with the very smallest discharges presented considerable discrepancies in various experiments; this suggested the cause.

The discrepancies were not serious if explained, so that all that was necessary was to carefully repeat the experiments at the lower velocities observing the temperatures of the effluent water. This was done, and further experiments were made (see Art. 33).

TABLE III. Experiments on Resistance in Pipes made January 29, 1883.

Pipe No. 4, lead.—Diameter (as measured 0·242 inch), 6·15 millims. Length: total, 16 feet; to first gauge hole, 9·6 feet; between gauge holes (5 feet), 1·524 metres. Water from the Manchester Main.

Reference number	Pressures: Mercury in water	Pressures: Bisulphide of carbon in water	Pressures: Reduced to metres of water	Discharges: Time in seconds taken to fill flask: 1. 303 c.c.	2. 1160 c.c.	3. 500 c.c.	4. 1000 c.c.	Discharges: Velocity through orifice in thin plate: Gauge No. 2	Gauge No. 3	Gauge No. 4	Gauge No. 5	Discharges: Reduced to c.m. per second	Temperature: Centigrade	Temperature: Fahrenheit	Slope of pressure in water i	Velocity in metres per second v	log i	log v
1	20	...	0·0131	130	...	...	...	...	...	...	...	2·33	12	...	0·0086	0·0785	$\bar{3}$·935	$\bar{2}$·895
2	40	...	0·0262	69	...	...	...	...	...	...	...	4·40	...	...	0·01720	0·1480	$\bar{2}$·236	$\bar{1}$·170
3	60	...	0·0393	45	...	...	...	...	...	...	...	6·73	11	...	0·0258	0·2265	$\bar{2}$·412	$\bar{1}$·355
4	80	...	0·0524	34	...	...	...	...	...	...	...	8·91	...	...	0·0345	0·3000	$\bar{2}$·537	$\bar{1}$·477
5	100	...	0·0656	28	...	...	...	...	...	...	...	10·70	10	...	0·0430	0·3640	$\bar{2}$·634	$\bar{1}$·561
6	120	...	0·0787	23	...	...	...	...	...	...	...	13·2	9	...	0·0516	0·4426	$\bar{2}$·713	$\bar{1}$·646
7	140	Unsteady	0·0918	21	...	...	...	...	...	...	...	14·5	8	...	0·0602	0·4865	$\bar{2}$·780	$\bar{1}$·687
8	160	...	0·1040	...	80	...	...	...	...	...	...	14·5	7	...	0·0682	0·5106	$\bar{2}$·834	$\bar{1}$·708
9	160	...	0·1040	...	76	...	...	...	...	...	...	15·2	6	...	0·0682	0·5106	$\bar{2}$·834	$\bar{1}$·708
10	180	...	0·1181	...	71	...	...	...	...	...	...	16·3	...	...	0·0774	0·5483	$\bar{2}$·889	$\bar{1}$·739
11	200 *l*	...	0·1313	...	71	...	...	...	...	...	...	16·3	...	...	0·0861	0·5483	$\bar{2}$·935	$\bar{1}$·739
12	220	...	0·1443	...	69	...	...	...	...	...	...	16·8	...	...	0·0946	0·5650	$\bar{2}$·976	$\bar{1}$·752
13	240	...	0·1574	...	67	...	...	...	...	...	...	17·3	...	...	0·1033	0·5822	$\bar{1}$·014	$\bar{1}$·765
14	260	...	0·1707	...	66·5	...	...	...	...	...	...	17·4	...	...	0·1120	0·5862	$\bar{1}$·049	$\bar{1}$·768
15	280	...	0·1837	...	64	...	...	...	...	...	...	18·1	6	...	0·1206	0·6096	$\bar{1}$·081	$\bar{1}$·785
16	300	...	0·1968	...	61·5	...	...	...	...	...	...	18·8	...	...	0·1292	0·6339	$\bar{1}$·111	$\bar{1}$·802
17	320	...	0·2099	...	60	...	...	...	...	...	...	19·3	...	...	0·1378	0·6520	$\bar{1}$·139	$\bar{1}$·813
18	320	...	0·2099	...	...	...	...	4·7	...	...	...	19·1	...	...	0·1378	0·6413	$\bar{1}$·139	$\bar{1}$·807
19	400	...	0·2613	...	54	...	...	5·3	...	...	...	21·5	...	...	0·1714	0·7228	$\bar{1}$·234	$\bar{1}$·859
20	500	...	0·3274	...	...	...	...	6·0	...	...	...	24·3	...	...	0·2148	0·8185	$\bar{1}$·332	$\bar{1}$·913
21	700	...	0·4592	...	...	...	...	7·4	...	...	...	30·0	...	...	0·3014	1·033	$\bar{1}$·479	0·014
22	1000	...	0·6562	...	...	...	...	9·4	...	...	...	38·1	...	...	0·4306	1·283	$\bar{1}$·634	0·108
23	1500	...	0·9355	...	...	...	...	11·7	...	...	...	47·5	5	...	0·6138	1·268	$\bar{1}$·788	0·103
24	2000	...	1·2480	...	...	...	...	13·6	...	...	...	55·1	...	...	0·8185	1·854	$\bar{1}$·913	0·268
25	2500	...	1·5560	...	...	...	...	15·8	...	...	...	64·2	...	...	1·021	2·158	0·009	0·334
26	3000	...	1·8710	...	...	...	...	17·5	...	...	...	71·0	...	...	1·228	2·388	0·089	0·378
27	3500	...	2·1830	...	...	...	...	19·1	...	...	...	79·1	...	...	1·433	2·661	0·156	0·425
28	4000 *r*	...	2·4950	...	...	...	...	20·1	...	...	...	81·1	...	...	1·637	2·729	0·214	0·436
29	4000	...	2·4950	...	...	...	...	...	4·9	...	...	79·5	...	...	1·637	2·674	0·214	0·427
30	5000	...	3·1120	...	...	...	...	...	5·7	...	...	92·5	...	...	2·042	3·112	0·310	0·493
31	6000	...	3·7420	...	...	...	...	...	6·5	...	...	105·0	...	...	2·455	3·540	0·390	0·549

Table IV.

Conditions the same as in Table III. except the temperatures at the lower velocities.

Reference number	Pressures: Mercury in water	Pressures: Bisulphide of carbon in water	Pressures: Reduced to metres of water	Discharges: Time in seconds taken to fill flask: 1. 303 c.c.	2. 1160 c.c.	3. 500 c.c.	4. 1000 c.c.	Discharges: Velocity through orifice in thin plate: Gauge No. 2	Gauge No. 3	Gauge No. 4	Gauge No. 5	Discharges: Reduced to c.m. per second	Temperature: Centigrade	Temperature: Fahrenheit	Slope of pressure in water i	Velocity in metres per second v	log i	log v
36	20	...	0·01313	...	...	227	...	...	...	...	...	1·750	10	50	0·008591	0·0740	$\bar{3}$·934	$\bar{2}$·869
37	40	...	0·02625	...	...	131	...	...	...	...	...	3·289	8	46·4	0·01718	0·1390	$\bar{2}$·235	$\bar{1}$·143
38	60	...	0·03936	...	...	80	...	...	...	...	...	4·966	7	44·6	0·02577	0·2100	$\bar{2}$·411	$\bar{1}$·322
39	80	...	0·05249	...	...	61	...	...	...	...	...	6·517	6	42·8	0·03436	0·2755	$\bar{2}$·536	$\bar{1}$·440
40	100 l	...	0·06562	...	...	50·5	...	...	...	...	...	7·871	5	41	0·04296	0·3327	$\bar{2}$·633	$\bar{1}$·522
41	120	...	0·07871	...	...	...	86	...	...	...	...	11·650	5	41	0·05153	0·3918	$\bar{2}$·712	$\bar{1}$·593
42	140	...	0·09184	...	...	...	76	...	...	...	...	13·16	5	41	0·06296	0·4426	$\bar{2}$·779	$\bar{1}$·646
43	160	...	0·1040	...	...	...	66	...	...	...	...	15·18	5	41	0·06808	0·5106	$\bar{2}$·833	$\bar{1}$·708
44	180	Unsteady	0·1181	...	...	...	62	...	...	...	...	16·15	5	41	0·07727	0·5433	$\bar{2}$·888	$\bar{1}$·735
45	200	...	0·1313	...	...	...	61	...	...	...	...	16·41	5	41	0·08591	0·5521	$\bar{2}$·934	$\bar{1}$·742
46	220	...	0·1443	...	...	...	60	...	...	...	...	16·52	5	41	0·09441	0·5560	$\bar{2}$·975	$\bar{1}$·745
47	240 l	...	0·1574	...	...	...	58	...	...	...	...	17·26	5	41	0·1031	0·5808	$\bar{1}$·013	$\bar{1}$·764
48	280	...	0·1837	...	...	...	55	...	...	...	...	18·20	5	41	0·1203	0·6124	$\bar{1}$·080	$\bar{1}$·787
49	320	...	0·2099	...	...	...	52	...	...	...	...	19·06	5	41	0·1375	0·6413	$\bar{1}$·138	$\bar{1}$·807
50	360	...	0·2250	...	...	...	50	...	...	...	...	20·00	5	41	0·1437	0·6730	$\bar{1}$·168	$\bar{1}$·828
51	400	...	0·2625	...	...	...	47	...	...	...	...	21·29	...	41	0·1718	0·7162	$\bar{1}$·235	$\bar{1}$·855

TABLE V.

Pipe No. 5, lead.—Diameter (as measured 0·498 inch), 12·7 millims. Length: total, 16 feet; to first gauge hole, 9·6 feet; between gauge holes (5 feet), 1·524 metres. Water from the Manchester Main.

Reference number	Pressures			Discharges									Temperature		Slope of pressure in water	Velocity in metres per second		
	Mercury in water	Bisulphide of carbon in water	Reduced to metres of water	Time in seconds taken to fill flask			Velocity through orifice in thin plate					Reduced to c.m. per second	Centigrade	Fahrenheit				
				1. 303 c.c.	2. 1160 c.c.	3. 500 c.c.	Gauge No. 1	Gauge No. 2	Gauge No. 3	Gauge No. 4	Gauge No. 5				i	v	log i	log v
52	...	1	0·001219	...	...	...	4·5	...	...	...	...	4·346	12	...	0·00080	0·0346	$\bar{4}$·902	$\bar{2}$·539
53	...	2	0·002438	...	...	...	8·4	...	...	...	...	8·110	...	...	0·00159	0·0646	$\bar{3}$·203	$\bar{2}$·810
54	...	3	0·003656	...	...	...	11·2	...	...	...	...	9·841	...	...	0·00239	0·0784	$\bar{3}$·379	$\bar{2}$·894
55	...	4	0·004876	...	...	...	16·4	...	...	...	...	15·85	11	...	0·00319	0·1262	$\bar{3}$·504	$\bar{1}$·101
56	...	5	0·006082	...	...	...	...	4·4	...	...	...	17·83	...	...	0·00398	0·1420	$\bar{3}$·600	$\bar{1}$·152
57	...	6 l	0·007312	...	...	...	...	5·3	...	...	...	21·48	10	...	0·00478	0·1711	$\bar{3}$·680	$\bar{1}$·233
58	...	7	0·008532	...	...	...	...	6·0	...	...	...	24·33	...	...	0·00558	0·1937	$\bar{3}$·747	$\bar{1}$·287
59	...	8	0·009750	...	...	...	...	7·0	...	...	...	28·38	8	...	0·00638	0·2260	$\bar{3}$·805	$\bar{1}$·354
60	Unsteady	9	0·01097	...	...	...	...	7·0	...	...	...	28·38	...	...	0·00717	0·2260	$\bar{3}$·856	$\bar{1}$·354
61	...	10	0·01219	...	...	...	...	7·6	...	...	...	30·84	7	...	0·00798	0·2455	$\bar{3}$·902	$\bar{1}$·390
62	...	11	0·01340	...	...	...	...	8·0	...	...	...	32·44	...	...	0·00877	0·2583	$\bar{3}$·943	$\bar{1}$·412
63	...	20 b	0·01365	...	...	...	...	8·0	...	...	...	32·44	...	...	0·00893	0·2583	$\bar{3}$·951	$\bar{1}$·412
64	...	12	0·01463	...	...	...	...	8·4	...	...	...	34·05	...	...	0·00957	0·2710	$\bar{3}$·981	$\bar{1}$·433
65	...	13 l	0·01582	...	...	...	...	8·6	...	...	...	34·84	...	...	0·01036	0·2774	$\bar{2}$·015	$\bar{1}$·443
66	...	14	0·01707	...	...	...	...	8·8	...	...	...	35·65	...	...	0·01117	0·2838	$\bar{2}$·048	$\bar{1}$·453
67	...	15	0·01837	...	...	...	...	9·0	...	...	...	36·48	6	...	0·01203	0·2905	$\bar{2}$·080	$\bar{1}$·463

68	...	40	0·02729	...	...	...	...	10·8	...	...	...	43·76	...	...	0·01787	0·3484	$\bar{2}$·252	$\bar{1}$·542
69	...	60	0·04093	...	...	...	...	13·6	...	...	...	55·09	...	...	0·02680	0·4386	$\bar{2}$·428	$\bar{1}$·642
70	...	80	0·05458	...	...	...	...	16·3	...	...	...	66·07	...	...	0·03573	0·5261	$\bar{2}$·553	$\bar{1}$·721
71	...	100	0·06824	...	...	...	...	18·2	...	...	...	73·80	...	...	0·04467	0·5875	$\bar{2}$·650	$\bar{1}$·769
72	...	120	0·08185	...	...	...	...	20·2	...	...	...	81·85	...	...	0·05483	0·653	$\bar{2}$·739	$\bar{1}$·814
73	...	120	...	...	...	...	...	...	4·8	...	...	77·81	5	...	...	0·620	...	$\bar{1}$·792
74	...	140	0·9550	...	...	...	...	...	5·2	...	...	84·34	...	...	0·06252	0·672	$\bar{2}$·796	$\bar{1}$·827
75	...	160 l	0·1092	...	...	...	...	...	5·8	...	...	93·98	...	...	0·07145	0·749	$\bar{2}$·854	$\bar{1}$·874
76	...	170	0·1159	...	...	...	...	...	6·0	...	...	97·28	...	...	0·07586	0·775	$\bar{2}$·880	$\bar{1}$·889
77	...	180	0·1228	...	...	...	...	...	6·3	...	...	102·1	...	...	0·08036	0·813	$\bar{2}$·905	$\bar{1}$·910
78	...	190	0·1295	...	...	...	...	...	6·5	...	...	105·2	...	...	0·08473	0·838	$\bar{2}$·928	$\bar{1}$·923
79	...	200	0·1365	...	...	...	...	...	6·6	...	...	107·0	...	...	0·08934	0·852	$\bar{2}$·951	$\bar{1}$·930
80	...	216	0·1433	...	...	...	...	...	6·8	...	...	110·2	...	...	0·09376	0·878	$\bar{2}$·972	$\bar{1}$·943
81	...	220	0·1500	...	...	...	...	...	7·0	...	...	113·6	...	...	0·09818	0·904	$\bar{2}$·992	$\bar{1}$·956
82	...	230	0·1567	...	...	...	...	...	7·3	...	...	118·4	...	...	0·1026	0·942	$\bar{1}$·011	$\bar{1}$·974
83	...	240	0·1637	...	...	...	...	...	7·5	...	...	121·7	...	...	0·1072	0·993	$\bar{1}$·030	$\bar{1}$·986
84	250 l	...	0·1637	...	...	...	...	...	7·5	...	...	...	5	...	0·1072	...	$\bar{1}$·030	...
85	300	...	0·1968	...	...	...	...	...	8·6	...	...	139·4	...	...	0·1289	1·110	$\bar{1}$·110	0·045
86	400 l	...	0·2625	...	...	...	...	...	10·3	...	...	166·8	...	...	0·1718	1·328	$\bar{1}$·235	0·123
87	500	...	0·3281	...	...	...	...	...	11·7	...	...	189·7	...	...	0·2148	1·511	$\bar{1}$·332	0·179
88	1,000	...	...	...	...	...	...	...	11·5	...	...	186·3	...	...	...	1·483	...	0·171
89	1,500	...	0·4798	...	...	...	...	...	14·2	...	...	230·2	...	...	0·3141	1·833	$\bar{1}$·497	0·263
90	2,000	...	0·6398	...	...	...	...	...	17·3	...	...	280·6	...	...	0·4188	2·234	$\bar{1}$·622	0·349
91	2,000	...	...	...	...	...	...	...	...	8·3	...	269·2	...	...	...	2·143	...	0·331
92	3,000	...	0·9595	...	...	...	...	...	...	10·8	...	350·0	...	...	0·6281	2·787	$\bar{1}$·798	0·445
93	5,000	...	1·596	...	...	...	...	...	...	18·7	...	479·8	...	...	1·045	3·820	0·019	0·582
94	7,000	...	2·239	...	...	...	...	...	...	...	8·7	605·4	...	...	1·466	4·820	0·166	0·683
95	7,000	...	2·239	...	...	...	...	...	...	...	10·2	563·7	...	...	1·466	4·488	0·166	0·652
96	9,000	...	2·878	...	...	...	...	...	...	...	11·4	660·7	...	...	1·884	5·261	0·275	0·721
97	11,000	...	3·516	...	...	...	...	...	...	...	12·4	736·3	...	...	2·302	5·875	0·362	0·769
98	13,000	...	4·150	...	...	...	...	...	...	...	13·5	803·6	5	...	2·717	6·398	0·434	0·806
99	15,000	...	4·798	...	...	...	...	...	...	...	13·7	875·0	...	...	3·141	6·967	0·497	0·843
100	15,500	...	4·955	...	...	...	...	...	...	...	...	887·2	5	...	3·244	7·064	0·511	0·849

32. *The results of the experiments.*—A considerable number of preliminary experiments were made until the results showed a high degree of consistency. Then a complete series of experiments were made consecutively with each tube. The results of these are given in Tables III. and V.

33. *The critical velocities.*—The determination of these, which had been the main object of the experiments, was to some extent accomplished directly during the experiments, for starting from the very lowest velocities, it was found that the fluid in the differential gauge was at first very steady, lowering steadily as the velocity was increased by stages, until a certain point was reached, when there seemed to be something wrong with the gauge. The fluid jumped about, and the smallest adjustment of the tap controlling the velocity sent the fluid in the gauge out of the field of the microscope. At first this unsteadiness always came upon me as a matter of surprise, but after repeating the experiments several times, I learnt to know exactly when to expect it. The point at which this unsteadiness is noted is marked in the tables.

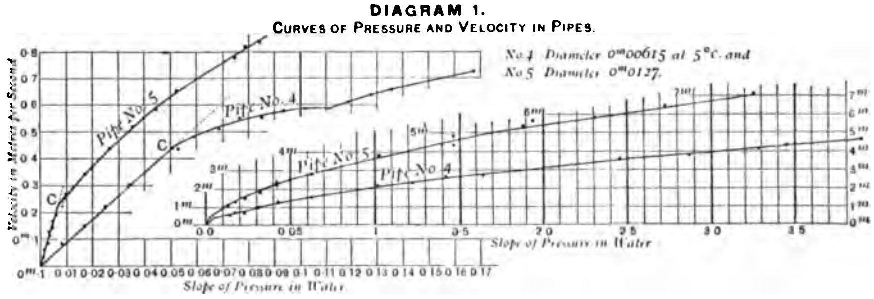
It was not, however, by the unsteadiness of the pressure gauge that the critical velocity was supposed to be determined, but by comparing the ratio of velocities and pressures given in the columns v and i in the tables. This comparison is shown in diagram I. below, the values of i being abscissæ and v ordinates. It is thus seen that for each tube the points which mark the experiments lie very nearly in a straight line up to definite points marked C, at which divergence sets in rapidly.

The points at which this divergence occurs correspond with the experiments numbered 6 and 59, which are immediately above those marked unsteady.

Thus the change in the law of pressure agrees with the observation of unsteadiness in fixing the critical velocities.

DIAGRAM 1.

CURVES OF PRESSURE AND VELOCITY IN PIPES.



According to my assumption, the straightness of the curves between the origin and the critical points would depend on the constancy of temperature, and it was the small divergences observed that suggested a variation of temperature which had been overlooked. This variation was confirmed by further experiments, amongst which are those contained in Table IV. These showed that the probable variation of the temperature was in Table III. from 12° C. to 9° C. at the critical point, and from 12° C. to 8° C. in Table V., which variations would account for the small deviation from the straight.

It only remained, then, to ascertain how far the actual values of v_c, the velocity at the critical points, corresponded with the ratio $\frac{\mu}{D}$ or $\frac{P}{D}$.

For tube 4 from the Table III.

$$D = 0{\cdot}00615 \text{ metres,}$$

$$v_c = 0{\cdot}4426 \text{ metres per second at } 9^\circ \text{ C.,}$$

at this temperature $P = {\cdot}757$ (see p. 73).

Hence putting
$$B_c = \frac{P}{v_c D},$$

we have
$$B_c = 279{\cdot}7.$$

Again, for tube 5, Table V., at 8° C.

$$D = {\cdot}0127,$$

$$v_c = {\cdot}2260,$$

$$P = {\cdot}7796,$$

whence
$$B_c = 272{\cdot}0.$$

The differences in the values of B_c thus obtained, would be accounted for by a variation of a quarter of a degree in temperature, and hence the results are well within the accuracy of the experiments.

To each critical velocity, of course, there corresponds a critical value of the pressure. These are determined as follows.

The theoretical law of resistance for steady motion may be expressed by

$$A_c D^3 i = B_c P v.$$

And multiplying both sides by $\frac{D}{P^2}$,

$$\frac{A_c D^3 i}{P^2} = B_c \frac{D}{P} v.$$

This law holds up to the critical velocity, and then the right-hand number is unity, and, if B_c has the values just determined:

$$A_c = \frac{P^2}{D^3 i_c},$$

by Table III.

$$i_c = \cdot 0516,$$

$$P^2 = \cdot 573,$$

$$D^3 = \cdot 000{,}000{,}232,$$

which give $$A_c = 47{,}750{,}000.$$

By Table V.

$$i_c = \cdot 00638,$$

$$P^2 = \cdot 607,$$

$$D^3 = \cdot 00000205,$$

which give $$A_c = 46{,}460{,}000,$$

which values of A_c differ by less than by what would be caused by half a degree of temperature.

The conclusion, therefore, that the critical velocity would vary as $\frac{\mu}{D}$ is abundantly verified.

34. *Comparison with the discharges calculated by Poiseuille's formula.*—Poiseuille experimented on capillary tubes of glass between ·02 and ·1 millim. in diameter, and it is a matter of no small interest to find that the formula of discharges which he obtained from these experiments is numerically exact for the bright metal tubes 100 times as large.

Poiseuille's formula is—

$$Q = 1836{\cdot}724\,(1 + 0{\cdot}0336793\,T + 0{\cdot}000220992\,T^2)\,\frac{HD^4}{L},$$

T = temperature in degrees centigrade.

H = pressure in millims. mercury.

D = diameter in millims.

L = length in millims.

Q = discharge in millims. cubed.

Putting $$i = \frac{13{\cdot}64H}{L},$$

$$P = (1 + 0{\cdot}336793\,T + 0{\cdot}000220992\,T^2)^{-1},$$

$$v = \frac{4Q}{\pi D^2},$$

and changing the units to metres and cubic metres this formula may be written

$$47700000 \frac{D^3}{P^3} i = 278 \frac{D}{P} v,$$

the coefficients corresponding to A_c and B_c.

The agreement of this formula with the experimental results from tubes 4 and 5 is at once evident. The actual and calculated discharges differ by less than 2 per cent., a difference which would be more than accounted for by an error of half a degree in the temperature.

35. *Beyond the critical point.*—The tables show that, beyond the critical point, the relation between i and v differs greatly from that of a constant ratio; but what the exact relation is, and how far it corresponds in the two tubes, is not to be directly seen from the tables.

In the curves (diagram I. page 90) which result from plotting i and v, it appears that after a period of flatness the curves round off into a parabolic form; but whether they are exact parabolæ, or how far the two curves are similar with different parameters, is difficult to ascertain by any actual comparison of the curves themselves, which, if plotted to a scale which will render the small differences of pressure visible, must extend 10 feet at least.

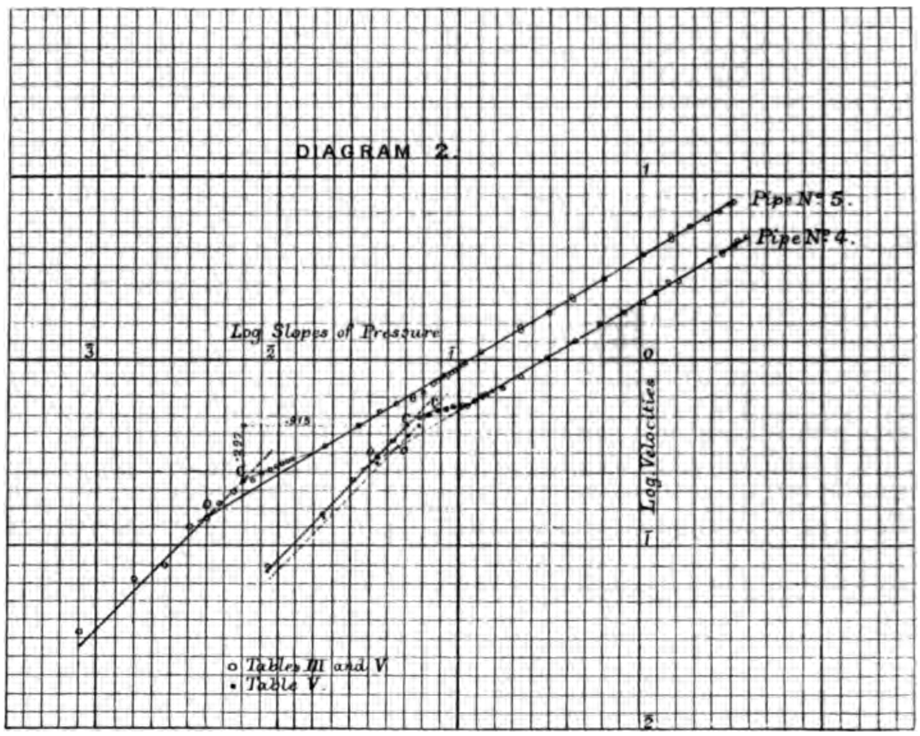
36. *The logarithmic method.*—So far the comparison of the results has been effected by the natural numbers, but a far more general and clearer comparison is effected by treating the logarithms of l and v.

This method of treating such experimental results was introduced in my paper on *Thermal Transpiration*, page 283, Vol. I.

Instead of curves, of which i and v are the abscissæ and ordinates, $\log i$ and $\log v$ are taken for the abscissæ and ordinates, and the curve so obtained is the logarithmic homologue of the natural curve.

The advantage of the logarithmic homologues is that the *shape* of the curve is made independent of any constant parameters, such parameters affecting the position of all points on the logarithmic homologue similarly. Any similarities in shape in the natural curves become identities in shape in the logarithmic homologues. How admirably adapted these logarithmic homologues are for the purpose in hand is at once seen from diagram II., which contains the logarithmic homologues of the curves for both pipes 4 and 5.

A glance shows the similarity of these curves, and also their general character. But it is by tracing one of the curves, and shifting the paper rectangularly until the traced curve is superimposed on the other, that the exact similarity is brought out. It appears that, without turning the paper at all, the two curves almost absolutely fit.



It also appears that the horizontal and vertical components of the shift are—

Horizontal shift .... ·913

Vertical shift .... ·294

which are, within the accuracy of the work, respectively identical with the differences of the logarithms of $\frac{D^3}{P^2}$ and $\frac{D}{P}$ for the two tubes.

37. *The general law of resistance in pipes.*—The agreement of the logarithmic homologues shows that not only at the critical velocities, but for all velocities in these two pipes, pressure which renders $(D^3/\mu^2)\,i$ the same in both pipes corresponds to velocities which render $(D/\mu)\,v$ the same in both pipes. This may be expressed in several ways. Thus if the tabular value

of i for each pipe, plotted in a scale, be multiplied by a number proportional to D^3/P^2 for that particular pipe, and the values of v by a number proportional to D/P, then the curves which have these reduced values of i and v for abscissæ and ordinates will be identical.

A still more general expression is that if

$$i = F(v)$$

expresses the relation between i and v for a pipe in which $D=1$, $T=0$, $P=1$,

$$\frac{D^3 i}{P^2} = F\left(\frac{Dv}{P}\right)$$

expresses the relation for every pipe and every condition of the water.

The determination of the relation between circumstances of motion and the physical condition of the water in such a general form was not contemplated when the experiments were undertaken, and must be considered as a result of the method of logarithmic homologues, which brought out the relation in such a marked manner that it could not be overlooked. Nor is this all.

It had formed no part of my original intention to re-investigate the law of resistance in pipes for velocities above the critical value, as this is ground which had been very much experimented upon, and experiments seemed to show that the law was either indefinite or very complex—a conclusion which did not seem inconsistent with the supposition that above this point the resistance depended upon eddies which might be somewhat uncertain in their action. But although it was not my intention to investigate laws, I had made a point of continuing the experiments through a range of pressures and velocities very much greater I think than had ever been attempted in the same pipe.

Thus it will be noticed that in the larger tube the pressure in the last experiment is four thousand times as large as in the first. In choosing the great range of pressures I wished to bring out what previous experiments had led me to expect, namely, that in the same tube for sufficiently small pressures the pressure is proportional to the velocity, and for sufficiently great pressures, the pressure was proportional to the square of the velocity. Had this been the case not only would the lowest portion of the logarithmic homologues up to the critical point have come out straight lines inclined at 45 degrees, but the final portion of the curve would have come out a straight line at half this inclination; or with a slope of two horizontal to one vertical.

The near approach of the lower portions of the curve to the line at 45° led me, as I have already explained, to discover that the temperatures had risen at the lower velocities, and to make a fresh set of experiments, some of which are given in Table IV., in which, although the temperatures were not constant, they were sufficiently different from the previous ones to show that the discrepancy in the lower portions of the curves might be attributed to variations of temperature, and the agreement with the line of 45° considered as within the limits of accuracy of experiment.

When the logarithms of the upper portions of the curve came to be plotted, the straightness and parallelism of the two lines was very striking.

There are a few discrepancies which could not be in any way attributed to temperature, as with so much water moving this was very constant, but on examination it was seen that these discrepancies marked the changes of the discharge gauges. The law of flow through the orifices not having been strictly as the square roots of the heights, the manner in which the gauges had been compared forbade the possibility of there being a general error from this cause; the middle readings on the gauge were correct, so that the discrepancies, which are small, are mere local errors.

This left it clear that whatever might be their inclination the lines expressed the laws of pressures and velocities in both tubes, and since the lines are strictly parallel, this law was independent of the diameter of the tube. This point has been very carefully examined, for it is found that the inclination of these lines differs decidedly from that of 2 to 1, being 1·723 to 1, and so giving a law of pressures through a range 1 to 50 of

$$i \propto v^{1 \cdot 723}.$$

This is different from the law propounded by any of the previous experimenters, who have adhered to the laws

$$i = v^2,$$

or

$$i = Av + Bv^2.$$

That neither of these laws would answer in case of the present experiments was definitely shown, for the first of these would have a logarithmic homologue inclined at 2 to 1, and the second would have a curved line. A straight logarithmic homologue inclined at a slope 1·723 to 1 means no other law than

$$i \propto v^{1 \cdot 723}.$$

I have therefore been at some pains to express the law deduced from my experiments on the uniform pipes so that it may be convenient for application. This law as already expressed is simply

$$\frac{D^3}{P^2} i = f\left(\frac{Dv}{P}\right),$$

where f is such that

$$x = f(y)$$

is the equation to the curve which would result from plotting the resistance and velocities in a pipe of diameter 1 at a temperature zero.

The exact form of f is complex, this complexity is however confined to the region immediately after the critical point is passed.

Up to the critical point

$$A_c \frac{D^3}{P^2} i = B_c \frac{Dv}{P}.$$

After the critical point is passed the law is complex until a velocity which is $1{\cdot}325 v_c$ is reached. Then as shown in the homologues the curve assumes a simple character again,

$$A \frac{D^3}{P^2} i = \left(B \frac{Dv}{P}\right)^{1{\cdot}723}$$

that is, the logarithmic homologue becomes a straight line inclined at 1·723 to 1.

Referring to the logarithmic homologues (diagram 2, page 94), it will be seen that although the directions of the two straight extremities of the curve do not meet in the critical point, their intersection is at a constant distance from this point, which in the logarithmic curves is, both for ordinates and abscissæ,

$$0{\cdot}154.$$

These points o are therefore given by

$$\log \frac{D^3 i_c}{P^2} = \log \frac{D^3 i_o}{P^2} + 0{\cdot}154$$

$$\log \frac{Dv_c}{P} = \log \frac{Dv_o}{P} + 0{\cdot}154.$$

Therefore putting

$$A = \frac{P^2}{D^3 i_o}, \quad B = \frac{P}{Dv_o}$$

$$\log A = \log A_c + 0{\cdot}154$$

$$\log B = \log B_c + 0{\cdot}154$$

and by the values of A_c and B_c previously ascertained (Art. 33, p. 92),

$$\log A = 7{\cdot}8311, \quad A = 67{,}700{,}000$$

$$\log B = 2{\cdot}598, \quad B = 396{\cdot}3$$

For feet

$$\log A = 6{\cdot}28414, \quad A = 1{,}935{,}000$$

$$\log B = 1{\cdot}56603, \quad B = 36{\cdot}9.$$

We thus have for the equation to the curves corresponding to the upper straight branches

$$A\frac{D^3}{P^2}i = \left(B\frac{Dv}{P}\right)^{1\cdot722}.$$

And if n have the value 1 or 1·722 according as either member of this equation is < or > 1 the equation

$$A\frac{D^3}{P^2}i = \left(\frac{BDv}{P}\right)^n$$

is the equation to a curve which has for its logarithmic homologue the two straight branches intersecting in o, and hence gives the law of pressures and velocities, except those relating to velocities in the neighbourhood of the critical point, and these are seldom come across in practice.

By expressing n as a discontinuous function of $B_c\frac{Dv}{P}$ the equation may be made to fit the curve throughout.

38. *The effect of temperature.*—It should be noticed that although the range is comparatively small, still the displacement of the critical point in Tables III. and IV. is distinctly marked. The temperatures were respectively 9° C., 5° C.

$$\text{At } 9^\circ \ \log P^{-1} = 0\cdot12093$$

$$\text{At } 5^\circ \ \log P^{-1} = 0\cdot06963$$

$$\text{Difference} \quad = \ \cdot05130$$

This should be the differences in the values of $\log v_c$ in Tables III. and IV. The actual difference is ·062. Also the differences in $\log i_c$ should be the differences in P^2 or ·10260, whereas the actual difference is ·121.

The errors correspond to a difference of about 1° C., which is a very probable error.

It would be desirable to make experiments at higher temperature, but there were great difficulties about this which caused me, at all events for the time, to defer the attempt.

SECTION IV.

Application to DARCY'S *experiments.*

39. DARCY'S *experiments.* The law of resistance came out so definitely from my experiments that, although beyond my original intention, I felt constrained to examine such evidence as could be obtained of the actual experimental results obtained by previous experimenters.

The lower velocities, up to the critical value, were found, as has already been shown (Art. 35), to agree exactly with Poiseuille's formula.

For velocities above the critical values the most important experiments were those of Darcy--approved by the Academy of Sciences and published 1845—on which the formula in general use has been founded. Notwithstanding that the formula as propounded by Darcy himself could not by any possibility fit the results which I have obtained, it seemed possible that the experiments on which he had based his law might fit my law. A comparison was therefore undertaken.

This was comparatively easy, as Darcy's experimental results have been published in detail.

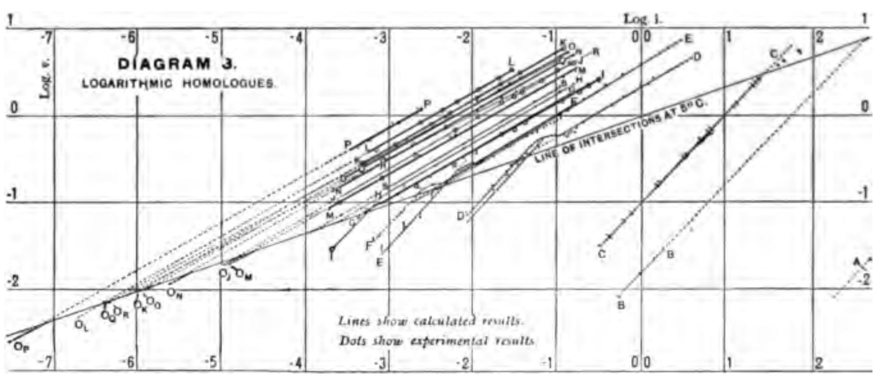
Altogether he experimented on some 22 pipes, varying in diameter from about the size of my largest, $0^{m}\cdot0014$ up to $0^{m}\cdot5$. They were treated in several sets, according to the material of which they were composed—wrought-iron gas-pipes, lead pipes, varnished iron pipes, glass pipes, new cast-iron and old rusty pipes.

The method of experimenting did not differ from mine except in scale, the distance between Darcy's gauge points being 50^{m} instead of 5 feet in my case. The great length between Darcy's gauge points entailed his having joints in his pipes between these points, and the nature of his pipes was such as to preclude the possibility of a very uniform diameter. His experiments appear to have been made with extreme care and very faithfully recorded, but the irregularity in the diameters, which appears to have been as much as 10 per cent., and the further irregularity of the joints, preclude the possibility of the results of his experiments following very closely the law for uniform pipes. Another important matter to which Darcy appears to have paid but little attention was temperature. It is true that in many instances he has given the temperature, but he does not appear to have taken any account of it in his discussion of his results, although it varied as much as 20° C. in the cases where he has given it, and as his pipes, 300 metres long, were in the open air, the effect of the sun on the pipes would have led to still larger differences.

The effect of these various causes on his results may be seen, as he took the precaution to use two pressure gauges on separate lengths of 50^m of his pipes, and the records from these two gauges by no means always agree, particularly for the lower velocities. In one case the results are as wide apart as 15 to 7, and often 10 or 15 per cent. In arriving at tabular values for i he has taken the mean of the two gauges.

Taking these things into account, I could not possibly expect any close agreement with my results; still, as experiments on pipes of such large diameters are not likely to be repeated, at any rate with anything like the same care and success, they offered the only chance of proving that my law was general.

40. *Reduction of the experimental results.* Rejecting all the experiments on rusty and rough pipes, *i.e.*, selecting the lead, the varnished, the glass, and new cast-iron pipes, which ranged from half-an-inch to twenty inches diameter, I had the logarithmic homologues drawn. These are shown on diagram 3. In the case of two of the smaller pipes the



	Diameter	Temp.	Surface	
A	0 mm. 014	10° C.	Glass	Poiseuille
B	0 270	,,	,,	Poiseuille
C	0 650	,,	,,	Poiseuille
D	6 15	5	Lead No. 4	
E	12 70	5	,, No. 5	
F	14 00	×	,,	Darcy
G	27 00	×	,,	Darcy
H	41 00	×	,,	Darcy
I	26 00	12° C.	Varnished	Darcy
J	82 60	21° C.	,,	Darcy

	Diameter	Temp.	Surface	
K	196 mm. 00	×	Varnished	Darcy
L	285 00	21° C.	,,	Darcy
M	81 90	15° C.	Cast Iron new	Darcy
N	137 00	15°	,, ,, ,,	Darcy
O	188 00	×	,, ,, ,,	Darcy
P	500 00	×	,, ,, ,,	Darcy
Q	243 20	×	C. I. incrusted	Darcy
R	244 70	×	ib. cleaned	Darcy
S	49 68	×	Glass	Darcy

smallest velocity is well below the critical point, and in several of the other pipes the smallest velocity is near the critical velocity. This accounts for the lower ends of the logarithmic curves being somewhat twisted; for the remainder of the logarithmic homologues are nearly straight; some are slightly bent one way and some another, but they are none of them more bent than may be attributed to experimental inaccuracy.

The inclinations of the upper ends of the lead and bituminous pipes is 1·746, slightly greater than mine; but in the cases of the glass pipes and the cast-iron pipes the slopes are 1·82 and 1·92 respectively.

So much appeared from the logarithmic homologues themselves, but the most important question was, would the curves agree with the results calculated from the formula

$$A\frac{D^3}{P^2}i = \left(B\frac{D}{P}v\right)^n ?$$

41. *Comparison with the law of resistance.* In applying this test I was at first somewhat at a loss on account in some cases of the want of any record of the temperature, and the doubt as to such temperatures as had been recorded being the temperature of the water in the pipes between the gauges.

The dates at which the experiments were made to a certain extent supplied the deficiency of temperature, the temperatures given fixing the law of temperature, so that the probable temperature could be assumed where it was not given.

Assuming the temperature, the values of

$$i_0 = \frac{P^2}{AD^3},$$

$$v_0 = \frac{P}{BD}.$$

were calculated for each tube, using the values of A and B as already determined, $\log i_0$ and v_0 are the co-ordinates of O the intersection of the two straight branches of the logarithmic curves, so that the application of the formula to the results was simply tested by continuing the straight upper branches of the logarithmic homologues to see whether they passed through the corresponding point O.

The agreement, which is shown in diagram 3, page 100, is remarkable. There are some discrepancies, but nothing which may not be explained by inaccuracies, particularly inaccuracies of temperature.

42. *The effect of the temperature above the critical point.*—It is a fact of striking significance, physical as well as practical, that while the temperature of the fluid has such an effect at the lower velocities that, *cæteris paribus*, the discharge will be double at 45° C. what it is at 5° C., so little is the effect at the higher velocities that neither Darcy nor any other experimenter seems to have perceived any effect at all.

In my experiments the temperature was constant, 5° C. at the higher velocities, so that I had no cause to raise this point till I came to Darcy's result, and then, after perplexing myself considerably to make out what the temperatures were, I noticed the effect of the temperature is to shift the curves 2 horizontal to 1 vertical, which corresponds with a slope of 2 to 1, and so nearly corresponds with the direction of the curves at higher velocities that variations of 5° or 10° C. produce no sensible effect; or, in other words, the law of resistance at the higher velocity is sensibly independent of the temperature, *i.e.*, of the viscosity.

Thus not only does the critical velocity at which eddies come in, diminish with the viscosity, but the resistance after the eddies are established is nearly, if not quite, independent of the viscosity.

43. *The inclinations of the logarithmic curves.*—Although the general agreement of the logarithmic homologues completely establishes the relations between the diameters of the pipes, the pressures, and velocities, for each of the four classes of pipes tried, viz., the lead, the varnished pipes, the glass pipes, and the cast-iron, there are certain differences in the laws connecting the pressures and velocity in the pipes of different material. In the logarithmic curves this is very clearly shown as a slight but definite difference between the inclination of the logarithmic homologues for the higher velocities.

The variety of the pipes tried reduces the possible causes of this difference to a small compass. It cannot be due to any difference in diameters, as at least three pipes of widely different diameters belong to each slope. It is not due to temperature. This reduces the cause for the different values of n to the irregularity in the pipes owing to joints and other causes, and the nature of the surfaces.

The effect of the joints on the values of n seems to be proved by the fact that Darcy's three lead pipes gave slightly different values for n, while my two pipes without joints gave exactly the same value, which is slightly less than that obtained from Darcy's experiments.

Darcy's pipes were all of them uneven between the gauge points, the glass and the iron varying as much as 20 per cent. in section. The lead were by far the most uniform, so that it is not impossible that the differences in the values of n may be due to this unevenness.

But the number of joints and unevenness of the tarred pipes corresponded very nearly with the new cast-iron, and between these there is a very decided difference in the value of n. This must be attributed to the roughness of the cast-iron surface.

44. *Description of Diagram* 3.

Diagram 3.—In this diagram the experiments of Poiseuille and Darcy are brought into comparison with those of the present investigation.

In consequence of the number of lines, the general aspect of the diagram is somewhat confused, but such confusion vanishes so soon as it is clearly perceived that each line of dots indicates the logarithmic homologue for some particular pipe as determined by experiment, reduced and plotted in exactly the same manner as for diagram 2, page 94; *DD* and *EE* being exact repetitions of the logarithmic homologue for pipes 4 and 5, on a somewhat smaller scale.

It is at once apparent from diagram 3 how, for the most part, the experiments have been well below or well above the critical values. In the small pipes of Poiseuille the velocities were below the critical values, and hence lie in straight lines inclined at 45°.

The smallest pipe on which Poiseuille experimented had a diameter of 0·014 millim.; only one experiment, marked *A*, is shown in the diagram, as the remaining three extended outside the range of the plate. They fall exactly on the dotted line through *A*, and do not reach the critical value.

The same is true of all the rest of Poiseuille's experiments, except those made on a much larger pipe, diameter 0·65 millim., hence it is thought sufficient to plot only one, namely *BB*.

CC shows the experimental results obtained with the pipe 0·65 millim. diameter, and these reach the critical value as given by the formula, and then diverge from the line.

It is important to notice, however, that the points are not taken directly from Poiseuille's experiments, which have been subjected to a correction rendered necessary by the fact that Poiseuille did not measure the resistance by ascertaining the pressure at two points in the pipe, but by ascertaining the pressure in the vessels from which and into which the water flowed through the pipe, so that his resistance includes, besides the resistance of the pipe, the pressure necessary to impart the initial velocity to the water. This fact, which appears to have been entirely overlooked, had a very important influence on many of Poiseuille's results. Poiseuille endeavoured to ascertain what was the limit to the application of his law, and, with the exception of his smallest tubes, succeeded in attaining velocities at which the results were no longer in accordance with his law.

When I first examined his experiments I expected to find these limiting velocities above the critical velocities as given by my formula. In all cases, however, they were very much below, and it was then I came to see that Poiseuille had taken no account of the pressure necessary to start the fluid.

It then became interesting to see how far the deviations were to be explained in this way.

In pipes of sensible size the pressure necessary to start the fluid lies between

$$\frac{v^2}{2g} \text{ and } 1{\cdot}505\frac{v^2}{2g},$$

according to whether the mouthpiece is trumpet-shaped or cylindrical. Poiseuille states that he was careful to keep both ends of his pipe cylindrical, hence according to the law mouthpieces of sensible size, the pressures which he gives should be corrected by $1{\cdot}505\,\frac{v^2}{2g}$.

This correction was made, and it was then found that with all the smaller tubes Poiseuille's law held throughout his experiments, and with the larger pipe it held up to the critical value and then diverged in exact accordance with my formula, as shown by the line CC.

Darcy's experiments in the case of three tubes F, G, I, fall below the critical value, and in all these cases agree very well with the theoretical curve as regards both branches.

This, however, must be looked upon as accidental, as at the lower velocities Darcy had clearly reached the limit of sensitiveness of his pressure gauges; thus, for instance, the experiment close by the letter F is the mean of two readings which are respectively 7 and 15; there is a tendency throughout the entire experiments to irregularity in the lower readings, which may be attributed to the same cause, and this seems to explain the somewhat common deviation of the one or two lower experiments from the line given by the middle dots.

A somewhat similar cause will explain cases of deviation in the one or two upper experiments, for the discrepancy in the two gauges here again becomes considerable.

For these reasons the intermediate experiments were chiefly considered in determining the slopes of the theoretical lines.

These slopes were obtained as the mean of each class of tubes:—

Lead jointed	1·79
Varnished	1·82
Glass	1·79
New cast-iron	1·88
Incrusted pipe	2·
Cleaned pipe	1·91

and then in the cases in which the temperature was given, I, J, L, M, N, the points O having been determined by the formulæ:

$$\text{Log}\ i_0 = 2 \log P - 3 \log D - 7{\cdot}851$$

$$\text{Log}\ v_0 = \ \log P - \ \log D - 2{\cdot}598$$

the lines having the respective slopes were drawn through these points, and in all cases agreed closely with the experiments.

In the cases where the temperature was not given, the values of $\log i_0$ and $\log v_0$ were calculated for 5° C., these are shown along the line marked "line of intersections at 5°," through these points lines are shown drawn at an inclination of 2 to 1, which are the lines on which O would lie whatever might be the temperature. These with the respective slope lines were drawn so as most nearly to agree with the experiments, these intersect the lines at 2 to 1 in the points O which indicate the temperatures, and considering the extremely small effect of the temperature these are all very probable temperatures with the exception of G, H, and S, in which cases O is above the line for 5° C. This indicates strongly that in these cases there must have been a small error, 2 or 3 per cent., in determining the effective diameter of the pipes.

It seemed very probable that roughness in the pipes, such as might arise from incrustation or badly formed joints, would affect the logarithmic homologues, and for this reason only the smoother classes of pipes were treated; but with a view to test this idea, the homologues Q and R, which related to the same incrusted pipe before and after being cleaned were drawn, and their agreement is such as to show that for such pipe the effect of incrustation is confined to the effect on the diameter of the pipe, and on the value of n which it raises to 2. This, however, was a large pipe, and the velocities a long way above the critical velocity, so that it is quite possible that the same incrustation in a smaller pipe would have produced a somewhat different effect.

The general result of this diagram is to show that throughout the entire range—from pipes of $0{\cdot}^{\text{m}}000014$ to $0{\cdot}^{\text{m}}5$ in diameter, and from slopes of pressure ranging from 1 to 700,000—there is not a difference of more than 10 per cent. in the experimental and calculated velocities, and, with very few exceptions, the agreement is within 2 or 3 per cent., and it does not appear that there is any systematic deviation whatever.

45.

THE TRANSMISSION OF ENERGY.

"Cantor Lectures delivered before the Society of Arts in 1883."

I.

(*Delivered* April 23, 1883.)

SOME few days ago, during a conversation with a friend, I remarked that I was going to give some lectures at the Society of Arts upon the transmission of energy, whereupon my friend inquired, "Is that the transmission of energy by electricity?" To this I replied, "No." The fact is that we have heard so much about electricity that I began to think it was time to recall attention to the fact that there are other means of performing mechanical operations.

I am not sure whether, during the various lectures which have been given in this room on electricity, the actual term, transmission of energy, has been used. But whether it has or not, some of the leading ideas connected with it have been before you.

I think it may be said that the great interest which the public has manifested in the recent advance in the arts relating to electricity has arisen, in a large measure, from the cry of joy with which Faure's battery was received. A cry which said, in so many words, "Here we have at last a means of utilising our waterfalls and natural sources of power in a way that may relieve us of all the anxiety about our coal-fields." To those who had studied the subject it was evident at the time that this cry was premature. And to some of us, at all events, it seems to be a mistake to encourage false hopes, or, rather, knowingly to base hopes on a false foundation, to hold out as a means of replacing our coal what was, in all probability, only another means of increasing its rate of consumption, for every step in art which facilitates the application of power must increase the demand on the acting sources.

But this is not all; the exaggerated claim set up for electricity, diverted, for a time at all events, attention from the true claim, which would have been sufficient in itself had it not thus been put out of sight. It is not our object at present to save our coal, but to turn it to the best advantage, to get the greatest result we can, and if Faure's battery or any subsequent advance in this direction conduces to this, it is no small matter. Now, during the last ten or fifteen years an entirely new aspect has been given to mechanics by the general recognition of the physical entity which we call energy, in different forms.

We recognise the one thing under different forms in the raised hammer, the bent spring, the compressed air, the moving shot, the charged jar, the hot water in the boiler, and the separate existence of coal, corn, or metals, and oxygen. We see in the revolution of the shafts and the travel of belts in our mills, the passage of water, steam, and air along pipes, the conveyance of coal, corn, and metals, and the electric currents, the transmission of this same thing—energy—from one place to another; and in all mechanical actions we perceive but the change of form of the same thing.

Taking this general or energy point of view we may get rid of all the complication arising from special purpose, and recognise nothing but the form of energy in its source, the distance it has to be transmitted, and the special form that must be given to it for its application. And this view, although not the best in which to study the special purpose of mechanics or contrivances, is of great importance, inasmuch as it has revealed many general laws, and many fundamental limits to the possibilities of extension in certain directions.

My object in these lectures is to direct your attention to some of the leading mechanical facts and limits revealed by this view.

There is one general remark I would wish to make, by way of caution. I hope nothing I may say will be interpreted by any of my hearers into a prediction as to what may happen in the future. I have to deal with facts, and I shall try to deal with nothing but facts. Many of these facts, or the conclusions to be immediately drawn from them, may appear to bear on the possibilities—or, rather, the impossibilities—of art. But in the Society of Arts I need not point out that art knows no limit; where one way is found to be closed, it is the function of art to find another. Science teaches us the results that will follow from a known condition of things; but there is always the unknown condition, the future effect of which no science can predict. You must have heard of the statement in 1837, that a steam voyage across the Atlantic was a physical impossibility, which was said to have been made by Dr Lardner. What Dr Lardner really stated, according to his own showing, was that such a voyage exceeded the then present limits of steam-power. In this he was within the mark, as anyone would be if he were to say now that conversation between England and America exceeded the limit of the power of the telephone. But to use such an argument against a proposed enterprise, is to ignore the development of art to which such an enterprise may lead.

I wish to do nothing of this kind, and if, in following my subject, I have to point out circumstances which limit the possibilities of present art, and even seek to define the limits thus imposed, it is in the hope of concentrating the efforts of art into what may be possible directions, by pointing out the whereabouts of such barriers as science shows to be impassable.

Although the terms energy and power are in continual, we might almost say familiar, use, such use is seldom in strict accordance with their scientific meaning. In many ways the conception of energy has been rendered popular, but a clear idea of the relation of energy to power is difficult. This arises from the extreme generality of the terms; in any particular case the distinction is easy. I was going to say that it is easiest to express this distinction by an analogy, but, as a matter of fact, everything that seems analogous is really an instance of energy. Power may be considered to be directed energy; and we may liken many forms of energy to an excited mob, while the directed forms are likened to a disciplined army. Energy in the form of heat is in the mob form; while energy in the form of a bent spring, or a raised weight, matter moving in one direction, or of electricity, is in the army form. In the one case we can bring the whole effect to bear in any direction, while in the other case we can only bring a certain portion to bear, depending on its concentration. Out of energy in the mob form we may extract a certain portion, depending on its intensity and surrounding circumstances, and it is only this portion which is available for mechanical operations.

Now energy in what we may call its natural sources has both these forms. All heat is in the mob form, hence all the energy of chemical separation, which can only be developed by combustion, is in the mob form; and this includes the energy stored in the medium of coal. The combustion of 1 lb. of coal yields from ten to twelve million foot-pounds of energy in the mob form of heat; under no circumstances existing at present can all this be directed, nor have we a right, as is often done, to call this the power of coal. What the exact possible power is we do not know, but probably about four-fifths of this, that is to say, from eight to ten million foot-pounds of energy per pound of coal is the extreme limit it can yield under the present conditions of temperature at the earth's surface. But before this energy becomes power, it must be directed. This direction is at present performed by the steam-engine, which is the best instrument art has yet devised, but the efficiency of which is limited by the fact that before the very intense mob energy of the fire is at all directed, it has to be allowed to pass into the less intense mob energy of hot water or steam. The relative intensities of these energies are something like twenty-five to nine. The very first operation of the steam-engine is to diminish the directable portion of the energy of the pound of coal from nine million to three millions. In addition to this there are necessary wastes of directable energy, and a considerable expenditure of already directed energy in the necessary mechanical operations. The result is that, as the limit, in the very highest class engines the pound of coal yields about one and a-half millions of foot-pounds; in what are called "first-class engines," such as the compound engines on steamboats, the pound of coal yields one million, and in the majority of engines, about five or six hundred thousand foot-pounds. These quantities have been largely increased during the last few years; as far as science can predict, they are open to a further increase. In the steam-engine art is limited to its three million foot-pounds per pound of coal; but gas-engines have already made a new departure, and there seems no reason why art should stop short of a large portion of the nine millions.

Other important natural sources of mechanical powers are energy in an already directed or army form, wind and water power. Here the power needs no development, but merely transmission and adaptation, and hence it has one important advantage over the energy of chemical separation. These have both been, and are, good servants to man. But there appears to be what are greater drawbacks—in the irregularity of these forces as regards time, and the distribution as regards space.

The application of the power of the wind to the propulsion of ships has, doubtless, influenced the economy of the world more than any other mechanical feat; and, not very long ago, water-power played no relatively unimportant part of the work of the world. But it would seem that both these have had their day, and are now relegated to work of a secondary kind. Some further development of art might however bring them to a foremost place again, by developing their use to a hitherto unprecedented extent. Hitherto both wind and water have only had a local application—that is to say, they were used where and when they were wanted. Wind was only used in the sailing of ships on voyages, and for mills, distributed so as to be within range of such corn as was too far from water; while water-power, though very valuable to a certain limited extent, when near habitable country, was otherwise allowed to run to waste; and these wastes included by far the larger sources of this power—the larger rivers and waterfalls, the tidal estuaries, and last, but not least, the waves of the sea, a source which has never been utilised for good. A modern idea is, that it needs nothing but a possible development of art to render these larger sources not only available for power in their immediate neighbourhood, but available to supply power wherever it is wanted, and so displace the coal, or replace the power as coal becomes exhausted. The desirability of such a result fully explains the entertainment of the pleasant idea; but, unfortunately, when we come to look closer into the question, the probability of its accomplishment diminishes rapidly. Many of the considerations of which I shall have to speak bear directly on this question; so that I shall now defer its further consideration, merely pointing out that, to accomplish this result, the power must not only be extracted from the water on the spot and at the same time, but it must be transmitted over hundreds or thousands of miles, and must be stored till it is wanted.

It may well be thought that energy in a directed form, or in the army form, may be better transmitted than in the undirected or mob form. As a matter of fact, however, energy has never been and never can be transmitted as mechanical power in large quantities, over more than trifling distances, say, as a limit, twenty or thirty miles. I say never can, because such transmission depends on the strength of material; and unless there is some other material on the earth of which we know nothing, we know the limit of this. This is a part of my subject into which I shall enter more closely in my second and third lectures.

In deprecating the idea that wind and water will ever largely supply the place of coal, I do not for a moment wish it to be thought that I take a gloomy view of the mechanical future of the earth. This, I believe, admits of immense development, and will not for long depend, as it does at present, on the adjacency of coal-fields. This will be explained as I proceed.

It must not be forgotten that, after all, the most important source of energy is not coal, but corn and vegetable matter. The power developed in the labour of animals exceeds the power derived from all other sources, including coal, in the ratio of, probably, 20 or 30 to 1; so that, after all, if we could find the means of employing such power for the purposes for which coal is specially employed—such as driving our ships, and working our locomotives—an increase of 10 per cent. in the agricultural yield of the earth would supply the place of all the coal burnt in engines. The energy which may be derived from the oxidisation of corn has as yet only been artificially developed in the form of heat, and this may be the only possible way; but physiology has not yet advanced to the point of explaining the physical process of the development of energy consequent on the oxidisation of the blood; and it is at all events an open question whether the energy of corn may not be really a form of directed energy, in which case corn would yield six or eight times as much energy as coal does at present, consumed in our engines. As consumed in animals, it yields a larger proportion of energy—two or three times as much, and may be more—whereas by burning it in steam-engines, we cannot get half as much. Should we find an artificial means of developing anything like the full directable power of corn—a problem which has not yet been attempted—coal would no longer be necessary for power. I do not mention this as a prediction, but as showing that there are, besides wind and water, other, and as yet untried, directions from which mechanical energy may be derived in the future.

Electricity is not a natural source of energy, for the simple reason that the metals have mostly been burnt or oxidised during the past history of the earth. But still it is important, at this stage of my lecture, to point out that the energy consequent on the separate existence of metals and oxygen can be developed without combustion, in a totally directed form, *i.e.*, totally available for power.

There are many peculiarities which distinguish the group of elementary substances we call metals, but there is no more distinctive feature than this. This is not a primary source of power, but, as it at present appears, it promises to become the most important secondary source. We cannot find metals existing in a separate form but by the use of power; where and when it exists, we can separate them from the salts, and so store the energy in a form completely available for power. The economical questions relating to such storage of energy will be considered in their place later in the course.

It is not, however, only as effecting storage of power that electricity demands our attention, it also affords a means of transmitting power, which has long held an important place in art, and to which all eyes have been recently turned in expectation of something new and startling.

Before considering the developments of art, and the circumstances on which their further development depends, I shall turn, for a moment, to the processes of nature. The mechanics of the universe, no less than those relating to human art, depend on the transmission of energy. In nature energy is transmitted in all its forms and under all circumstances, both those which we can imitate in art, and those we can not.

The most important point with regard to the artificial transmission of energy is the proportion of power spent in effecting the transmission, and the circumstances on which this proportionate loss depends. Is such loss universal? So far as we know, it is attendant in a greater or less degree on all artificial means of transmission, and on all transmissions effected by nature on the surface of the earth. If it were not, this earth would be no place to live upon. No motion would ever cease. As it is, the winds and waters are rapidly brought to rest by the friction which they encounter. Currents of wind and currents of water form the principal means by which energy is transmitted over the surface of the earth. But there are other means which experience less resistance. Oscillatory waves, those of sound, are a very efficient means of transmitting energy. Sounds are not transmitted to an unlimited distance, chiefly because by the spreading of the wave the sound becomes weaker and weaker as it proceeds. It is also destroyed by the friction of the solid surface of the earth. Hence the sounds which reach us from bodies high up, as the explosion of a meteor, are heard much further than such sounds made at the surface of the earth, although there are two records of artillery having been heard two hundred miles. Owing to such incidental destruction of sound we cannot say from experience that sound waves in air are destroyed, but from the physical properties of gases we know they are.

Waves on the sea are another very efficient means of transmitting power, a means which may be called nature's mill. The waves which take up the energy or power from the wind in mid ocean travel onwards, carrying this energy, and experience such slight resistance that they will, after travelling hundreds or thousands of miles, destroy the shores on which they expend the last of their energy. If we could find a means of utilising the energy of waves, we should not only save our coal, but also save our country from the waves; still, water waves experience resistance which we can better estimate theoretically than practically.

These are the principal ways in which energy is transmitted from one part of the earth to another. There are others, such as earthquakes, but they all show the same thing, that power is spent in the transmission of energy.

If we look away into interstellar space, the case is altered. Here we see two ways in which energy is transmitted—heat, or light, and the motion of the heavenly bodies. In neither of these can we see any direct evidence of resistance or loss of power; and, as judged by any terrestrial measure, there certainly is none. The distance at which we see stars is a sufficient proof of the freedom with which a wave of light travels; while the regularity of the motion of the planetary bodies shows that they encounter no sensible resistance. Yet, although not directly perceivable, there are circumstances that strongly suggest that in both these forms, transmission of energy is resisted. If space is unlimited, and there are stars throughout it, why do not we see them at greater distances than we do? Under these circumstances there could be no spot in the heavens at which at a sufficiently great distance there was not a star, so that, if the light were not stopped, the whole heavens would be one fiery envelope as bright as the sun. This is a question which philosophers have not decided. But one, and the favourite, way out of the difficulty, is to suppose that the light does encounter resistance, even in interstellar space. This is a subject on which your Chairman of Council has boldly launched; and whether his hypothesis be right or wrong, it has brought to the front a very interesting subject.

With regard to the resistance encountered by the planetary bodies, our evidence is even slighter. A few domesticated comets seem to diminish their speed; and it is not so long since we were all on the *qui vive*, by the promise of the spectacle of an old friend, who seemed to have come earlier than he was expected, on purpose to verify a prediction of plunging into the sun, but instead of doing so he passed away and was pronounced a stranger, to the joy of the nervous, but somewhat to the discomfiture of astronomers.

The energy which we derive from the sun comes to us in the form of sunshine, in a highly directed but extremely scattered form, being uniformly distributed all over the illuminated disc of the earth. It reaches the outer atmosphere nearly in the same condition as it left the sun, having traversed ninety odd millions of miles without any sensible expenditure of power. In the twenty or thirty miles of the lower atmosphere, however, it encounters very great, but variable, resistance. Sometimes half of it, or three-quarters of it, may reach the earth's surface. This is rare in our country, and on the average not more than a very small fraction ever reaches the surface.

When the sun does shine, the sunshine is a form of energy which may be, and is, very largely directed so as to yield power. Any such direction which may be accomplished by human art is undertaken at an enormous disadvantage, on account of the scattered manner in which the energy reaches us. The sunshine must be collected before we can make any mechanical use of it.

In the abstract, there are two methods. The one would be to accumulate the energy of sunshine on a given place, over a long time. This is nature's method. The energy on each portion of the earth's surface, during days, weeks, the whole year, or many years, is accumulated by the growth of vegetables. Corresponding to this, however, art has as yet developed no means whatever. If we don't use the sunshine as it falls, energy is lost for all mechanical purposes. I say if we don't, not that we do use it, but because we can, and have done so in a small way. By means of a lens, or reflectors, the sunshine which falls on a certain place may be concentrated on to a smaller space, and so be sufficient to perform some mechanical operation. In this way small vapour engines have been worked by sunshine. But the cost of the apparatus necessary for such concentration is out of all proportion to the result accomplished, and shows the art difficulties must be got over by a new departure. There is the further consideration that sunshine on land is too valuable for the maintenance of vital energy to allow of its being devoted to mechanical purposes.

As regards the perfectness of nature's method, so far as I know, no attempts have even been made to test this. It is probably very wasteful, as are all nature's methods, but it is effective. In the first instance, the energy of sunshine is stored on the spot where it falls, in the tissues, but chiefly in the sap of the grass and vegetation. If this is not removed, a large portion of the energy of the year's growth, that which is in the sap, is stored in the seed, and the rest, although apparently again scattered on the decay of the tissues, is to some extent preserved in the ground, and either forwards the next year's crop, or takes the permanent form of peat; and our coal-fields are but evidence of the way in which the directable energy of sunshine has been stored under circumstances where there was no immediate purpose for which to apply it. Under present circumstances, however, this energy is almost everywhere too valuable to admit of secular storage.

It is either removed directly by nature's method, the teeth of animals, or allowed to accumulate for a longer period, and then removed by human industry. The further aggregation of this energy involves the transmission of energy in a mechanical sense, and hence involves the expenditure of power. Nature works by means of directly converting this energy into power. The plant accumulates the energy of sunshine, the animal collects and appropriates this energy. This collection is accomplished by the expenditure of power, which means a redistribution of that portion of the energy which is capable of direction. The scheme of nature, therefore, is a cycle. The vegetation accumulates the energy, as far as time is concerned, leaving it in a scattered form, requiring power to collect it; this power is in the grass, and only wants direction; this it receives in the animal, which again expends some of the energy in the operation of collecting. If vegetable energy be supplied to the animal in a collected form, then a large portion of the directed energy is available for mechanical purposes. And in this way we may form a rough estimate of the directed energy to be obtained from sunshine in this country. The common agricultural rule is one horse or bullock to two acres, such a horse pulling 120 lbs. at a rate of 3·6 feet per second for eight hours a day. That is a nominal horse.

We thus get something like 3,000,000,000 over and above the energy necessary for the energy spent in eating the corn and moving itself, which we must put down as at least equal in amount. Taking only the available portion, we have the equivalent per acre of nearly three tons of coal burnt in such steam-engines as exist at present. Now the average weight of the vegetable produce from one acre, taking the form of straw and corn, would be about two tons. So that, as far as mechanical power is concerned, coal burnt in our present steam-engines, and corn and straw eaten by horses, yield about the same energy, weight for weight.

The energy which we derive from sunshine is scattered all over the earth, and if it is to be utilised at any spot other than that at which the sunshine falls, it must be transmitted by the expenditure of power.

The energy required for immediate operations of agriculture absorbs a large proportion of the actual energy grown. The surplus is available for purposes of art, and we may say that the primary object of man has been to render this surplus as large as possible. This is accomplished, in the first instance, by applying the residue of energy to so ameliorate the conditions of agriculture as to increase the yield and diminish the labour. In this way the land is levelled, enclosed, and drained; buildings are erected, and finally, but most important of all, roads are made. The effect of roads in increasing the surplus energy is probably greater than any other human accomplishment. The only means of transmitting for purposes of collection or other purpose aggregate energy in the shape of corn, without roads, is on the backs of animals. In this way two or three hundred miles was the absolute limit to the distance an animal could proceed, carrying its own food. On a good road a horse will draw a ton of food at twenty miles a day, which would mean that it would proceed 800 miles before it had exhausted its supply, or whatever surplus energy there might be available on one spot, half this would be available at 400 miles distance. The labour of maintaining the roads should, of course, be deducted, but this is very small.

The labour of constructing canals is very great, but the result is equal; a horse can move 800 tons twenty miles a day; or a horse could draw his own food for 80,000 miles on a canal. That is to say, with a canal properly formed, a horse could go five times round the world without consuming more energy than was in the boat behind it. Or corn could be sent round the world with a consumption of one-fifth. On railways, at low speeds, the force required is about ten times greater than on a canal, so that the expenditure in going round the world would be about equal to the total energy drawn. If for a moment we replace the horse by the steam-engine, and the corn by coal, we have to add the weight of the engine to the coal, and diminish the efficiency by one-third; we so get that the consumption of coal for the same load of coal as of corn, would be about double, or an engine would go about one-fourth round the world, consuming in coal the net weight in the train, that is exclusive of carriages and engine. Or for every thousand miles corn is carried by rail, something like 10 per cent. of the energy of the corn is expended in draft. This is exclusive of the expenditure in wear and repairs, which will be certainly equal, if not greater. Taking, then, the mean distance by rail between London and the West of America, as 2,000 miles, the present expenditure in the energy of corn in transit is somewhere about 20 per cent. The expenditure of energy on the ocean varies, but if transported by steam it would be probably 10 per cent. more, so that at the present time we are actually receiving available mechanical energy, transported in the form of corn, over 2,000 miles of land and 3,000 miles of sea, entirely by artificially directed power, with an expenditure of less than 20 per cent.; a proportion which 200 years ago would have had to have been spent in transmitting it, fifty miles over land; a result which has been accomplished by the employment in the meantime of the residual energy over and above that necessary for agriculture, together with a further supply drawn from our coal beds.

Turning now our consideration to coal, we find that per weight as used at present, this yields rather less power than corn, but not less than two-thirds, and it then appears that coal may be transmitted at the present time, between any two places on the earth which are connected by rail and water, with an expenditure of less than 50 per cent.

In instituting this comparison, the standard has been the actual available power, as developed in our present engines and in horses, with which, weight for weight, there is not much difference. But the adaptability of this energy, so developed for particular purposes, renders the one medium much more valuable than the other. Thus for agricultural purposes, weight for weight, horse food is worth in money ten times as much as coal. This shows the extreme difference in the value of energy according to its adaptability; and the extension, for which there is unlimited scope, of the adaptability of steam power, may render it ten times as valuable as at present; nor would this be any small proportion compared with the total energy employed in the work of the world. In this country there are said to be between two and three million horses, and we may put the labouring men down at five millions, or the total power derived from corn as over three million horses. From the best information going, the work done by steam in this country does not exceed the labour of two million horses, so that more than half the energy is still derived from corn. A greater proportion of the actual corn used for horse food comes across the Atlantic; and for many years maize was sold in this country at an average price of £6 or £7 a ton, the cost of transit being a very small matter. Of course the same cost, say £1 per ton, applied to coal would be a serious matter, considering the low price of the latter. But if, in the present state of our art, energy can be transmitted by corn from any part of the world to this country with an insensible rise, there is no reason to suppose but that, with the advance which science shows us, there is every reason to expect coal may be transmitted with a corresponding small increase in its cost, wherever the demand for it is sufficient to recompense the outlay necessary for opening the roads or canals.

II.

(*Delivered* April 30, 1883.)

In my last lecture I dealt with the transmission of energy through the means of coal and corn, showing that by either of these means power may be transmitted by rail, with an expenditure of 1/12,000th per mile, or by water of 1/120,000th per mile, this either through the agency of horse or steam.

This ease of transmission, however, depends entirely on the railroad or water, and is only possible between places so connected. Hence such means are only applicable to what may be called the mains of power.

We come to-day to consider other means of transmitting energy in smaller quantities applicable to its distribution for immediate application. Such transmission is not a matter of secondary importance, although the distances over which it is transmitted may be comparatively insignificant. To emphasise this, I may recall what was previously mentioned, namely, that the relative price of corn and coal shows that the power given out by horses is at least ten times as valuable as that of steam, for more than half the purposes for which energy is used; or that it answers better to burn our coal in bringing corn from America to plough in England, than to use the coal here for ploughing.

In fact, for most of the detailed purposes for which power is used, to draw it from a large source (such as a steam-engine), distribute it and adapt it to its purpose, is ten or twenty times more costly than its transportation in large quantities over thousands of miles.

Now the means of artificially transmitting power may be considered as three. The power may be stored in matter in various ways, and the matter with the energy transported—as, for instance, in our watch-springs. The second means is the transmission of power by moving matter, without actually storing the power in the matter—as in shafts and belts, hydraulic connection, &c. And the third method, which is distinct from the others, is the transmission of energy, in the form of heat or electricity, by the flow of currents through conductors; in this way all the power in the steam passes through the boiler-plates from the furnace into the boiler. Of course, each one of these means includes an infinite variety of detailed contrivances, more or less dissimilar. But there is good reason for classing them under these three heads, for all the contrivances under each of these heads are subject to the same general limits, whether those of efficiency or distance.

There is one thing in common to all these means of transmission, and that is that they all involve a material medium. The quantity of matter required constitutes a primary consideration in all of them. This quantity of matter is fixed by what we may call the properties of matter, one of the most important of which, as regards the first two means, is the possible strength of material. Looking round, we see the effect of the limited strength of material in all nature's works. Of course it may be that we shall be able to work with stronger materials than we have at present. Organic materials, such as the feathers and tissues of animals, are stronger than steel, weight for weight, so that there is a possibility of improvement, but that man will go beyond nature in constructing organic fibre seems improbable, and such possibility of improvement as exists may be discounted. At present we may set down our strongest working material as steel, the art of working in which is so perfect, that we may calculate on nearly the greatest strength for all purposes. I have taken fifteen tons on the square inch as the limit of safe working tension, in making the estimates which I shall now bring before you. First of all, I will ask your attention to the possibilities of transporting power in a stored form.

The question of economy in the conveyance of energy in a stored form is simply one of the intensity with which it can be stored. If we want to carry energy about, we must have it stored in some material form—and this material has to be carried by ordinary means—so that the question of economy is simply the amount of available energy that we can store in a given amount of material.

If energy, stored in a particular manner, is more readily available for some special purpose than that stored in another, then it may, on the whole, be more economical to carry it in that form. This is abundantly illustrated in our watch-springs.

The greatest amount of energy that can be stored in a given weight of steel is very small, compared with other means. To take a familiar unit, to store the energy necessary to maintain one horse-power for one hour would require no less than fifty tons of steel—that is to say, fifty tons of steel in the form of watch-springs, all fresh wound-up, would not supply one horse-power for one hour; and yet this is the commonest form in which energy is carried about.

It is the adaptability of the spring, and the readiness with which energy can be put in and taken out, which recommend the steel spring.

India-rubber will store much more energy than the same weight of any other material, say, eight or ten times as much as steel; but of this, several tons would be required to store the horse-power for one hour. A much more capacious reservoir, according to its weight, is compressed air. There are certain difficulties in getting the energy in and out without loss; but with air, compressed to four times the pressure of the atmosphere, we should only require about 20 lbs. of air to yield the amount of one horse-power for one hour. Of course, if we were going to carry this air about, to the weight of the air would have to be added the weight of a case to contain it, and such a case, in the form of steel tubes, would weigh something like 230 lbs.; so that, in any form in which we can carry compressed air about, we shall have about 300 lbs. to carry for each horse-power per hour.

Another means of storing energy, very largely used, is hot water. This is largely used in a way not always recognised. The boiler serves another purpose besides that of converting the energy of the furnace into the power of the steam. It stores the power, and equalises the stream between the fire and the engine, a function the importance of which has been brought to the front in the recent efforts to apply electricity for communication of power, where the want of a similar reservoir between the generator and the motor has, in many cases, proved fatal to the enterprise, a want which secondary batteries are now being used to meet. Hot water has also been employed as an independent reservoir, and as such it is better in some respects than compressed air. The fundamental limits are of much the same kind. In this case, however, the absolute limit is temperature. The vessel in which the water is carried must be strong enough to withstand the pressure, and all materials lose their strength as they get hot. The considerations are here much the same as in the steam-engine, and 400° Fah. appears to be about the limit. At this temperature, for every 4 lbs. of water the cases would weigh 1 lb., and there would be no advantage of large over small cases; except as a matter of construction, the proportionate weight would be the same. The gross power of a pound of water, the steam being used without condensation, is about 20,000 foot-pounds, or we should require 50 lbs. to store 1,000,000; this is the extreme limit again. The present accomplishment would be about 150 lbs. per 1,000,000 foot-pounds stored—

rather less than compressed air. The only other means of packing power, that is at present looked to, is that of the much talked about secondary battery. Here there is a great deal of doubt as to what is actually accomplished; take the most reliable statements, from which it seems that in order to get 1,000,000 foot-pounds, something like 100 lbs. of battery is required, which will make this means of storing energy very much the same as compressed air or hot water.

It is important to notice that the initial cost of the energy stored by these means differs considerably. This cost is rather difficult to estimate; but a practical estimate may be formed in this way:—

Taking the power, as delivered by the steam-engine, as 1, how much of this power will be given out after secondary storage? Here the hot water has an advantage, for it is heated directly by the coal, and is all on its way to the steam-engine.

With compressed air, there are three operations, each as costly as the steam-engine, and at least half the initial power is spent during the compression, storage, and expansion; so that the energy is at least double as costly in coal, and six times as costly in machinery. I have put it down as three times as costly as the energy in hot water, but this is considerably below the mark. The electricity has also to go through three operations, and cannot be less costly than compressed air.

Now, if we revert for one moment to the consideration of the main transmission of power, we see at what an immense disadvantage any form of packed energy is, compared with coal or corn; as at present packed it weighs at least 100 times as much.

While the limits imposed by the strength of material render it certain, as far as compressed air and hot water are concerned, that the weight can never be reduced by more than half, these limits are sufficient to show that packed energy cannot be transported over long distances, even if it can be obtained directly from such falls as Niagara. But this is no argument against the importance of these means for short distances and special purposes. As I have already pointed out, our watches show that circumstances may render the very heaviest means the best for particular purposes. And if in any of its forms packed energy were directly available for household purposes, though it cost ten or twenty times as much as power direct from the steam-engine, its use would still be assured.

One fact should be noticed, that in all these forms the power is packed, and needs nothing but drawing off, whereas corn or coal do not contain the power. The oxygen is an equally essential ingredient. In this fact lies the great advantage of corn and coal for transportation. They are really, so to speak, but cheques for power, which can be cashed at any spot where a bank, in the form of a steam-engine or a horse, exists. But, of course, not being energy, they are not generally current—in fact they are worthless, except where the bank exists, and even there when they represent such small amounts that the banks refuse them. Now these forms of packed power are, so to speak, generally current; that is to say, they are available under almost all circumstances, and in greater or less degrees of smallness; from the very smallest, which is the watch-spring in our pockets, which supplies a continuous stream of power in less than one ten thousand millionth of a horse-power; or the Whitehead torpedo, which carries some million foot-pounds of energy under the sea. Perhaps the most pressing purpose for which these forms of packed energy are wanting is that of locomotion.

The distance which a locomotive body, be it animal or machine, can travel, loaded or free, is limited by the ratio of the power which it carries to its gross weight. The speed which it can attain is limited by the rate at which it can use its energy compared with its weight. Hence there are two particulars in which we can compare the different forms of stored energy for locomotive purposes.

Let us take the horse and the locomotive. A full-sized horse weighs, say, 1,500 lbs., and, at a rate of 2½ miles an hour, will go five hours without food, doing about 10,000,000 foot-pounds of work, including the work necessary to move itself; this represents the largest result, or about 150 lbs. per 1,000,000 foot-pounds. If the horse is put to ten miles an hour, it will not do more than 1·5 million foot-pounds in a single journey, besides moving itself. Probably the greatest rate at which a horse can use its energy is about 4,000,000 foot-pounds per hour, or 750 lbs. per horse-power.

A locomotive with its tender, say, weighing sixty tons, exerts 500 horse-power gross—270 lbs. per horse-power; so that a first-class locomotive with tender is about one-fifth as heavy for its power as the horse; but then the horse cannot go more than ten miles an hour.

Now, in a general way, passenger locomotives carry coal and water for eighty or one hundred miles, *i.e.* two hours; or the locomotive already mentioned expends at one run about 2,000,000,000 foot-pounds; which means that the gross weight of the locomotive is about 60 lbs. or 70 lbs. per 1,000,000 foot-pounds of power with which the locomotive starts.

In thus taking the gross weight of the horse or locomotive, we must remember that this includes the weight of carriage and machinery, and that in whatever form the energy is carried, this weight must be added. In the locomotive the weight of water and coal in the tender for two hours' journey weighs about one-quarter the gross load; and if we add the weight of the boiler, we may consider the carriage and machinery at one-half to one-third the gross load. Taking the latter, and substituting for the boiler, coal, and water, energy in either of the above forms, the coal, water, and boiler would be about 40 lbs. per 1,000,000: so that, if we took compressed air instead, we should have one-fourth the power; or the engine would run for thirty minutes instead of two hours, a distance of twenty-five miles instead of a hundred. A fireless locomotive might do more than this, say, thirty-five minutes, or thirty miles, at the same speed as the locomotive. Faure's battery, if it could be made to work at all, would carry the locomotive forty-eight minutes, or thirty-five to forty miles.

These figures seem to show that the locomotive has little to fear from any of these rivals, that is, under circumstances where the smoke and steam are no harm, and where a full-sized locomotive is required. But there are already some cases where the locomotive is required and where the burning of coal is impossible. Should the Channel Tunnel be made, there will be a great field for some form of packed energy. As regards horses, however, there is nothing to show why the horse should not be rivalled by some one of the forms of packed energy. There have been inventors who have constructed carriages to go by clockwork. This has now become possible, substituting hot water, compressed air, or a battery for the spring, and such means have already rivalled the horse on tramways. The fact that horses are at all used for tramcars is a matter of as much surprise as that steam should be used on underground railways. For locomotives driven by compressed air might certainly be made cheaper and better in every way.

At the present time it would probably answer well, from a pecuniary point of view, to supply in compressed air energy at the rate of *2d.* or *3d.* per million foot-pounds, provided a sufficient quantity could be required; so that if simple and efficient means of applying such energy to perform the heavier part of manual labour could be found, we might get as much power for *6d.* as a man will do in a day at *2s.* But it is the means of applying it that is wanting.

Even for horse work—except where there is a railway or tramway—the mechanical means are wanting. We have no mechanical substitute for the horse's foot. So that there are more than a million horses in this country continually engaged in the operations of husbandry, where they work in groups so as to get three or four horse-power at one operation, an amount of power not too small for the direct application of steam power; and although for twenty-five years steam-engine makers have been doing their very best to adapt the power of the steam-engine to this labour, which exceeds any other actual application of power, the possibility of steam ploughing with economy is still a question. The use of steam on paved or on macadam roads is much the same, so that, until steam has been applied to such purposes, there is little hope for other forms of stored energy.

Coming back for a moment to Faure's battery, I would carefully point out that the result which I have put down—100 lbs. per 1,000,000 foot-pounds of energy—refers to what has been already accomplished, and not to any possible limit. The principles involved in the chemical action of these batteries, in fact in all batteries, are well understood; and so far as these principles are involved, it is easy to define limits; but there are a number of secondary actions which are not so well understood, and which have hitherto prevented any approach to the theoretical limits. In the Faure's battery, the theoretical limits are about 3 lbs. per 1,000,000 foot-pounds. That is to say, the oxidisation of 1 lb. of lead to litharge, and the deoxidisation of 1 lb. of peroxide, together, yield 360,000 foot-pounds. How far, at present, Faure's battery is within this limit, at once appears something like twenty-four times. Should this be accomplished, power could be packed at the rate of 1,000,000 foot-pounds for 3 lbs., or say 6 lbs. weight, to allow for wastes, a result which would most certainly displace steam in the locomotive, but which would still leave coal and corn six times the lightest vehicle of power.

It should be noticed, however, that although the means of doing so are still entirely wanting, could other metals, such as iron or zinc, be used instead of lead, the results would be much greater. This is shown by the relative amount of power necessary to oxidise or deoxidise these materials, which we see for iron and zinc are five or six times greater than for lead; here is an apparent opportunity for art.

Should this be realised, then, indeed, coal might be displaced as the cheapest medium for the transmission of power, but that would be a small matter compared with the change that would occur in our ways of applying power. For the dream of Jules Verne, of 20,000 miles under the sea, would become a reality, and, instead of steamboats, we should travel in submarine monsters as yet unnamed, which we may call steam-fish.

But if science as yet imposes no limits beyond those I have mentioned, at the same time it holds out no prospect. The chemistry of these batteries has been very deeply considered, and those who have studied the subject most deeply apparently see no direction in which to direct their efforts; so that any great advance in this art must entail a great discovery in science.

There now only remains for me to consider the transmission of power as power, or by electricity, a most important branch of my subject, which I must take in my next lecture.

III.

(*Delivered* May 7, 1883.)

So far I have spoken only of the conveyance of power by means of coal, corn, or in one or other of the several forms of packed energy. To-night I come to consider the transmission of power by what are more distinctly mechanical methods, and by currents along pipes and conductors. These are the means by which power is almost always distributed, *i.e.*, transmitted from the acting agent, be it horse, water-wheel, or steam-engine, to its operation, whatever it may be. In most cases the distance of such transmission is so short as to be the subject of small consideration in determining the means to be employed. That is to say, the means are chosen rather by their adaptability to receive and render up the power than by the efficiency with which they transmit it. Thus, if we take an ordinary mill, the shaft which receives the power from the engine is generally driven at that speed which is best adapted to receive the power from the engine, and deliver it to the machinery in the mill, without considering whether a much smaller shaft might be used if it were caused to run at a much higher speed. Thus, in a mill driven by an engine of two or three hundred horse-power, the shaft which receives the power will generally be five or six inches in diameter, whereas it would be possible to use a shaft of two inches diameter if the efficiency of the shaft were the only consideration. Or, again, take a screw steamboat. The distance from the engines to the screw may be 250 feet, the power 10,000 horse. This could be transmitted by a shaft twelve inches in diameter, if allowed sufficient speed, but the screw has to make sixty revolutions per minute, and this determines the speed at which the shaft is made to run, and hence the shaft is made thirty inches instead of twelve inches. This is because, owing to the smallness of the distance, the efficiency of the means of transmitting the power is a small consideration. There are, however, many circumstances under which it is impossible to bring the source of power close to its work, and then either mechanical power is not used, or the efficiency of the means becomes a consideration.

In other cases it is a question whether it is better to distribute the sources of power, such as steam-engines, so that they may be near their work, or to use one large source, and distribute the power by some mechanical means. This rivalry exists in almost all engineering work which covers a large area, and, generally, a compromise is come to, engines being distributed about the works, and the power of these distributed to the machines by means of shafting. In many cases separate engines are used for each machine, but not often separate boilers, the power being distributed by steam-pipes.

Dockyards have long afforded a field for the competition of the various means of distributing power. Here, generally, the distances between the operating machines, such as cranes and capstans, is considerable, and the work required from each machine very casual. And every means of distribution is or has been in use, from a separate engine and boiler to each machine as at Glasgow, separate engines drawing their steam from central boilers, to a complete system of hydraulic transmission from a central pumping station, as at Grimsby or Birkenhead.

But the question between centralisation or distribution of steam-engines is not by any means the only one, or most important one, which depends on mechanical means of distributing power. Every improvement in the means of distributing power from a central engine opens a fresh field for its use.

The considerations relating to this subject are numerous. Hitherto, as regards the main transmission of power, the principal consideration has been the percentage of loss according to the distance; but, as regards the final distribution of power, the form in which it is distributed must be such as admits of its being at once available for its purpose. Thus hydraulic distribution is favoured in dockyards, because it is required for heavy forces and slow motions, but where rapid motion is required, hydraulic distribution gives place to some other.

Again, where the quantity of power that has to be distributed is a most important consideration, the distribution by means of water or compressed air will generally be the most efficient, whereas these would be by far the most costly means for small quantities. It thus has to be remembered that, besides the general question of efficiency, each means has particular recommendations for particular purposes.

It is not, however, with these particular recommendations that I am concerned. My object is to show the limits within which the use of each means is confined, however fit it may be for its purpose. Taking first the mechanical means, which are shafts and ropes, we find that the possible limits to both these means are absolutely defined by the strength of material. The amount of power any piece of material will transmit by motion against resistance, is simply the mean product of the stress or force acting in the direction of motion on the section multiplied by the velocity, so that, if the stress is uniform over the section, the work is the product of the area and intensity of stress and the velocity.

In a revolving shaft, neither the stress nor the velocity is uniform over the section, both varying uniformly from nothing in the middle to their greatest value on the outside; so that their mean product is exactly half the product of the greatest values. The greatest power per square unit of section a shaft can transmit is half the product of the greatest stress into the velocity at the outside of the shaft.

Taking, then, the greatest safe working stress for steel at 15,000 lbs. on the square inch; taking what is the greatest practical velocity at the surface, 10 feet per second (the speed of railway journals); the work transmitted is 75,000 foot-pounds per second per square inch of section—135 horse-power; so that we should have to have a shaft of upwards of 7 square inches in section to transmit 1,000 horse-power, that is, a shaft of over 3 inch diameter. The friction between such a shaft and lubricated bearings is well known, ·04; so that, calculating the weight of the shaft 24 lbs. per foot, we have power spent in friction about 52,000 foot-pounds per mile, that is one-tenth the total power the shaft will transmit. That is, if we put 1,000 horse-power into a 3-inch shaft, making 500 revolutions per minute, we ought, at the end of a mile, to be able to take 900 horse-power out of it. If we had to go farther, the size of the shaft might be diminished, so that in the next mile we should again lose a tenth, and if we repeat this process seven times, we shall, at the end of seven miles, have left about half the original power put in.

It will be thought, perhaps, that a 3-inch shaft is very small to transmit so large a force; this is because the speed of 500 revolutions per minute is inconveniently high for purposes of employing the power; but if it were merely a question of transmission, it would be about the best speed. This, then, shows the limit of the capacity of shafts as transmitters of work.

Turning now to steel ropes, these have a great advantage over shafts, for the stress on the section will be uniform, the velocity will be uniform, and may be at least ten to fifteen times as great as with shafts—say 100 feet per second; the rope is carried on friction pulleys, which may be at distances of five or six hundred feet, so that the coefficient of friction will not be more than ·015, instead of ·04. Taking all this into account, and turning to actual results, the work transmitted per inch would be 1,500,000 foot-pounds per second; or that a $\frac{3}{4}$-inch rope is all that is necessary to transmit 1,000 horse-power in one direction, this would make the loss per mile only 1-60th. But in practice, rope has to be worked backwards and forwards, and the tension in the backward portion of the rope must be half the tension in the forward portion. This reduces the performance from 1-60th to 1-20th, which would cause half the work to be lost in ten miles. If we use a bigger rope, and run at lower speed, then the coefficient of friction would be reduced to ·01, and the distance extended to fifteen miles.

Experience with ropes is large, and they have been found, without question, to have been the most efficient mechanical means of transmitting power to long distances, but their use is subject to drawbacks. The ropes wear somewhat rapidly, as do also the pulleys on which they run, and this circumstance is very much against their use in any permanent work. Nevertheless, they are used for working mines, steep inclines, and steam-ploughs; while at Schaffhausen they have been used for transmitting power to considerable distances.

Turning to the transmission of power along pipes, we find the conditions somewhat modified. The formula for the amount of power transmitted by water is the same, namely, the product of the pressure and area of section into the velocity. But the resistance obeys different laws. In the case of shafts and ropes, we have seen that the distance is subject to an absolute limit.

In the case of fluid in pipes this is not so. No matter how long a pipe may be, if there is no leakage, water would flow along the pipe until the level of its surface were the same at both ends. But the rate of flow would diminish with the length and diameter of the pipe. Thus we can transmit power through a perfectly tight pipe, however small, and however long; but when we come to consider the gross power that can be transmitted through a given pipe, with a given percentage of loss, the question is different. Given the size and strength of the pipe, the gross amount of power, and the percentage of loss, and the limits are fixed. Thus, taking a 12-inch pipe capable of standing 1,400 lbs. on the square inch, the loss in transmitting 1,000 horse-power would be about 5 per cent. per mile, at first increasing—as the pressure fell to 700 lbs.—to 10 per cent. We should thus have lost half the power in about seven miles. We cannot say that seven miles is the absolute limit, for with a 24 inch pipe, which would cost four times as much per mile, we could transmit the same power thirty times as far with the same loss. The cost of laying a 12-inch pipe for seven miles, however, would probably be as much as even 1,000 horse-power would stand; while a 24-inch pipe for 200 miles would be out of all proportion. Then there is the consideration of leakage, which, although very small for short lengths, is larger for greater lengths.

Seven miles is at present an outside economical limit of hydraulic transmission, even for such a large amount of power: but with air the case is different. This flows so much easier than water, that the cost of transmitting the same power through the same distance, with the same loss, would be about 12 per cent., or, at the same cost per mile, the air may be transmitted 100 times as far with the same loss. The total cost, however, would thus be 100 times as great, which would exceed the economical limit; but not only theory but practice has shown that power may be economically transmitted five times as far by air as by water—something like thirty miles. But on comparing these two means, one circumstance must not be lost sight of, and that is, that getting the power into the pipe in the form of compressed air, will cost twice as much as getting it in in the form of water. This is a great advantage for water where the distance is short, but where the distance is long, the greater efficiency of air more than compensates for this initial loss.

Like water, air can only be transmitted economically where the quantity is large, the friction being proportionately greater in small pipes than in large, varying as the four-fifths power of the diameter.

This is a great drawback, both as regards hydraulic and compressed air transmission. It does not affect ropes and shafts in the same way, but even in these cases considerations of durability prevent these means being used efficiently for the transmission of small quantities of power to considerable distances, so that, with the possibility already mentioned, there remains an opening for any means that will enable power to be transmitted efficiently in small quantities, and such a means we have in the flow of electricity along wires or conductors. In considering electricity, we may well start with the questions, (1) Will electricity enable us to transmit power in large quantities more efficiently than the foregoing means? (2) Will it enable us to transmit small quantities? These questions may be more definitely answered than they could a few weeks ago. Thanks to the experiments of M. Deprez, who appears to have been the only one, out of all those who are advocating the use of electricity, who has had the courage to try and see what can be done, we can now say with certainty that a current of electricity, equivalent to 5 horse-power, may be sent along a telegraph wire 1-6th of an inch in diameter, some ten miles long (there and back) with an expenditure of 29 per cent. of the power, because this has already been done. In order to do this, it would seem that M. Deprez has perfected his apparatus so as to have nearly reached the possible limit. Compared with wire rope, this means falls short in actual efficiency, as M. Hirn sends 500 horse-power along a $\frac{3}{4}$-inch rope. To carry this amount, as in the experiment of Deprez, one hundred telegraph wires would be required; these wound into a rope would make it more than 1·4 inches in diameter, four times the weight of M. Hirn's rope. With the moving rope the loss per mile is only 1·4 per cent., while with the electricity it was nearly 6; so that, as regards weight of conductor and efficiency, the electric transmission is far inferior to the flying rope. Nor is this all. With the flying belt, M. Hirn found the loss at the ends, in getting the power into and out of the rope, 2½ per cent.; whereas, in M. Deprez's experiment, 30 per cent. was lost in the electric machinery alone, which is very small as such machinery goes. But this is not all. No account is here taken of the loss of power in the transmission to and from the electric machinery, a matter which is, I believe, very much under-estimated.

The machines made revolutions at 1,000 and 700, much too high for direct connection with either a steam-engine or any mechanical operator: the power, then, had at each end to be transmitted through gearing, or a system of belts. And supposing this alteration of speed to have been five or six at each end, experience tells us that a loss of at least 15 per cent. must ensue. This loss was indeed apparent, for the dynamometer was connected with the machine with a belt, which showed a loss from this one belt alone of 20 per cent. Taking the whole result, it does not appear that more than 15 or 20 per cent. of the work done by the steam-engine could have been applied to any mechanical operation at the other end of the line, as against 90 per cent. which might have been realised with wire rope transmission. To set off against this, electricity has the enormous advantage in the conductor being fixed, and in the fact that it is likely to be, if anything, less costly and more efficient for small quantities of power than for large. These advantages will certainly insure a very large use for electricity in the distribution of power, particularly for high speed machinery.

There is yet another means of communicating and distributing energy now coming rapidly into vogue. This is by the transmission of coal-gas along pipes. The distances, often many miles, through which the gas is often transmitted before reaching the engine, are such that, with any other means of distributing power, would considerably enhance the cost of the power. But in the case of gas, it does not appear that these distances are at all a matter of consideration. This may be at once explained. It takes about ten cubic feet of gas to develop 1,000,000 foot-pounds in a gas-engine, whereas of air compressed in the ordinary way it would require something like 140 cubic feet to yield the same power. Hence the comparative cost of transmission is the cost of transmitting ten cubic feet of gas against that of 140 cubic feet of compressed air, and these would be about as one to twenty-five; so, as a means of distributing energy, gas is twenty-five times more efficient than compressed air.

I have now placed before you, as far as circumstances will allow, the various means by which energy, in a form available for power, may be transmitted over long distances, together with the circumstances which limit such transmission. By means of the railway and steamboat, corn and coal can be, nay is, transmitted half-way round the earth with an expenditure of power less than half the power represented by the coal carried, but this can only be done where the quantity to be transmitted is very large.

At present this efficiency is unrivalled, no means of packed energy or of current energy approaching even 1 per cent. And further, there is apparent room for a large diminution in the present expenditure, small as it is, in the improvement of the steam-engine as a means of directing the energy of coal. For the distribution of power, this means ceases to be efficient, nor can it be employed to transmit energy which has already taken the form of power. For these purposes other means have to be employed. These various means, although they differ greatly in efficiency, all fall so far below the efficiency of coal and corn, that a hundred miles appears to be the outside limit any economical transmission of power in quantity for mechanical purposes, could be at present effected; and hence any power, be it derived from wind or water, must be used within this radius of its source; and, except in places far out of the reach of rail or water, this limit may be divided by ten.

So far as efficiency of transmission in considerable quantities, neither secondary batteries nor electrical transmission are more efficient than compressed air or belts, but when it comes to transmitting small quantities, then electric transmission has a decided advantage. The cost of the electric conductor diminishes with the quantity to be transmitted, and by making the conductor sufficiently large, its efficiency may be increased to any extent.

At the present time, electric conductors are continuous half-way round the world, and whenever a message is sent from England to Australia direct energy is transmitted 10,000 miles, but in what quantity? The energy of the current, as it arrives, is not much more than sufficient to keep a watch going, at any rate not more than 1·1000 millionths of horse-power. The value of such energy, estimated at £17 per minute, would be equivalent to a billion pounds per horse-power per hour, whereas the highest price paid for animal labour in Australia or England is not more than 6*d.* per horse-power per hour. This shows the difference between the transmission of electricity for telegraphic purposes and its transmission for mechanical purposes. Energy differs in value greatly, but for operations that can be performed by men or horses, the price of energy must be regulated by the highest price of corn.

The prosperity of any spot in the past depended on the fertility of the adjacent soil. But the use of coal has altered this, and now the present prosperity of this country is owing to the adjacency of our coal-fields, these having rendered it possible to bring our food across the earth. The improved means of transmitting coal and corn, it would seem, have, or may again, change this, and if, instead of looking on the life of this country as limited by the life of our coal-fields, we look boldly forward, and foster every political, social, and mechanical, which may render this a favourite o live upon, we need not fear that the necessity of bringing our coal m a distance will make a difference which will counterbalance the advantage we shall derive from the mechanical facilities we shall have here.

46.

[*Read before Section A at the* "British Association," 1883.]

ON THE EQUATIONS OF MOTION AND THE BOUNDARY CONDITIONS FOR VISCOUS FLUIDS.

TAKING the ordinary equations of motion for viscous fluid, and supposing a tube indefinitely broad in the direction z, bounded by solid surfaces

$$y = \pm c \qquad\text{(1)},$$

which tube may be supposed continued in a circle so as to make a circular trough. Suppose it full of water, at rest, and subject to an acceleration X, the equation of motion gives

$$\frac{du}{dt} = \mu \frac{d^2u}{dy^2} + X \qquad\text{(2)},$$

or by altering the arrangement, instead of X we may have $-\frac{1}{\rho}\frac{dp}{du}$.

Now initially $u = 0$, $\quad \therefore \frac{du}{dy}, \frac{d^2u}{dy^2} = 0$;

$$\therefore \frac{du}{dt} = X,$$

or

$$= -\frac{1}{\rho}\frac{dp}{du},$$

right up to the surface.

But by the boundary condition at the solid surface $u = 0$ always;

$$\therefore \frac{du}{dt} = 0; \ \therefore X = 0,$$

which shows that the boundary conditions are at variance with the equation of motion.

The equations simplify to

$$\frac{\partial u}{\partial t} = \mu \frac{d^2 u}{dy^2} - \frac{1}{\rho} \frac{dp}{dx} + X \text{ .... (3)},$$

with the boundary condition that u is always zero at the boundaries.

For initial conditions we will take p constant, and u uniformly zero.

The equation (2) then becomes

$$\frac{\partial u}{dt} = X \text{ .... (4)}.$$

If now we suppose X to have a uniform value the equation of motion gives $\frac{du}{dt} = X$ throughout the fluid, *i.e.* at the boundaries. This is contrary to the boundary conditions, for if u is always zero at the boundaries $\frac{du}{dt}$ must also be zero.

The functions wanting were rendered evident in the following manner.

By differentiating equation (3) with respect to t, remembering that X and p are constants, and that $\frac{\partial u}{\partial t} = \frac{du}{dt}$,

$$\frac{d}{dt}\frac{du}{dt} = \mu \frac{d^2}{dy^2}\left(\frac{du}{dt}\right) \text{ .... (5)},$$

an equation of which the integrals are well known,

$$\frac{du}{dt} = \Sigma \left(A e^{(\sqrt{\mu} y + \mu t)}\right) \text{ .... (6)},$$

and which may be determined to suit the initial and boundary conditions.

But it does not follow that the value of $\frac{du}{dt}$ in equation (6) is the same as in (4) because the integral of (5) includes an arbitrary function of y,

$$\frac{du}{dt} = \mu \frac{d^2 u}{dy^2} + f(y) \text{ .... (7)}.$$

If we determine $f(y)$ to suit equation (4) then equation (7) will not fit the initial boundary conditions.

If however we determine $f(y)$ so that equation (4) shall be satisfied at some small distance r from the boundary, and the boundary condition satisfied we have

$$f(y) = X\left(1 - e^{-\frac{py \pm c}{r}}\right) \text{ .... (8)},$$

where p is a numerical large quantity.

Such a function satisfies all the boundary conditions, but the general value of the function would be

$$f(y) = X - \Sigma\left(X_0 e^{\frac{p_0 \sqrt{y \pm c}}{r}}\right),$$

where

$$\Sigma X_0 = X \quad \text{.................................} (9).$$

The addition of such a function to the equation of motion would meet the initial conditions of the case in question, in which X is independent of t, but this is all, for the boundary conditions include that we must have all the differential coefficients of u with respect to t zero at the boundary, to meet which case it would be necessary to have a function

$$F(yt) = \Sigma\left(X_0 e^{\frac{p_0 (y \pm c)}{r}}\right) + t\left(\Sigma X_1 e^{\frac{p_1 y \pm c}{r}} t\right) + \text{\&c.} \quad \text{............} (10).$$

Instead of adding such functions, however, it seems better to consider in what way the equation of motion can be modified so that these functions result from integration. This would be the case if instead of equation (3) we had the equation

$$\frac{du}{dt} - \Sigma \frac{r_0}{p_0} A_0 \frac{d^2}{dy^2}\left(\frac{du}{dt}\right) = -\frac{dp}{dx} + X + \mu \frac{d^2u}{dy^2} \quad \text{.............} (11),$$

where

$$\Sigma A_0 = 1.$$

That is, if we add the term $\Sigma \frac{r_0}{p_0} A \frac{d^2}{dy^2} \frac{du}{dt}$ to the equation, it becomes compatible with the boundary conditions, and the term itself is of the same order as others which have been neglected in constructing the equations of motion, and the strong presumption is that such terms have been neglected.

The case pursued here is the simplest possible, but by a similar method it may be shown that the general case for a fluid at constant density will be met if the equations of motion be modified as follows:

$$\left.\begin{aligned} \frac{du}{dt} - \frac{r^2}{p^2} \nabla^2 \frac{du}{dt} &= -\frac{dp}{dx} + X + \mu\nabla^2 u \\ \frac{dv}{dt} - \frac{r^2}{p^2} \nabla^2 \frac{dv}{dt} &= -\frac{dp}{dy} + Y + \mu\nabla^2 v \\ \frac{dw}{dt} - \frac{r^2}{p^2} \nabla^2 \frac{dw}{dt} &= -\frac{dp}{dz} + Z + \mu\nabla^2 w \end{aligned}\right\} \quad \text{..................} (12).$$

The equations of motion were not originally the outcome of any complete hypothesis of the molecular constitution of fluids. They involved certain assumptions which would enter into such an hypothesis, but by no means completely define it, any more than would the phenomena of approximately steady motion suffice to define the complete phenomena of motion.

The original basis of the equations of motion for viscous fluids were certain experimental phenomena, and it is important to notice that all these phenomena belong to what may be called approximately steady motions. So that neither the experimental verification of these equations, nor the molecular hypothesis on which they were originally based, was in any sense complete or general. And if the original framers of these equations had attempted to carry them to the second order of small quantities, it would only have been done by further molecular assumptions—and anything like a complete experimental verification was entirely wanting.

This aspect of the case was changed by the foundation of a complete molecular hypothesis of gases, for founding as it did the dynamical theory of gases on complete fundamental assumptions, the equations of motion followed as a consequence of these assumptions—and although not attempted, could have been obtained to any degree of small quantities. Maxwell contented himself with showing that the equations of motion resulting from his assumptions agreed with the equations of motion obtained by Stokes to the first order of small quantities*, but it was perfectly possible to have pursued his reasoning to the second order of small quantities. Having then found that certain terms of the second order were wanting in the equations of motion to meet the boundary conditions as shown by experiment, the most probable method of defining these terms seemed to be to carry the dynamical theory of gases to the second order of small quantities.

For this investigation I adopted the same method as that which I have explained in my paper on the dimensional properties of matter in the gaseous state†, merely extending the method to meet the case of varying motion. The result was that I found terms of the form required, but they entered into the equations with the opposite sign to those required to meet the boundary conditions, and would thus only introduce arbitrary constants of a periodic character. Besides which, these terms clearly vanished at the boundaries, *i.e.* if the boundary were regarded as a plane of total reflection; while according to the theory, as regards the first order of small quantities, the boundary produced no tangential effects whatever.

Having considerable confidence in the method I was using in deducing the equations of motion from the fundamental assumption; it naturally occurred to me to re-examine the fundamental assumptions, to see if these had been introduced into the theory in their fulness. It was then I observed that the theory, both as applied by Maxwell, and myself, neglected any possible dimensions of a molecule, and it became clear that by neglecting

* *Phil. Trans.*, 1867, p. 81.

† See Paper 33, Vol. I., pp. 257 ff.

this we had neglected that which made it possible for the boundary to produce an acceleration on the fluid.

By neglecting the dimensions of a molecule, the cause of transference of momentum across a surface reduces itself to the carriage of momentum by the moving molecules, whereas if we take the size of molecules into account, a certain portion of the area of any ideal surface drawn through the gas must be occupied by the solid matter of the molecules, and the stresses in these molecules will be the cause of the transference of momentum across the surface. This cause of the transference of momentum across a plane had been ignored with the dimensions of the molecule in the theory of gases*.

It became necessary therefore to take this into account to see what effect it had on the equations of motion.

It is clear that this effect would involve the elasticity of the molecules themselves, as the rate at which momentum would traverse them would be that of the propagation of sound in a solid, but considering the relative elasticities of solids and gases, it seemed legitimate to take the elasticity of the molecules as infinite compared with that of the gas, *i.e.* to assume the molecules as absolutely rigid—and the same for groups of molecules in contact, either directly or through other molecules with a solid surface.

Now if we imagine a surface plane for the instant to be moving with the mean velocity of the matter which it traverses, and suppose that m molecules are cut by an unit of this plane and if the m molecules, cut by a plane parallel to the first and at a distance r, (the diameter of a molecule) $\frac{m}{q}$ are in contact with those on the first, then if we have a third plane also at a distance r from the second, $\frac{m}{q}$ of the molecule cut by this will be directly in contact with the second, and $\frac{m}{q^2}$ indirectly in contact with those on the first, so that of the m molecules on two planes at a distance y from each other

$$mq^{-\frac{y}{r}} = me^{-\frac{py}{r}},$$

where $$p = \log_e q,$$

will be indirectly in contact with each other.

Now since according to the assumptions, we regard this connection as

* See Paper 33, Vol. I., pp. 257 ff., "On Certain Dimensional Properties of Matter in the Gaseous State."

rigid, we see that if ρ is the density of the matter on any plane, this matter is rigidly connected with matter

$$=\rho e^{-\frac{py}{r}},$$

at a distance y on either side of the plane, which therefore is an expression for the rigidity of a gas. And it may be noticed that, although the distance to which this rigidity extends is limited by the value of $\frac{r}{p}$ at a surface such as $y = 0$, it is absolute, so that at a solid surface all the matter (not molecules) in contact with the surface, has the mean motion of this surface whatever may be the value of $\frac{p}{r}$.

The expression $\rho e^{-\frac{py}{r}}$ has been obtained on the hypothesis of the distribution of molecules in a gas, and even so, without any very great degree of refining.

It is impossible without a more definite hypothesis than has been propounded at present as to the constitution of a liquid, to say what form the expression for rigidity might there take, but it is reasonable to suppose that as regards the law of molecular contact, it would be the same as that of a gas, only p instead of being large would be very small, but as regards the rigidity, the same assumption could not be made any more than a whole fishing-rod can be considered rigid in the same degree as a single joint of such a rod.

47.

ON THE GENERAL THEORY OF THERMO-DYNAMICS.

A LECTURE DELIVERED TO THE INSTITUTION OF CIVIL ENGINEERS. 15 NOVEMBER, 1883.

From "The Proceedings of the Institution of Civil Engineers, 1883."

In lecturing on any subject, it seems to be a natural course to begin with a clear explanation of the nature, purpose, and scope of the subject. But in answer to the question—What is thermo-dynamics? I feel tempted to reply—It is a very difficult subject, nearly, if not quite, unfit for a lecture. The reasoning involved is such as can only be expressed in mathematical language. But this alone should not preclude the discussion of the leading features in popular language. The physical theories of astronomy, light, and sound involve even more complex reasoning, and yet these have been rendered popular, to the very great improvement of the theories. Had it appeared to me that it was the necessity for mathematical expression which alone stood in the way of a general comprehension of this subject, I should have felt compelled to decline to deliver this lecture, honourable as I acknowledge the task to be.

What I conceive to be the real difficulty in the apprehension of the leading features of thermo-dynamics is, that it deals with a thing or entity (if I may so call heat) which, although we can recognise and measure its effects, is yet of such a nature that we cannot with any of our senses perceive its mode of operation.

Imagine, for a moment, that clocks had been the work of Nature, and that the mechanism had been on such a small scale as to be imperceptible even with the highest microscope. The task of Galileo would then have been reversed; instead of inventing machinery to perform a certain object, his task would have been from the observed motion of the hands to have discovered the mechanical principles and actions of which these motions were the result. Such an effort of reason would be strictly parallel to that which was required for the discovery of the mechanical principles and actions of which the phenomena of heat were the result.

In the imaginary case of the clock, the discovery might have been made in either of two ways. The scientific method would have been to have observed that the motion of the hands of the clock depended on uniform intermittent motion; this would have led to the principle of the uniformity of the period of vibrating bodies, and on this principle the whole theory of dynamics might have been founded. Such a theory would have been as obscure, but not more obscure, than the theory of thermo-dynamics. But there was another method in the case of timekeepers, the one by which the theory of dynamics was actually brought to light—namely, the invention of an artificial clock, the action of which could be seen, and, so to speak, understood. It was from the pendulum that the constancy of the periods of vibrating bodies was discovered, and from this followed the dynamical theories of astronomy, light, and sound. There is no great difficulty in the apprehension of these theories, because they do not call for the creation of a mental picture, but merely for the exaggeration or diminution of what we can actually see in the clock.

As regards the mechanical theory of heat, however, no visible mechanical contrivance was discovered or recognised which afforded an example of this action; apparently, therefore, the only possible method was the scientific method—namely, the discovery of the laws of its action from the observation of the phenomena of heat, and accepting these laws, without forming any mental image of the dynamical origin, was the only method open. This is what the present theory of thermo-dynamics purports to be.

But although the theory of thermo-dynamics may be said to have been discovered in the form in which it is now put forward, this is not quite true. For one of the discoverers of the second law, and the one who had priority over the others, worked by the aid of a definite mechanical hypothesis as to the actual molecular motions and forces on which the phenomena of heat depend, and many of the most important steps in the theory are solely to be attributed to his labours. But to return to the theory. This may be defined as including all the reasoning based on two perfectly general experimental laws, without any hypothesis as to the mechanical origin of heat. In this form thermo-dynamics is a purely mathematical subject and unfit for a lecture. But as no one who has studied the subject doubts for a moment the mechanical origin of these laws, I shall be following the spirit, if not the letter of my subject, if I introduce a conception of the mechanical actions from which these laws spring. And this I shall do, although I should hardly have ventured, had it not been that, while considering this lecture, I hit on certain mechanical contrivances which afford sensible examples of the action of heat, in the same way as the pendulum is an example of the same principles as those involved in the phenomena of sound and light. These examples, thanks to the ready aid of Mr Forster in constructing the apparatus, I am in a position to show you, and I am not without hope that these kinetic engines may in a great measure remove the source of obscurity on which I have dwelt.

The general action of heat to cause matter to expand, or to tend to expand, is sufficiently obvious and popular. That the expanding matter will do work is also sufficiently obvious, but the exact part which the heat plays in doing this work is very obscure.

It is now known that heat performs two, and it may well be said three, distinct parts in doing the work. These are—

(1) To supply the energy equivalent to the work done.

(2) To give the matter the elasticity which enables it to expand, *i.e.*, to convert the inert matter into an acting machine.

(3) To convey itself (*i.e.*, heat) in and out of the matter.

This third function is generally taken for granted in the theory of thermo-dynamics.

In order to make any use of thermo-dynamics, a knowledge of the experimental phenomena of heat is necessary; but as time will not permit of my entering largely into these, I have had some of the leading facts suspended as diagrams. One or two it will be well to mention.

Heat as a quantity is independent of temperature, the thermal unit taken being the amount of heat necessary to raise 1 lb. of matter 1° Fahrenheit.

Temperature represents the intensity of heat in matter. Matter in most of its forms expands more or less uniformly as we add heat to it; hence the expansion of matter measures temperature. Gases such as air expand in absolute proportion to the heat added under a constant pressure.

Absolute temperature is an idea derived from the observed rate of contraction of gases; they would vanish to nothing with the temperature 461° below zero Fahrenheit. For the other phenomena I must refer to the diagrams as I proceed.

Our knowledge of these facts has been accumulating during the last two hundred years, and it was in a very complete condition forty years ago, before thermo-dynamics was born. The birth of this science may be considered as the result of the recognition of work—motion against resistance as a true measure of mechanical action, and of accumulated work or energy as the potency of all sources of power. These ideas have now become extremely popular, and all are able to recognise in the raised weight, the bent spring, the moving hammer, the same thing, energy, which is measured by the amount of work which can be derived from any of these sources.

Before the recognition of this means of measuring mechanical potency, any definite idea of the true mechanical action of heat was impossible, for we had not recognised the only mechanical action by which it can be measured.

In 1843 Joule conclusively proved that, by the expenditure of 772 ft.-lbs. a thermal unit of heat must be produced, provided all the work was spent in producing heat. The simplicity of the ideas here involved, and the completeness of Joule's proof, acted at once to render the first law popular. No language can be too strong in which to express the importance of this discovery; yet, as was long ago pointed out by Rankine, the very popularity of Joule's law went a long way to obscure the fact that it did not constitute the sole foundation of the theory of thermo-dynamics. Before Joule's discovery it was recognised that heat acted a part in causing work to be performed. It was clearly seen that it was heat which caused the water to expand into steam, against the resistance of the engine, and the necessity of heat to cause matter to expand was recognised.

To make matter do work it was only necessary to heat it. It would expand, raising a weight; and since after doing its work the matter was still hot, it was supposed that the only necessity for the heat was to add increased elasticity to matter. It was seen that the heat that had once been used was so degraded in temperature that it could not be all used again. So that, although there was no idea that heat was actually consumed in doing the work, it was seen that for continuous work a continuous supply of heat at a high temperature was necessary. As regards the exact proportion of heat required for the supply of elasticity, to perform a certain quantity of work, fairly clear ideas prevailed. It was seen that this depended on various circumstances. These were formulated by Carnot, who in 1828 gave a formula, which is equivalent to our second law of thermo-dynamics, of which it was the parent.

Now this idea that heat merely caused work to be done was not absurd, as is sometimes supposed. Indeed we may say that the present popular idea that the whole heat is convertible into work is more erroneous than the old idea in the ratio of 10 to 1; because the old idea that the function of heat is to supply elasticity was right, as far as it went. Although the present idea that the function of heat is to supply energy from which the work is drawn is also right, yet in any known possible heat-engine ten times more heat is necessary for the purpose of giving elasticity to matter than is converted into work by elasticity. This error, which seems to be very general amongst those who have not made a special study of the subject, may, I think, be attributed—first, to the popularity of the first law of thermo-dynamics, and secondly to the fact that although the second law of thermo-dynamics is nothing more nor less than a statement of the proportion which the quantity of heat necessary to produce elasticity bears to the quantity which this elasticity will convert into work, yet that it is the invariable custom in stating this law to omit all attempt to explain the purpose which this excess of heat serves; the reason for this omission being that experiment only shows that this heat is necessary, and hence this is all that we have a right to say.

If such an error prevails it is only a popular error, for it certainly did not affect the progress of the science. No sooner did Joule's law become known than it was taken up by Rankine, who, in 1849, published a complete theory of thermo-dynamics, based, as I have said, on a hypothetical constitution of matter. This was almost simultaneously followed by theories based on an improved form of Carnot's reasoning by Thomson and Clausius.

Rankine's theory was based on a hypothetical constitution of matter. He invented a system of molecular motions and constraints, which he called molecular vortices, and he then calculated the effects of these motions by the theory of mechanics. The fact that his reasoning was based on a hypothesis was considered by many as a fault in his reasoning. But on the other hand the clear idea thus obtained, as to the reason of everything he was doing, gave him such an advantage over those who were working by experimental laws, of the meaning of which they would venture no opinion, that he was led to make discovery after discovery in advance of his competitors, while some of his discoveries are still beyond the reach of experiment.

There was, however, a difficulty Rankine had to face; some properties of matter were pointed out which his hypothetical matter did not possess. This was not much to be wondered at, for although Rankine had invented machinery which would account for the mechanical action of heat, there was no reason to suppose this to be the only machinery. Rankine, with a view to the difficult calculations he had to make, had chosen machinery as simple as possible. Instead, however, of trying to complicate it, he, yielding to the opinion of his cotemporaries, adopted the general conclusions to which it had led him as axiomatic laws, and so cut himself adrift from his hypothesis.

It comes to be, then, that the student of thermo-dynamics finds as a reason why we must pass a large amount of heat through his engine, besides that which is converted into work, he is to accept an axiomatic law as to the greatest possible amount that can be converted under the circumstances.

To tell a child who asks why he cannot have more food, that he can only have 6 oz. a day, would be considered cruel. So to tell a student who wants to know why, out of the ten million foot-lbs. in 1 lb. of coal, a steam-engine can only give one million as work, that he is only allowed $\frac{T_1 - T_2}{T_1 + 461}$, is cruel, yet this is all he can have from the theory of thermo-dynamics based on its experimental laws.

Rankine, when compelled to abandon his hypothesis as the foundation of his theory by the objections justly urged against it, pointed out the great disadvantage of a mechanical theory conveying no conception of the mechanical basis of its laws; and called on all those who taught the subject, to try and find some popular means of illustrating the second law.

This call was made twenty years ago; but, I believe, up to the present time no such illustration has been forthcoming. When undertaking this lecture I had no idea of such an illustration, and I did not intend to say much as to the reason of the second law. But, as I have said, three weeks ago an idea occurred to me. It arose in this way: Heat acts in matter to transform heat into work by molecular mechanism. Having much studied the subject, I have in my mind a picture, right or wrong, of the mechanism, and the part which heat acts. The question occurred—Is there no way of making a machine such that, although the parts are in visible motion, and the energy transformed to work is visible energy, yet the energy supplied shall have the characteristics of heat-energy, and the machine shall act simply in virtue of the elasticity caused by the motion of its parts?

The question had no sooner arisen than several ways of carrying out the idea presented themselves.

The general idea of the mechanical condition which we call heat is, that the particles of matter are in active motion; but it is the motion of the individuals in a mob, with no common direction or aim. Rankine assumed the motion to be rotatory, but it now appears more probable that the motion in the particles is oscillatory, undulatory, rotatory, and all kinds of motion, whatsoever; so that the communication of heat to matter means the communication of internal agitation—mob agitation. If, then, we are to make a machine to act the part of hot matter, we must make a machine to perform its work in virtue of the communication of internal promiscuous motion amongst its parts. The action of heat-mechanism to do work is simply that of expansion of volume, or the increased effort to expand owing to increased agitation. I first tried to think of some working arrangements of small bodies which should forcibly expand when shaken; but it appeared that it would be much easier to effect a contraction. This was as good. As long as any definite alteration in shape could be produced against resistances by a definite amount of agitation in its parts, we should have a machine illustrating the action of the heat-engine.

Suppose we want to raise a bucket from a well. Our best way is to pull or wind up the rope, but that is because the energy we employ is in a completely directable form. Suppose we had no such directable energy, but could only shake the rope, it having been first made fast at the top (Fig. 1, next page). Then, it being a heavy rope, a chain is better; suppose we shake the chain laterally, waves will run down the chain, and, if we go on shaking, the chain will assume a continuously changing sinuous form (Figs. 2 and 3); and, as the chain does not stretch, the bucket must be raised to allow for the sinuosities. The chain will have changed its mechanical character, and from being a tight line or tie in a vertical direction, will possess kinetic elasticity, that is, elasticity in virtue of its motion, causing it to contract its vertical length.

The bucket will be raised, although not to the top of the well, and work will have been done in raising it, but the work spent in shaking the chain will be not only the equivalent of the work spent in raising the bucket, but also of all the kinetic agitation in the chain necessary to raise the bucket. Having raised the bucket as far as possible with a certain power of agitation, if the supply of agitation be cut off, then that already in the chain will sustain the bucket until it is destroyed by friction, when the bucket will gradually descend.

But if we want to do more work, to raise another bucket, we may take that which is raised off at the level at which it is raised; then, to get the chain down again, we must allow it to cool, *i.e.*, allow the agitation to die out; then, attaching another bucket, to raise this, we shall again have to supply the same heat, perform the same work, *i.e.*, the work to raise the bucket, and the agitation-energy of the chain. Thus we see that the energy necessary to the working of the machine serves two purposes, it supplies the energy necessary to raise the bucket, and the energy necessary to convert the chain from an inextensible tie into an elastic contracting system, capable of raising the weight, neither of which portions of energy is again serviceable after the bucket has been raised. The one portion is already converted into work, and the other, although still in existence in the chain as energy, can only sustain the position of the chain. Before it could be used to do more work it must be got out of the chain and back again, which

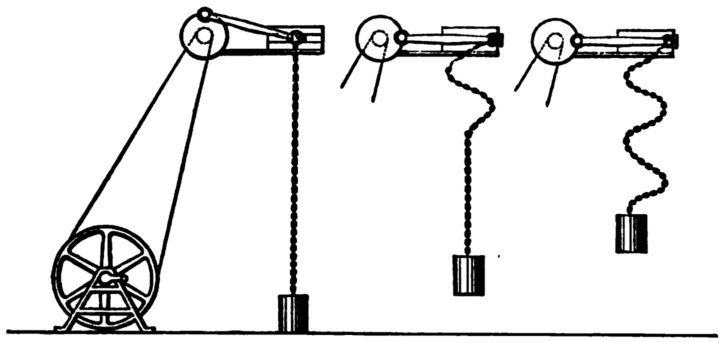


Fig. 1. Fig. 2. Fig. 3.

is just the thing you cannot do; we can get some of it out and some of it back, but not all.

It must not be supposed that this method of raising a bucket by shaking the rope is recommended as the best means. No one would dream of using it if we could get a direct pull, but that is nothing to the point. We are considering the action of heat, and we have limited ourselves to using energy of the same kind that heat supplies; that is, energy in the form of promiscuous agitation, absolutely without direction, so that the question is, how can we raise the bucket by shaking?

I feel that there is a childish simplicity about this illustration, that may at first raise the feeling of "Abana and Pharpar, rivers of Damascus," in the minds of some of my hearers, but, should this be the case, I have every confidence that calm reflection will have the same effect as on Naaman.

The case of the shaken rope, as I have put it, is no mere illustration of the action of heat, but an instance of the same application of the same principles. The sensible energy in the shaking rope only differs from the energy of heat, *i.e.*, a bar of metal is the scale of the motion; we see that in the chain but not in the bar, not because the molecules of the bar are moving slower, but because the scale of motion is infinitely smaller. The temperature of the bar from absolute zero measures the mean square of the velocity of all its parts, multiplied by some constant depending on the mass of the parts which are moving together; so the mean square of the velocity of the chain multiplied by the weight per foot of the chain really represents the absolute temperature of the sensible energy in the chain.

The apparatus which I have on the table is an obvious adaptation of the rope and the bucket. There are three different illustrations apparently very different in form, but all working by the same principle.

Here is the chain (Figs. 1, 2, 3), by the shaking of which (addition of promiscuous energy) a weight of 2 lbs. is raised 3 feet, or 6 foot-lbs. of work done; here is another sort of chain, a series of parallel horizontal bars of wood, connected and suspended by two strings (Figs. 4, 5, and 6). By giving a circular oscillation to the upper bar, the whole apparatus is set into a

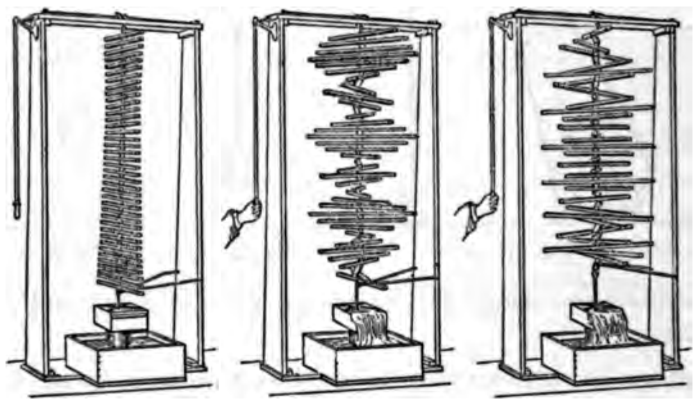


Fig. 4. Fig. 5. Fig. 6.

twisting motion (agitation); the strings are continually bent, and the vertical length of the whole system is shortened, and a weight of 10 lbs. or the bucket of the pump is caused to rise, raising water just as if we boiled water under the piston of a steam-engine. To get the bucket down again for another stroke, we must quiet or cool the chain, just as we must condense the steam, and the energy taken out of the chain in cooling corresponds exactly with the heat that must be taken out of the steam in order to condense it.

The waves of the sea constitute a source of energy in the form of sensible agitation; but this energy cannot be used to work continuously one of these kinetic-machines, for exactly the same reason as the heat in the bodies at the mean temperature of the earth's surface cannot be used to work heat-engines. A chain attached to a ship's mast in a rough sea would become elastic with agitation, but this elasticity could not be used to raise cargo out of the hold, because it would be a constant quantity as long as the roughness of the sea lasted.

In practical mechanics we have no source of energy consisting of sensible agitation, besides the waves of the sea; so that there has been no demand for these kinetic engines to transform sensible mob-energy into work; had there been, I might have patented my idea, though probably it would have long ago been discovered. But there has been a demand for what we may call sensible kinetic elasticity, to perform for sensible motion the part which the heat elasticity performs in the thermometer, and for this purpose the principle of the kinetic machine was long ago applied by Watt. The common governor of a steam-engine acts by kinetic elasticity, which elasticity, depending on the speed at which the governor is driven, enables the governor to contract as the speed increases. The motion of the governor is not of the form of promiscuous agitation, but, though systematic, all the motion is at right angles to the direction of operation, so that the principle of its action is the same.

The kinetic elasticity of the governor performs the same part as the heat elasticity in the matter of the thermometer; the first measures by contraction the velocity of the engine, and the other measures by expansion the velocity of the molecules of the matter by which it is surrounded, so that we now see that while measuring the speed of sensible revolution, we are performing on a different scale the same operation as measuring the temperature of bodies which depends on the molecular velocities, and that quite unconsciously we have constructed instruments to perform the two similar operations which act by means of the same mechanical action, namely, kinetic elasticity.

These kinetic examples of the action of heat must not be expected to simplify the theory, except in so far as they give the mind something definite to grasp; what they do is to substitute something we can see for what we can barely conceive.

The theory of thermo-dynamics can be deduced from any one of these kinetic examples by the application of the principles of mechanics; such application involves complex dynamical reasoning, such as can only be executed by the aid of mathematics, and would be altogether unfit to introduce into a lecture. I shall therefore pass on to some considerations resulting from the theory of thermo-dynamics.

The discovery of the two laws has enabled us to perfect and complete our experimental knowledge of the phenomena of heat. But probably the greatest practical use is that these two laws enable us to calculate with certainty, from the experimental properties of any matter, the extreme potency of any source of power.

Thus we find by experiment that a pound of coal burnt in a furnace yields fourteen to sixteen thousand thermal units of heat. The first law, Joule's law, tells us at once that this is equivalent to from 11,000,000 to 13,000,000 foot-lbs. of energy. But this is not, as seems to be generally supposed, the power of coal. The second law of thermo-dynamics tells us that in order that this energy might be realised, it must be capable of being developed at an infinite temperature, whereas we know that this cannot be the case; and there is a growing idea that the temperature at which coal will burn is not so extremely high, about 3,000° Fahrenheit. Taking this temperature, and assuming the temperature of the atmosphere to be 60°, we have for the proportion of the heat of coal, that we could with a perfect engine call power, $\frac{2940}{3461}$, about 80 per cent., or from 9,000,000 to 11,000,000 foot-lbs.

Again, we know the heat properties of all known liquids and gases, so that we can, by the second law, tell the greatest possible proportion of the heat received, which can be converted into power by any of these agents.

In the steam-engine, for instance, we see that the present limits of art restrict the temperatures absolutely to 400°, and practically the limits are much less; while the lowest temperature that can be worked to in a condenser is 100°. Then, as the limit to the possibility, we have one-third as the greatest proportion, or three out of the nine million foot-lbs.

The greatest actual achievement by Mr Perkins has been about two millions, while the best engines in use only give us a little over one million, or about one-ninth of the possible realizable portion between 3,000° and the mean temperature of the earth's surface.

I cannot here enter upon these, but the reasons why higher temperatures cannot be used in the steam-engine are obvious enough.

The same reasons do not apply to hot air as an agent. This may be worked at much greater temperatures; and about thirty years ago, as soon as it appeared from the science of thermo-dynamics that the limit of efficiency depended on the range of temperature, attention was much directed to air as a substitute for steam. The attempts then made failed through what were then called practical, or art difficulties.

Just at the present time the possibility of other heat-engines than steam-engines has again come to the front; and as this is so, it seems desirable to call attention to a circumstance connected with heat-engines which has as yet occupied quite a subordinate place in the theory of heat-engines. This is the law as to the rate at which heat can be made to do work by an agent, such as steam or air. The greatest possible efficiency of the agent, *i.e.*, the proportion which the work done bears to the mechanical equivalent of the heat spent, is a matter of fundamental importance; but the rapidity with which the heat can be so transformed with a given amount of apparatus, as an engine of a given weight, is a matter of at least as great importance.

Which would be the best engine for a steamboat; one that would develop

20 H.P. for every ton gross weight, consuming 2 lbs. of coal per H.P. per hour, or one that only gave 2 H.P. per ton weight, and only consumed 1 lb. of coal? Unquestionably the former; yet hitherto the question of heat economy has been considered theoretically, to the exclusion of time economy. Yet the latter forms a legitimate part of the subject of thermo-dynamics, and has played a greater part in the selection of steam as the fittest agent than the consideration of the heat-economy.

In the theory of thermo-dynamics it is assumed that the working agent, be it water or any other, can be heated up and cooled down at pleasure, without any consideration as to the time taken for these operations, which are considered to be mere mechanical details.

Yet in the science of heat a great amount of labour has been spent; a great amount of knowledge gained as to the rate at which heat will traverse matter. And more than this; it is well known that heat cannot be made to enter and leave matter without a certain loss of power, *i.e.*, a certain lowering of the working range of temperature. It is by heat that heat is carried into the substance; and hence, as I have indicated, there is a third law of thermo-dynamics relative to this transmission. Heat only flows down the gradient of temperature, and in any particular substance the rate at which heat flows is proportional to the gradient of temperature. Hence to get the heat from the source or furnace into the working substance a certain time must be consumed, and this time diminishes as the difference of temperature of the furnace and the working substance increases.

The examples of the kinetic engines which I have shown you well illustrate this. If we shake the end of a chain, the wriggle passes along the chain at a given speed. It appears that an interval must elapse between the first shaking of the chain and the establishment of sufficient agitation to move the bucket; a further interval before the bucket is completely raised; and further still, another interval must elapse before the chain can be cooled again for another stroke; so that this kinetic engine will only work at a given rate. I can increase this rate by shaking harder, but then I expend more energy in proportion to the work done.

This exactly corresponds with what goes on in the steam-engine, only, owing to the agent water being heated, expanded, and cooled severally in the boiler, cylinder and condenser, the connection is somewhat confused.

But it is clear that for every H.P. something like 15 million foot-pounds of power have to pass from the furnace into the boiler. As out of this 15 we cannot use more than 2 million, the remaining 13 are available for forcing the heat from the products of combustion into the water, and out of the steam into the condensing water, and they are usefully employed for this purpose.

The boilers are made small enough to produce sufficient steam, and this size is determined by the difference of the internal temperature of the gases in the furnace and the water in the boiler, and whatever diminishes this difference would necessarily increase the size of the heating surface, *i.e.*, the weight of the engine. The power which this difference of temperature represents cannot be realised in the steam-engine, so that it is most usefully employed in diminishing the necessary size of the boiler. Still it is an important fact to recognise that our present steam-engines require the expenditure of more than five times as much of the power of the heat (not of the heat) in getting the heat into the working substance as in performing the actual operation. This loss of power does not so much occur in the resistance of the metal which separates the furnace from the water as in the resistance of the gases. Gas is a very bad conductor; and though a thin layer adjacent to the plates is always considerably cooled, little further cooling goes on until, by the internal currents, this layer is removed, and a fresh hot layer substituted in its place.

Similar resistance would occur inside the boiler between the water and the hot plate, nay does occur, until the water begins to boil, but then the evaporation of the water takes place at the hot surface, and every particle of water boiled absorbs a great deal of heat, which leaves the surface in the form of bubbles, allowing fresh water to come up.

If we had air inside the boiler instead of water, we should require from five to ten times the surface to carry off the same heat, which is a sufficient reason why what are called hot-air engines cannot answer, even did not the same argument hold with enormously greater force in the condenser.

Steam is as bad a conductor of heat as air as long as it does not condense, but, in condensing, steam will conduct heat to a cold surface at an almost infinite rate, for as the steam comes up to the surface it is virtually annihilated, leaving room for fresh steam to follow, which it will do if necessary with the velocity of sound. If, however, there is the least incondensable air in the steam this will be left as a layer against the fresh steam. Some years ago I made some experiments on this subject, which showed that 5 or 10 per cent. of air in the steam would virtually prevent condensation.

If a flask be boiled till all the air is out, and nothing but pure steam is left, and if the flask be then closed and a few drops of cold water introduced, the pressure instantly falls to zero, though it immediately recovers from the boiling of the water in the flask. If now a little air be admitted, and allowed to mix with the steam, the few drops of water produce scarcely any effect.

The facility with which steam carries heat to a cold surface is both an enormous advantage and some drawback; as compared with air it is an enormous advantage in enabling the steam to be cooled in the condenser. But during the working of the steam in the cylinder, when the steam is wanted to keep its heat, the facility with which it condenses is a great drawback, and necessitates the keeping of the cylinder hotter than the steam by a steam-jacket. For this part of its work the non-conductivity of incondensable air is a great advantage.

In dwelling thus on the conducting powers of air and steam, my purpose has been to prepare the way for a few remarks I wish to make on another form of heat-engine—the engine in which the heat is generated in the working substance itself.

The combustion-engine, in the form of the cannon, is the oldest form of heat-engine. Here the chemically separate elements in the form of gunpowder are the working substances put into the cylinder; they take in with them the potential energy of chemical separation, which by means of a spark take the kinetic form of heat. Here there is no conduction, the kinetic elasticity propels the shot, and all the heat over and above that used in imparting energy to the shot is lost. The advantages of this form of engine are two. There is no time necessary for conduction, and as the gas generated is not condensable, there is little loss of heat by conduction to the cold metal.

These two advantages are very great, but I should not have mentioned them in reference to guns were it not that there appears to be the dawning of an idea of taming this form of engine so as to substitute it for the steam-engine. To do this it is necessary to introduce coal or coal-gas;—and oxygen in the form of air in place of gunpowder. The thermo-dynamic theory applied to such engines shows that they should possess great advantages over the steam-engine in point of economy. And the considerations I have brought forward as to the loss of the power of heat in the transference of heat from the furnace to the boiler seem to promise such engines an enormous advantage in rate of work, while the substitution of a non-condensable gas for steam in the cylinder seems to get over the art-difficulty of making cylinders to work under high temperatures. We cannot expect any piston to work in a cylinder of over 300° or 400° temperature, but with non-condensing gases the cylinder may be kept cool with little cooling effect on the gases contained in it, even if the temperature of these is 3,000°. This will be the case if the gas in the cylinder is not in a violent state of internal agitation, but it should be remembered that all internal currents much facilitate the conveyance of heat to the walls.

There is one drawback shown by the theory of these engines. The simple expansion of the gases resulting from combustion is not sufficient to cool them to anything like the temperature of 60°, and to get the greatest economy some of the remaining heat should be used to heat the fresh charge. To do this, however, would necessitate the extraction of the heat from one mass of gas to communicate it to another, which would introduce all the difficulties of the boiler, increased by having gas instead of water.

But even wasting this heat, the theory still shows a large margin of economy for such engines over the present performance of steam-engines, a margin which is said to have been already realised in the gas-engine, which is a form of combustion-engine in a high state of efficiency. Now, by means of Dowson gas, Messrs Crossley seem to have obtained 2,000,000 out of the 10,000,000 ft.-lbs. in 1 lb. of coal. Further accomplishment in this direction is a question of art; but while on all other hands science shows impassable barriers not far in advance of the present achievements of art, in this direction thermo-dynamics extended to include the rate of operation shows no known barriers; while the fact that, as gas-engines, this system of combustion heat-engines has already established a footing assures them continual improvement.

In conclusion I would say, by way of caution, that the theory of thermodynamics does not lead to the conclusion, which seems to be generally held by those who have only realised the first law of the science, that the steam-engine is a semi-barbarous machine, wasting more than it uses, very well for those who know no science, but only waiting until those better educated have time to turn their attention to practical matters, and then to give place to something much better. Thermo-dynamics shows us not the faults but the perfections of the steam-engine, in which there is no waste of power, since all is used either in doing work or in promoting the rate at which the work can be done. Next to the watch, the steam-engine is the highest development of mechanical art, and the science of thermo-dynamics may be said to be the result of the study of the steam-engine.

48.

ON THE TWO MANNERS OF MOTION OF WATER.

[*From the* "Proceedings of the Royal Institution of Great Britain," 1884.]

(*Read* March 28, 1884.)

IT has long been a matter of very general regret with those who are interested in natural philosophy, that in spite of the most strenuous efforts of the ablest mathematicians, the theory of fluid motion fits very ill with the actual behaviour of fluids; and this for unexplained reasons. The theory itself appears to be very tolerably complete, and affords the means of calculating the results to be expected in almost every case of fluid motion, but while in many cases the theoretical results agree with those actually obtained, in other cases they are altogether different.

If we take a small body such as a raindrop moving through the air, the theory gives us the true law of resistance; but if we take a large body such as a ship moving through the water, the theoretical law of resistance is altogether out. And what is the most unsatisfactory part of the matter is that the theory affords no clue to the reason why it should apply to the one class more than the other.

When, seven years ago, I had the honour of lecturing in this room on the then novel subject of vortex motion, I ventured to insist that the reason why such ill success had attended our theoretical efforts was because, owing to the uniform clearness or opacity of water and air, we can see nothing of the internal motion; and while exhibiting the phenomena of vortex rings in water, rendered strikingly apparent by partially colouring the water, but otherwise as strikingly invisible, I ventured to predict that the more general application of this method, which I may call the method of colour-bands, would reveal clues to those mysteries of fluid motion which had baffled philosophy.

To-night I venture to claim what is at all events a partial verification of that prediction. The fact that we can see as far into fluids as into solids naturally raises the question why the same success should not have been obtained in the case of the theory of fluids as in that of solids? The answer is plain enough. As a rule, there is no internal motion in solid bodies; and hence our theory based on the assumption of relative internal rest applies to all cases. It is not, however, impossible that an, at all events seemingly, solid body should have internal motion, and a simple experiment will show that if a class of such bodies existed they would apparently have disobeyed the laws of motion.

These two wooden cubes are apparently just alike, each has a string tied to it. Now, if a ball is suspended by a string you all know that it hangs vertically below the point of suspension or swings like a pendulum. You see this one does so. The other you see behaves quite differently, turning up sideways. The effect is very striking so long as you do not know the cause. There is a heavy revolving wheel inside which makes it behave like a top.

Now what I wish you to see is, that had such bodies been a work of nature so that we could not see what was going on—if, for instance, apples were of this nature while pears were what they are—the laws of motion would not have been discovered; if discovered for pears they would not have applied to apples, and so would hardly have been thought satisfactory.

Such is the case with fluids: here are two vessels of water which appear exactly similar—even more so than the solids, because you can see right through them—and there is nothing unreasonable in supposing that the same laws of motion would apply to both vessels. The application of the method of colour-bands, however, reveals a secret: the water of the one is at rest, while that in the other is in a high state of agitation.

I am speaking of the two manners of motion of water—not because there are only two motions possible; looked at by their general appearance the motions of water are infinite in number; but what it is my object to make clear to-night is that all the various phenomena of moving water may be divided into two broadly distinct classes, not according to what with uniform fluids are their apparent motions, but according to the internal motions of the fluids, which are invisible with clear fluids, but which become visible with colour-bands.

The phenomena to be shown will, I hope, have some interest in themselves, but their intrinsic interest is as nothing compared to their philosophical interest. On this, however, I can but slightly touch.

I have already pointed out that the problems of fluid-motion may be divided into two classes: those in which the theoretical results agree with the experimental, and those in which they are altogether different. Now what makes the recognition of the two manners of internal motion of fluids so important, is that all those problems to which the theory fits belong to the one class of internal motions.

The point before us to-night is simple enough, and may be well expressed by analogy. Most of us have more or less familiarity with the motion of troops, and we can well understand that there exists a science of military tactics which treats of the best manœuvres and evolutions to meet particular circumstances.

Suppose this science proceeds on the assumption that the discipline of the troops is perfect, and hence takes no account of such moral effects as may be produced by the presence of an enemy.

Such a theory would stand in the same relation to the movements of troops, as that of hydrodynamics does to the movements of water. For although only the disciplined motion is recognised in military tactics, troops have another manner of motion when anything disturbs their order. And this is precisely how it is with water: it will move in a perfectly direct disciplined manner under some circumstances, while under others it becomes a mass of eddies and cross streams, which may be well likened to the motion of a whirling, struggling mob where each individual particle is obstructing the others.

Nor does the analogy end here: the circumstances which determine whether the motion of troops shall be a march or a scramble, are closely analogous to those which determine whether the motion of water shall be direct or sinuous.

In both cases there is a certain influence necessary for order: with troops it is discipline; with water it is viscosity or treacliness.

The better the discipline of the troops, or the more treacly the fluid, the less likely is steady motion to be disturbed under any circumstances. On the other hand, speed and size are in both cases influences conducive to unsteadiness. The larger the army, and the more rapid the evolutions, the greater the chance of disorder; so with fluid, the larger the channel, and the greater the velocity, the more chance of eddies.

With troops some evolutions are much more difficult to effect with steadiness than others, and some evolutions which would be perfectly safe on parade, would be sheer madness in the presence of an enemy. So it is with water.

One of my chief objects in introducing this analogy of the troops is to emphasise the fact, that even while executing manœuvres in a steady manner, there may be a fundamental difference in the condition of the fluid. This is easily realised in the case of troops. Difficult and easy manœuvres may be executed in equally steady manners if all goes well, but the conditions of the moving troops are essentially different. For while in the one case any slight disarrangement would be easily rectified, in the other it would inevitably lead to a scramble. The source of such a change in the manner of motion under such circumstances, may be ascribed either to the delicacy of the manœuvre, or to the upsetting disturbance, but as a matter of fact, both of these causes are necessary. In the case of extreme delicacy an indefinitely small disturbance, such as is always to be counted on, will effect the change.

Under these circumstances we may well describe the condition of the troops in the simple manœuvre as stable, while that in the delicate manœuvre is unstable, *i.e.* will break down on the smallest disarrangement. The small disarrangement is the immediate source of the break-down in the same sense as the sound of a voice is sometimes the cause of an avalanche; but if we regard such disarrangement as certain to occur, then the source of the disturbance is a condition of instability.

All this is exactly true for the motion of water. Supposing no disarrangement, the water would move in the manner indicated in theory just as, if there is no disturbance, an egg will stand on its end; but as there is always slight disturbance, it is only when the condition of steady motion is more or less stable that it can exist. In addition then to the theories either of military tactics or of hydrodynamics, it is necessary to know under what circumstances the manœuvres of which they treat are stable or unstable. And it is in definitely separating these conditions that the method of colour-bands has done good service which will remove the discredit in which the theory of hydrodynamics has been held.

In the first place, it has shown that the property of viscosity or treacliness, possessed more or less by all fluids, is the general influence conclusive to steadiness, while, on the other hand, space and velocity are the counter influence; and the effect of these influences is subject to one perfectly definite law, which is that a particular evolution becomes unstable for a definite value of the viscosity divided by the product of the velocity and space. This law explains a vast number of phenomena which have hitherto appeared paradoxical. One general conclusion is, that with sufficiently slow motion all manners of motion are stable.

The effect of viscosity is well shown by introducing a band of coloured water across a beaker filled with clear water at rest. Now the water is quite still, I turn the beaker round about its axis. The glass turns but not the water, except that which is close to the glass. The coloured water which is close to the glass is drawn out into what looks like a long smear, but it is not a smear, it is simply a colour-band extending from the point in which the colour touched the glass in a spiral manner inwards, showing that the viscosity was slowly communicating the motion of the glass to the water within. To prove this I have only to turn the beaker back, and the colour-band assumes its radial position. Throughout this evolution the motion has been quite steady—quite according to the theory.

When water flows steadily it flows in streams. Water flowing along a pipe is such a stream bounded by the solid surface of the pipe, but if the water be flowing steadily we can imagine the water to be divided by ideal tubes into a fagot of indefinitely small streams, any of which may be coloured without altering its motion, just as one column of infantry may be distinguished from another by colour.

If there is internal motion, it is clear that we cannot consider the whole stream bounded by the pipe as a fagot of elementary streams, as the water is continually crossing the pipe from one side to the other, any more than we can distinguish the streaks of colour in a human stream in the corridor of a theatre.

Solid walls are not necessary to form a stream: the jet from a fire hose, the falls of Niagara, are streams bounded by a free surface.

A river is a stream half bounded by a solid surface.

Streams may be parallel, as in a pipe; converging, as in a conical mouthpiece; or when the motion is reversed, diverging. Moreover, the streams may be straight or curved.

All these circumstances have their influence on stability in a manner which is indicated in the accompanying table:—

Circumstances conducive to

Direct or Steady Motion.	*Sinuous or Unsteady Motion.*
1. Viscosity or fluid friction which continually destroys disturbances. (Treacle is steadier than water.)	5. Particular variation of velocity across the stream, as when a stream flows through still water.
2. A free surface.	6. Solid bounding walls.
3. Converging solid boundaries.	7. Diverging solid boundaries.
4. Curvature with the velocity greatest on the outside.	8. Curvature with the velocity greatest on the inside.

It has for a long time been noticed that a stream of fluid through fluid otherwise at rest is in an unstable condition. It is this instability which gives rise to the talking-flame and sensitive-jet with which you have been long familiar in this room. I have here a glass vessel of clear water in front of the lantern, so that any colour-bands will be projected on the screen.

You see the ends of two vertical tubes one above the other. Nothing is flowing through these tubes, and the water in the vessel is at rest. I now open two taps, so as to allow a steady stream of coloured water to enter at the lower pipe, water flowing out at the upper. The water enters quite steadily, forms a sort of vortex ring at the end which proceeds across the vessel, and passes out at the lower tube. Now the coloured stream extends straight across the vessel, and fills both pipes. You see no motion; it looks like a glass rod. The water is, however, flowing slowly along it. The motion is so slow, that the viscosity is paramount, and hence the stream is steady.

I increase the speed; you see a certain wriggling sinuous action in the column; faster, the column breaks up into beautiful and well-defined eddies, and spreads out into the surrounding water, which, becoming opaque with colour, gradually draws a veil over the experiment.

The same is true of all streams bounded by standing water. If the motion is sufficiently slow, according to the size of the stream and the viscosity of the fluid, it is steady and stable. At a certain critical velocity, which is determined by the ratio of the viscosity to the diameter of the stream, the stream becomes unstable. Under any conditions, then, which involve a stream flowing through surrounding water, the motion will be unstable if the velocity is sufficient.

Now, *one* of the most marked facts relating to experimental hydrodynamics is the difference in the way in which water flows along contracting and expanding channels; these include an enormously large class of the motions of water, but a typical phenomenon is shown by the simple conical tubes. Such a tube is now projected on the screen; it is surrounded with clear still water. The mouth of the tube at which the water enters is the largest part, and it contracts uniformly for some way down the channel, then the tube expands again gradually until it is nearly as large as at the mouth, and then again contracts to the tube necessary to discharge the water. I draw water through the tube, but you see nothing as to what is going on. I now colour one of the elementary streams outside the mouth; this colour-band is drawn in with the surrounding water, and will show us what is going on. It enters quite steadily, preserving its clear streak-like character until it has reached the neck where convergence ceases; now the moment it enters the expanding tube it is altogether broken up into eddies. Thus the motion is direct in the contracting tube, sinuous in the expanding.

The hydrodynamical theory affords no clue to the cause why; and even by the method of colour-bands the reason for the sinuosity is not at once obvious. If we start the current suddenly, the motion is at first the same in both tubes, its change in the expanding pipe seemed to imply that here the motion was unstable. If so, this ought to appear from the equations of motion. With this view this case was studied, I am ashamed to say how long, without any light. I then had recourse to the colour-bands again, to try and see how the phenomena came on. It all then became clear: there is an intermediate stage. When the tap is opened, the immediately ensuing motion is nearly the same in both parts; but while that in the contracting portion maintains its character, that in the expanding portion changes its character. A vortex ring is formed which, moving forward, leaves the motion behind that of a parallel stream through the surrounding water.

If the motion be sufficiently slow, as it is now, this stream is stable, as already explained. We thus have steady or direct motion in both the contracting and expanding parts of the tube, but the two motions are not similar: the first being one of a fagot of similar elementary contracting streams, the latter being that of one parallel stream through the surrounding fluid. The first of these is a stable form; the second an unstable form, and, on increasing the velocity, the first remains, while the second breaks down; and we have, as before, the expanding part filled with eddies.

This experiment is typical of a large class of motions. Wherever fluid flows through a narrow, as it approaches the neck it is steady, after passing, it is sinuous. The same effect is produced by an obstacle in the middle of a stream; and very nearly the same thing by the motion of a solid object through the water.

You see projected on the screen an object not unlike a ship. Here the ship is fixed, and the water flowing past it; but the effect would be the same if we had the ship moving through the water. In the front of the ship the stream is steady, and so till it has passed the middle, then you see the eddies formed behind the ship. It is these eddies which account for the discrepancy between the actual and theoretical resistance of ships. We see, then, that the motion in the expanding channel is sinuous because the only steady motion is that of a stream through water. Numerous cases in which the motion is sinuous may be explained in the same way, but not all.

If we have a perfectly parallel channel, neither contracting nor expanding, the steady moving stream will be a fagot of perfectly steady parallel elementary streams all in motion, but moving fastest at the centre. Here we have no stream through steady water. Now when this investigation began it was not known, or imperfectly known, whether such a stream was stable or not, but there was a well-known anomaly in the resistance to motion in parallel channels. In rivers, and all pipes of sensible size, experience had shown that the resistance increased as the square of the velocity, whereas in very small pipes, such as represent the smaller veins in animals, Poiseuille had proved the resistance increased as the velocity.

Now since the resistance would be as the square of the velocity with sinuous motion, and as the velocity, if direct, it seemed that the discrepancy could be accounted for if the motion could be shown to become unstable for a sufficiently large velocity. This suggested the experiment I am now about to produce before you.

You see on the screen a pipe with its end open. It is surrounded by clear water, and by opening a tap I can draw water through it. This makes no difference to the appearance, until I colour one of the elementary streams, when you see a beautiful streak of colour extend all along the pipe. The stream has so far been running steadily, and appears quite stable. I now merely increase the speed; it is still steady, but the colour-band is drawn down fine. I increase the colour and then again increase the speed. Now you see the colour-band at first vibrates and then mixes so as to fill the tube. This is at a definite velocity; if the velocity be diminished ever so little the band becomes straight and clear; increase it again, it breaks up. This critical speed depends on the size of the tube in the exact inverse ratio; the smaller the tube, the greater the velocity; also, the more viscous the water the greater the velocity.

We have then not only a complete explanation of the difference in the laws of resistance generally experienced, and that found by Poiseuille, but also we have complete evidence of the instability of parallel streams flowing between or over solid surfaces. The cause of the instability is as yet not explained, but this much can be shown, that whereas lateral stiffness in the walls is unimportant, inextensibility or tangential rigidity is essential to the creation of eddies. I cannot show you this, because the only way in which we can produce the necessary conditions without a solid channel, is by a wind blowing over water. When the wind blows over water, it imparts motion to the surface of the water just as a moving solid surface; moving in this way, however, the water is not susceptible of eddies. It is unstable, but the result of disturbance is waves. This is proved by an experiment long known, but which has recently attracted considerable notice. If oil be put on the surface it spreads out into an indefinitely thin sheet which possesses only one of the characteristics of a solid surface, it offers resistance, very slight, but still resistance to extension and contraction. This, however, is sufficient to entirely alter the character of the motion. It renders the water unstable internally, and instead of waves, what the wind does is to produce eddies beneath the surface. This has been proved, although I cannot show you the experiments.

To those who have observed the phenomena of oil preventing waves, there is probably nothing more striking throughout the region of mechanics. A film of oil so thin that we have no means of illustrating its thickness, and which cannot be perceived except by its effect—which possesses no mechanical properties that can be made apparent to our senses—is yet able to entirely prevent an action which involves forces the strongest we can conceive, which upset our ships and destroy our coasts. This, however, becomes intelligible when we perceive that the action of the oil is not to calm the sea by sheer force, but merely, as by its moral force, to alter the manner of motion produced by the action of the wind, from that of the terrible waves upon the surface, into the harmless eddies below. The wind throws the water into a highly unstable condition, into what morally we should call a condition of great excitement. The oil by an influence we cannot perceive directs this excitement.

This influence, though insensibly small, is however now proved of a mechanical kind, and to me it seems that the phenomenon of one of the most powerful mechanical actions of which the forces of nature are capable, being entirely controlled by a mechanical force so slight as to be otherwise quite imperceptible, does away with every argument against the strictly mechanical sources of what we may call mental and moral forces.

But to return to the instability in parallel channels. This has been the most complete, as well as the most definite result of the colour-bands.

The circumstances are such as to render definite experiments possible. These have been made, and reveal a definite law of the instability, which law has been tested by reference to all the numerous and important experiments on the resistance in channels by previous observers; whereupon it is found that waters behave in exactly the same manner whether the channel, as in Poiseuille's experiment, is of the dimensions of a hair, or whether it be the size of a water main or of the Mississippi; the only difference being that in order that the motions may be compared, the velocity must be inversely as the diameter of the pipe. But this is not the only point explained if we consider other fluids than water. Some fluids, like oil or treacle, apparently flow more slowly and steadily than water. This, however, is only in smaller channels; the critical velocity increases with the viscosity of the fluid. Thus, while water in comparatively large streams is always above its critical velocity, and the motion always sinuous, the motion of treacle in streams of such size as we see is below its critical velocity, and the motion direct.

But if nature had produced rivers of treacle the size of the Thames, for instance, the treacle would have flowed just like water. Thus, in the lava streams from a volcano, although looked at close the lava has the consistence of a pudding, in the large and rapid streams down the mountain sides the lava flows as freely as water.

I have now only one circumstance left to which to ask your attention. This is the effect of the curvature of the stream on the stability of the fluid.

Here again we see the whole effect altered by very slight causes.

If water be flowing in a bent channel in steady streams, the question as to whether it will be stable or not turns on the variation in the velocity from the inside to the outside of the stream.

In front of the lantern is a cylinder with glass ends, so that the light passes through in the direction of the axis. The disk of light on the screen being the light which passes through this water, and is bounded by the circular walls of the cylinder.

By means of two tubes temporarily attached, a stream of coloured water is introduced right across the cylinder extending from wall to wall; the motion is very slow, and the taps being closed, and the tubes removed, the colour-band is practically stationary. The vessel is now caused to revolve about its axis. At first, only the walls of the cylinder move, but the colour-band shows that the water gradually takes up the motion, the streak being wound off at the ends into a spiral thread, but otherwise remaining still and vertical. When the spirals meet in the middle, the whole water is in motion, but the motion is greatest at the outside, and is therefore stable. The vessel stops, and gradually stops the water, beginning at the outside. If the motion remained steady, the spirals would unwind, and the streak be restored. But the motion being slowest at the outside against the surface, you see eddies form, breaking up the spirals for a certain distance towards the middle, but leaving the middle revolving steadily.

Besides indicating the effect of curvature, this experiment really illustrates the action of the surface of the earth on the air moving over it; the varying temperature having much the same influence as the curvature of the vessel on stability. The air is unstable for a few thousand feet above the surface, and the motion is sinuous, resulting in the mixing of the strata, and producing the heavy cumulus clouds; but above this the influence of temperature predominates, and clouds, if there are any, are of the stratus-form, like the inner spirals of colour. But it was not the intention of this lecture to trace the two manners of motion of fluids in the phenomena of Nature and Art, so I thank you for your attention.

49.

ON THE THEORY OF THE STEAM-ENGINE INDICATOR*.

[*From the* "Proceedings of the Institution of Civil Engineers, 1885."]

"ON THE THEORY OF THE STEAM-ENGINE INDICATOR AND THE ERRORS IN INDICATOR-DIAGRAMS."

By OSBORNE REYNOLDS, M.A., LL.D., F.R.S., M. Inst. C.E.

SECTION I.—INTRODUCTION.

IN 1856 Hirn published an experimental comparison of the indicated work, with the work done on the brake, and came to the conclusion that, whatever might be the cause, the indicated work was too small, being only just equal to the brake-work, leaving no margin for the air-pump and the friction of the engine.

This conclusion of Hirn's seems to have excited little notice. Rankine mentions it in "The Steam-engine," but expresses doubt whether it accords with subsequent experience, particularly that of marine engines.

Since that time many engine-experiments have been made. It does not appear, however, that these have been made with a view to verify the indicator, but rather that the indicator-diagrams have been taken as data from which to determine the efficiency of the engines; nor has, so far as the Author is aware, any definite theory of the disturbances to which the diagram is subjected as yet been published.

The importance of studying the disturbances, or, in other words, the errors in the diagrams, becomes evident, when it is considered to what an

* Joint paper with A. W. Brightmore, D.Sc.

extreme extent the indicator is now trusted to give a true measure of the work on the piston. In ninety-nine cases out of every hundred, there is absolutely no check within 20 or 30 per cent. In some classes of engines (winding and pumping) the work they are performing is of a measurable kind, but rarely or never is the work measurable to within 5 or 10 per cent. The only work which is definitely measurable is that done on the friction-brake as used by the Royal Agricultural Society; and even then, although the brake may give a measure of the actual work to within 1 per cent. or less, it does not furnish a check on the indicator to within from 5 to 20 per cent., for between the work measured by the indicator and that measured by the brake, is the unknown work done in overcoming the resistance of the engine. This, which varies from 5 to 20 per cent., is an absolutely unknown quantity, except in so far as it is found by subtracting the brake-power from the indicated-power, and hence furnishes no check within its own magnitude on these quantities.

There is thus absolutely no check on the indicator, which is now made the sole standard, not only of the performance, but of the value of engines. Considering what this means in mere money, where, as in the case of marine engines, large sums often depend on a margin of power which is a very small percentage of the whole, it becomes evident how important it is that the exact extent to which these instruments can be trusted should be well known. Yet, in spite of Hirn's warning, the results of the indicator appear to be accepted without question, solely on the ground of their general consistency, of the simplicity of its apparent action, and the excellence of its construction.

On close examination, it appears in this case, as in others, that the apparent simplicity of action is due to the obscurity of certain facts; for example, the possible stretching of a piece of string; and that, taking all the circumstances which may affect the diagram into account, its action is by no means a simple matter. It may be that, in some cases, these disregarded circumstances only produce an inappreciable effect, but even this cannot be known as long as they are disregarded.

The theory of the indicator has now been taught for many years in the engineering classes in Owens College, Manchester, and the calculations to a certain extent have been verified by experiments on the College engine. This engine, though by no means of a high class, has been rendered well adapted for this purpose by the addition of a brake-dynamometer and a speed-indicator. It has long been the Author's intention to publish this theory, but this has been deferred for want of time to make a sufficiently extensive series of experiments. Last year Mr Brightmore, Berkeley Fellow in Owens College, Manchester, undertook the experiments and carried them out very successfully. The results of his investigation appear to be of considerable importance, and as their interpretation depends on the theory, an account of this is submitted, to be read in conjunction with a Paper by Mr Brightmore.

For the diagram to be exact, it is necessary—

1. That the pencil of the indicator shall, under every change of pressure, instantly move through a distance in exact proportion to the change of pressure in the cylinder of the engine.

2. That the paper on which the diagram is taken shall change its position in exact accordance with the change of position of the piston of the engine.

The first of these is accomplished, so far as it is accomplished, by holding the piston of the indicator by a spring, carefully adjusted, so that the deflection is proportional to the load; and as there is no great difficulty in making a spring such that this proportion shall be maintained so long as the temperature is constant, and in making the instrument so that the temperature of the spring shall be 212° Fahrenheit, there is no reason to suppose that the indications of the indicator are not within 1 per cent. of the forces at each instant deflecting the spring.

But in order that these indications may correspond with the pressures of steam, it is necessary that there should be no other forces acting on the spring. Such forces, however, arise from the inertia of the weights to be moved and the friction, notably that entailed by the necessity of pressing the pencil on the paper.

In assuming the indicator as accurate, it is supposed that the forces resulting from inertia and friction are too small to be perceived; whether this is so or not, can only be ascertained by considering these forces.

The second of these conditions of exactness is accomplished by connecting a revolving drum, by means of mechanism, with the piston of the engine, so that, if there is no yielding in the mechanism, the drum will revolve through distances exactly proportional to the distance moved by the piston of the engine. There is no difficulty in arranging mechanism which will secure the corresponding motion of two bodies, if the forces can be kept constant on the mechanism. This is attempted in the indicator by pulling the drum in one direction by a spring, and connecting it with the piston by means of a cord wound round the drum, so that the spring always keeps the string in tension. Since all strings—in fact, all matter—is elastic, in order that the position of the drum may always correspond with the position of the engine-piston, it is necessary that the spring shall exert a constant force in all positions of the drum, and that there shall be no other forces.

As a matter of fact, however, the springs used do not exert a constant force, the force increasing as the drum is moved against the spring; and further, there are forces, namely, the forces arising from the inertia of the drum and the friction of the mechanism, principally of the drum on its supports. The diagram will, therefore, only be accurate in so far as these unequal forces are small; and the effect of these forces can only be ascertained after careful consideration.

It thus appears that there are five principal causes of disturbance; two of these (1) and (2) affect the motion of the pencil, and three (3) (4) and (5) the motion of the drum.

(1) The inertia of the piston of the indicator and its attached weights.

(2) The friction of the pencil on the paper and its attached mechanism.

(3) Varying action of the spring.

(4) Inertia of the drum.

(5) Friction of the drum.

These will be separately considered.

Section II.—Disturbances on the Pencil.

(1) *The effect of the inertia of the Pencil and its attached Mechanism.*—This, although obvious enough in a general way, presents the same problem as the planetary disturbances, which can only be definitely expressed by means of some form of mathematics. As the general solution of the problem is well known to mathematicians, and is unintelligible to those who are not, it will be best here to omit all the steps, and to proceed at once to the results, about which there can be no question.

These results may be best expressed in symbols, of which the meaning is as follows; taking lbs., feet, and seconds as general units, then put—

i for the indicated pressure at any instant;

p for the actual pressure corresponding to i;

w for the weight of any particular piece of mechanism attached to the pencil;

r for the ratio which the motion of this weight bears to the motion of the piston of the indicator;

W for $\Sigma(r^2 w)$ where Σ expresses the sum of all the quantities in the brackets;

g for 32·2, the acceleration of gravitation;

e for the number of lbs. to the inch on the diagram;

a for the area of the piston of the indicator in square inches;

s for the ratio of the motion of the pencil to that of the piston of the indicator.

In Richards' indicator—

$$a = 0{\cdot}5;$$
$$W = 0{\cdot}33;$$
$$s = 4.$$

For other indicators these may be found by measurement.

The relation between i and p, in so far as it is affected by inertia, is expressed by the equation—

$$\frac{W}{12esg} \cdot \frac{d^2 i}{dt^2} + ai = ap \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1).$$

The general solution to this equation is well known, and without going into detail, it will be sufficient to give the solution for the case, which is, N being the number of revolutions of the engine per minute—

$$i - p = (p_1 - p_3)\frac{\pi N^2 W}{900 \times 12aesg}\left\{A_1 \sin\frac{\pi N}{30}t + A_2 \sin\frac{2\pi N}{30}t + \&c.\right\} + C \sin\sqrt{\frac{12aesg}{W}}\,.\,t \ldots\ldots(2).$$

t expresses time in seconds;

p_1 greatest pressure;

p_3 least back pressure;

A_1, A_2 are coefficients depending on the shape of the true diagram;

C is a constant depending on the disturbed state of the pencil.

From equation (2) it appears that the effect of inertia is to cause two disturbances, corresponding to the two terms on the right-hand side. These may be considered separately.

The first term has the factor

$$A_1 \sin\frac{2\pi N}{60}t + A_2 \sin\frac{4\pi N}{60}t + \&c.,$$

which will go through a complete cycle when t changes by

$$\frac{60}{N},$$

that is, by the time of revolution of the engine in seconds. This disturbance will be the same during each revolution of the engine, and will be called the cyclic disturbance.

Given the shape of the true diagram, it would be possible to determine A_1, A_2 so as to find from equation (2) the value of $i-p$. But this would be a very complicated piece of work for such an irregular curve as the diagram, and as the object is not so much to find the magnitude as to find when this is small, it is sufficient to consider a circular or elliptic diagram; for such a diagram it is found that the mean difference of i and p, written $\overline{i-p}$, is given by

$$\overline{i-p} = \pm \frac{\pi^2 N^2 W}{12 \times 900 ages} \qquad \text{(3)},$$

the positive sign to be taken for the forward stroke and the negative for the backward.

If this effect were large compared with the mean acting pressure $\frac{p-p_1}{2}$, then in all probability the area as well as the form of a true diagram would be seriously disturbed; but if this effect is small, say 1 per cent. in the case of the oval, it will be small for the true diagram. Hence the increase of area is less than 1 per cent. so long as

$$\frac{2\pi^2 N^2 W}{12 \times 900 aesg} < 0{\cdot}01,$$

and from this it is found that the cyclic disturbance may be 1 per cent. for Richards' indicator when N and e have the values in Table I., and as this disturbance increases as $\frac{N^2}{e}$, its possible values for all other cases may be found.

TABLE I.—ENGINE SPEEDS AT WHICH THE ENLARGEMENT OF THE DIAGRAM BY INERTIA BECOMES 1 PER CENT. WITH THE RICHARDS' INDICATOR USED IN THIS INVESTIGATION.

Scale of Diagram in lbs. to an inch.	Number of Revolutions.
20	166
30	203
40	237
50	262
60	288
70	312
80	332
90	352
100	371

In the case of the oval or circular diagram the effect of this cyclic disturbance would be to increase the vertical diameter, as shown by the dotted line in Fig. 1. What it would be on the true diagram is very difficult to express, except to say that it would be to round-off all corners and increase its size much in the same way as in the oval.

The second term in equation (2) represents a disturbance which goes through its cycle in an interval of τ seconds, where

$$\tau = 2\pi\sqrt{\frac{W}{12aesg}} \qquad \text{.............................(4).}$$

This may be called the vibratory disturbance. The period represented by τ is that in which the pencil vibrates when disturbed. Such disturbances are introduced by the departure of the diagram from the true ellipse.

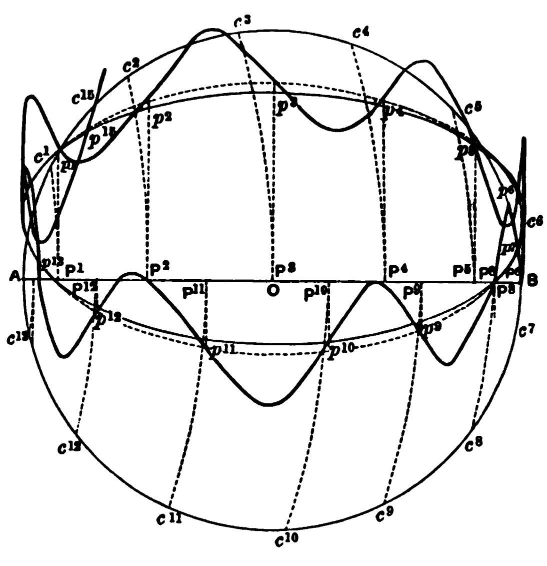


Fig. 1.

The result of such disturbance is shown by the waving line in Fig. 1.

The time occupied in completing each one of these waves as from p_1 to p_2 is constant, viz. τ equation (4).

Hence the number of waves in a complete revolution is given by

$$n = \frac{\frac{60}{N}}{2\pi\sqrt{\frac{W}{12aesg}}} \qquad \text{.............................(5).}$$

For Richards' indicator—

$$n = 486 \frac{\sqrt{e}}{N}.$$

In the diagram, owing to the unequal motion of the engine-piston, the lengths of these oscillations increase from the ends to the middle. If, however, a circle be drawn on the atmospheric line AB, having the extreme length of the diagram as diameter, this may be taken to represent the crank-circle on the same scale as AB represents the stroke. Then if the points p_1, p_2 &c., in which the waving line cuts the mean line, are first projected perpendicularly on to AB in P_1, P_2 &c., and then P_1, P_2 projected by means of a radius to represent the connecting-rod on to the crank-circle in the points c_1, c_2 &c., it will be found that the arcs c_1c_2, c_2c_3, are all equal, since the crank turns through equal arcs in equal times.

But for the effects of friction these oscillations, once set up, would go on for ever; so that even at low speeds a fair diagram would be impossible.

By friction the oscillations are gradually destroyed, so that they are more or less localized to the neighbourhood of the points at which they are produced, *i.e.*, the points where the curvature in the true diagram is sharp, particularly at the point of admission where the rise of pressure being instantaneous acts the part of a live-load, and forces the pencil twice as far as it ought to go. This sets up a series of oscillations.

It is seldom that the time of oscillation is exactly commensurable with that of revolution, so that if all the oscillations set up in one revolution are not destroyed by friction before the revolution is complete, the pencil will not describe the same path in two successive revolutions, a fact frequently observed in diagrams taken from locomotives at high speed.

The error which these oscillations cause in the area of the diagram depends on their magnitude, but also, and to a greater extent, on the smallness of n, the number in a revolution. But the evil of these oscillations is not so much an effect on the area, which, even did they exist to the extent shown in Fig. 1, in which n is between six and seven, would still be small. It is the disfigurement and the confusion they produce in the diagram which limits the usefulness of the instrument to cases in which they can be avoided.

So long as there are thirty of these oscillations in a cycle the necessary fluid friction of the indicator-piston will so far reduce them as to render a fair diagram possible, but when the number approaches fifteen it becomes necessary to call in the aid of considerable pencil-pressure to prevent their destroying the form of the diagram; and when n is as low as ten it is all the pencil will do to prevent them upsetting the diagram. The Author has never been able to produce a respectable diagram when the number is as low as ten, but accounts are continually published in which from the speed of the engine and strength of the springs the value of n must be below this. In such cases the pressure of the pencil must have been very great, and it becomes a question how far this cure is a less evil than the disease.

(2) *The Friction arising from the Pressure of the Pencil.*—This always acts to oppose the motion of the pencil, and therefore renders it too large during expansion and exhaust, and too small during compression and admission, and thus the general effect is to increase the size of the diagram.

In order to understand this effect, it is necessary to notice that this friction consists of two parts: (1) That of the pencil on the paper. (2) That of the mechanism, caused by sustaining the pressure of the pencil.

The effect of the actual friction of the pencil is greatly reduced by the motion of the paper. Thus, if while the drum is at rest, the pencil be lifted quietly it will be possible for friction to hold it above or below the atmospheric-line, by a distance depending on the pressure. If, when placed as high or low as it will stand, the drum be moved by the cord, the pencil at once approaches the atmospheric-line, describing a line as shown in Fig. 2

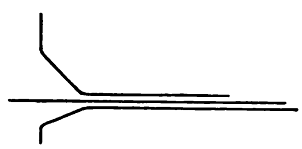


Fig. 2.

at first sloping toward the atmospheric-line at 45°, but finally becoming parallel. Fig. 2 represents the results with a 20-lb. spring; the distance at starting was equal to about 4 lbs., but eventually became about $\frac{1}{2}$ lb., at which it remained constant.

The distance at starting represents the extreme friction of pencil and mechanism. The final distance that of the mechanism alone.

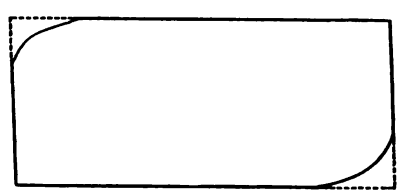


Fig. 3.

These effects on the diagram are different. That of the pencil causes the pencil to be behind its true position, by a quantity which will bear to the extreme distance, a ratio equal to the sine of the inclination of the curve it is describing at the instant, to the atmospheric-line.

The effect of this alone on a rectangular diagram would be to round off the corners as in Fig. 3.

With an early cut off, the effect would be as shown in Fig. 4.

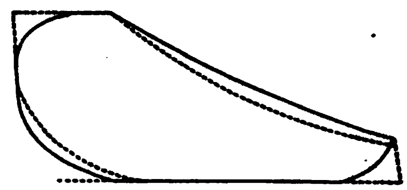


Fig. 4.

The friction of the mechanism causes the pencil to be behind its true position by a nearly constant quantity, and hence during expansion and exhaust the pencil will be too high, and during compression and admission the pencil will be too low. This is shown in Fig. 5. Its effect on the area of the diagram is therefore not very great.

Fig. 5.

The magnitude of these effects, taken together, on the area of the diagram, depends on the construction of the instrument and on pencil-pressure. From numerous experiments with Richards' and Thomson's indicators, it was found that a comparatively slight alteration of pencil-pressure from that just sufficient to mark the diagram, would cause an excess of 0·5 lb. during expansion, and an equal fall during compression. While if pencil-pressure were made sufficient to prevent serious oscillations when $n = 15$, the mean acting pressure was affected by as much as 1·5 lb. Thus it would appear possible to make a difference of as much as 5 per cent. in a locomotive in mid-gear by pencil-friction.

The conclusions, then, as regards the motion of the pencil, are, that the general effects of inertia and friction are both to increase the size of the diagram; that so long as the speeds are such that n is not greater than 15, the effect of inertia is less than 1 per cent., but that if n is less than 30, oscillations will show themselves unless the pencil-friction be increased. They may, by this, be kept down till $n = 15$, but not farther, and then the necessary friction will affect the area of the diagram about 5 per cent. A speed, therefore, which makes $n = 15$ is about the limiting speed at which diagrams can be taken accurate to 5 per cent., while for the diagrams to be sensibly accurate and free from oscillation the speeds must not be greater than will make $n = 30$.

These speeds for Richards' indicators are given in Table II.

TABLE II.

	N	
e	$n=30$	$n=15$
20	69	138
30	85	170
40	99	198
50	105	210
60	120	240
70	130	260
80	139	278
90	147	294
100	155	310

SECTION III.—DISTURBANCES ON THE DRUM.

These are the disturbances (3), (4), (5), section (1). They arise from the elasticity of the cord and mechanism connecting the drum with the piston of the engine. In order to express them definitely—

l is the indicated length of the diagram in inches;

y the yielding of the mechanism in inches per lb. of the tension;

I the moment of inertia of the drum.

(3) *The Inertia of the Drum.*—If the obliquity of the connecting-rod of the engine be disregarded, and x be put for the distance OP (Fig. 1), the force arising from inertia is proportional to N^2x and the disturbance arising from this cause will be yIN^2x. And as x will be positive or negative according as P is to the right or left of O, the diagram will be uniformly elongated.

The effect of the obliquity of the connecting-rod would be to increase this elongation at the back-end and diminish it at the front, increasing the area of the back-end diagram, and diminishing that of the front somewhat, but it is small unless the connecting rod is very short.

(4) *The effect of the varying Stiffness of the Spring.*—Let q be the difference of tension of the spring at the extreme ends of the diagram. Then the disturbance of the point P will be

$$-\frac{qyx}{l}.$$

This effect is therefore opposite to that of (3), and the joint effect will be

$$\left(IN^2 - \frac{q}{l}\right) yx,$$

and since IN^2 will be zero at small speeds, and it increases as the square of the speed, when the speed is low the diagram will be qy too short, but as the speed increases this shortening will diminish until at some speed $IN^2 = \frac{q}{l}$, and for higher speeds the diagram will be elongated. With the Richards' indicator, the critical speed appears to be $150 = N$. In most diagrams these effects are apparent, but, except when the connecting-rod is short, they do not affect the indicated pressure.

(5) *The effect of the Friction of the Drum.*—Let F be the tension on the string necessary to overcome the friction of the drum in either direction.

Then during the forward stroke the string will be stretched from this cause yF, and during the backward stroke it will be shortened yF. The effect will be to place the drum always behind its true position by yF. This is shown in Fig. 6.

Ac_1c_2, &c. represent the positions of the crank on its circle, as explained in reference to Fig. 1; but in this case c_1c_2, &c. are chosen so as to correspond with the equidistant positions of the piston. Projecting c_1c_2 with the connecting-rod as radius on to the atmospheric-line the points are obtained in which, for a true diagram, the pencil would be when the crank was in the positions c_1c_2, &c., but owing to the cause under consideration, as the crank moves from A towards B, the pencil will be (at the points ς) at a distance Fy behind its true position, and from B to A (at the points δ) Fy behind its true position.

When the crank arrives at A from B the pencil will not, as it should, arrive at A, but at the point (marked δ A) distant Fy towards B. This is the end of the indicated stroke, and here the drum will remain until the piston has reversed its position (with regard to δ A), that is, until the crank has reached A'; hence, as the crank moves from A to A', the drum will be stationary, and then move off distant Fy behinds its true position, which

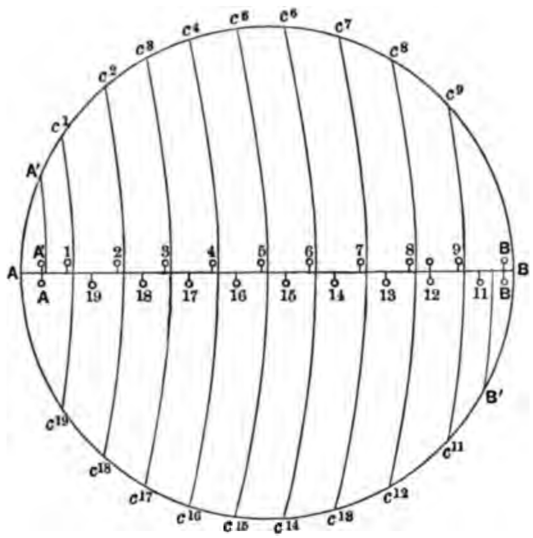


Fig. 6.

distance it will maintain until the crank reaches B, when the drum will again rest (at ♀ B) until the crank has reached B', when it will again start towards A distant Fy behind its true position.

The effect of this disturbance on a diagram is very great.

In the first place, it must be noticed that, supposing y the same, *i.e.*, the length of cord used the same, the effect will be the same on both diagrams. In starting from either end the drum does not move until the engine-piston has moved through a distance Fy, and the crank has moved through AA' or BB', so that, however the pencil of the indicator may have been moved, in this interval it will merely describe a vertical line (a very common feature of diagrams). For the rest of the motion the drum will move at a constant distance behind its true position, so that the two halves of the diagram will be of the right shape, but wrongly placed with regard to each other. If, then, the pressure at the ends of the true diagram rose and fell instantaneously, so that the extreme ends are vertical, as shown by the line $ACBD$ in Fig. 7, the indicated diagram $A'CBD'$ would be obtained from the true diagram by simply giving a horizontal shift (as in Fig. 7) $AA' = 2Fy$ to the lower half of the diagram-line ADB.

The apparent cut-off is then shortened by

$$AA' = 2yF \quad\text{.................................}(6).$$

The diagram is shortened by $2yF$.

The area is diminished by

$$\frac{p_1-p_3}{e}AA';$$

and putting $i_m = \text{area}\,\frac{e}{l}$.

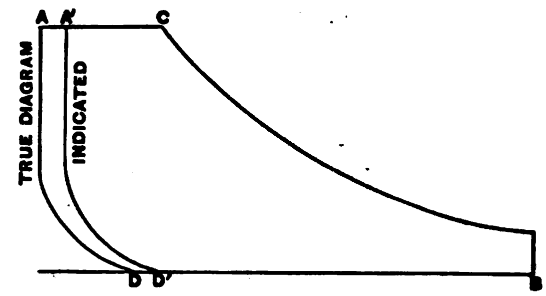


Fig. 7.

The effect f on p_m, or $p_m = i_m + f$, is given by

$$f = (i_1 - i_m - i_3)\frac{2Fy}{l+2Fy} \quad \text{..........................(7).}$$

It is thus seen that f increases with the expansion and compression, and is zero when these are zero.

This effect of the friction of the drum appears to be so important, and to have been so entirely unperceived, that it may be well to introduce a short discussion of the circumstances on which it depends, and on its effects.

The circumstances are the elasticity of the cord and the friction of the drum, and the important question is, how far these exist in the ordinary indicators? In answer to this, it may be said that the diagrams, which led to the discovery of this effect, were taken with an indicator which had been in constant use for several years. It was in apparently perfect condition, and the diagrams did not differ essentially from those which had been previously taken. The cord was one which was supplied by the maker. The manner of the discovery was as follows: For years the Author had pursued in the class the method of testing the vibrations of the indicator-pencil by projecting them on to the crank-circle, as shown in Fig. 1, and he had all along noticed that the first oscillation fell short, and shorter in the back-diagram than the front. The cause of this was not obvious, as there seemed to be several possible explanations, and it was partly with a view to determine this cause that Mr Brightmore's investigation was commenced. A slight error in the reducing-rod, which had a fixed centre and a slot in which a stud in the slide-block worked, was altered at Mr Brightmore's suggestion. This, however, did not get rid of the effect. A new cord obtained from the makers was substituted for the old one, and the effect was found to be much enhanced, the new cord being more elastic than the old one. This reduced it to the stretching of the cord, but it was only after carefully working out the effect of the inertia of the drum, and it was seen this effect was to lengthen, not shorten, the first oscillation at the back-end, that it occurred to the Author to look to the friction. The indicator was then taken to pieces, cleaned and oiled; then the effect was much reduced. Several new wires and cords were used which gave less effects, and eventually the steel wire was adopted by Mr Brightmore as the best. The test supplied by the oscillations could only be applied to diagrams taken at high speeds, and the test furnished by the effect upon area was vague. What was wanted was an independent means of determining the simultaneous positions of the drum and the engine-piston. As the best method of meeting this, it was decided to arrange an electric-circuit through the pencil to the drum, with sufficient electromotive force to prick the paper, making the engine-piston close this circuit at eleven definite equidistant points in its motion backwards and forwards. After some difficulty this was successfully carried out by Mr Brightmore and Mr Foster. In this way the stretching of the cord during the backward and forward strokes was definitely ascertained by Mr Brightmore. Taking the smallest results obtained with a cord, it appears from these experiments that the least difference of stretching was to make

$$2Fy = 0{\cdot}05C \text{ in inches} \qquad (8),$$

where C is the length of the cord in feet; so that there is obtained from equation (7)

$$f = (i_1 - i_m - i_3)\frac{0{\cdot}05C}{l + 0{\cdot}05C} \qquad (9).$$

This equation gives the value of f or $p_m - i_m$ for any diagram in terms of the length of the cord, on the assumption that the stretching is the same per foot of cord. The length of cord is generally 1·5 times the stroke for the front-end, and 2·7 times for the back-end, or 2·1 for both, hence putting S for the stroke in feet

$$f = (i_1 - i_m - i_3)\frac{0{\cdot}075S}{l + 0{\cdot}075S} \qquad (10)$$

for the front-end,

$$f = (i_1 - i_m - i_3)\frac{0{\cdot}135S}{l + 0{\cdot}135S} \qquad (11)$$

for the back-end, or

$$f = (i_1 - i_m - i_3)\frac{0{\cdot}105S}{l + 0{\cdot}105S} \qquad (12)$$

as the mean.

In the College engine, with 3 cwt. on the brake, at a speed of one hundred and seven revolutions,

$$S = 1{\cdot}5;$$
$$l = 5{\cdot}0;$$
$$\dot{i}_1 - \dot{i}_2 = 30{\cdot}0;$$
$$\dot{i}_m = 23{\cdot}0.$$

From (12)

$$f = 0{\cdot}24;$$

or

$$\frac{f}{\dot{i}_m} = 0{\cdot}01.$$

In a locomotive-diagram, Fig. 8, published in *Richards' Indicator*, by Porter,

$$S = 2;$$
$$l = 4;$$
$$\dot{i}_1 - \dot{i}_2 = 105;$$
$$\dot{i}_m = 40;$$
$$f = 3{\cdot}25;$$
$$\frac{f}{\dot{i}_m} = 0{\cdot}08.$$

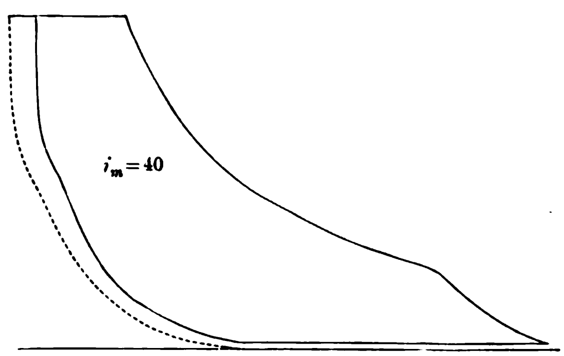


Fig. 8.

In the case of a condensing-engine $S = 3{\cdot}5$, cutting-off at apparently $\frac{1}{10}$,

$$\frac{f}{\dot{i}_m} = 0{\cdot}2;$$

and in the case of a compound-engine expanding ten times

$$\frac{f}{\dot{i}_m} = 0{\cdot}10.$$

These would seem to be the smallest results that can have occurred in ordinary practice. The conclusion, however, that hitherto the normal indicated power from engines has been from 10 to 20 per cent. too small is one which must be received with hesitation, or must wait for verification. Yet it may be pointed out that there are not wanting independent evidences of such an effect. There are features common to most diagrams which are shown in this investigation to be due solely to this effect.

(i) In diagrams taken from engines at high speeds the admission-line would not but for this effect be vertical. It would show a certain amount of detail, and the first oscillation would not have a sharp top. They would be as shown in Fig. 9, whereas they commonly are as in Fig. 10.

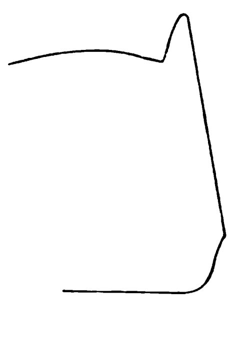


Fig. 9.

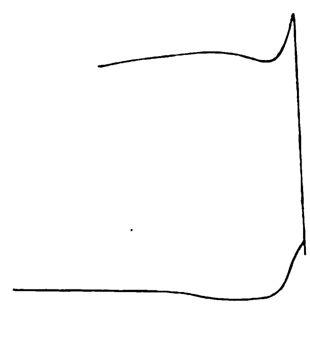


Fig. 10.

(ii) It is commonly found that the expansion-line is above the true expansion-line for the steam allowing for clearance. This fact has been much commented upon, and is sometimes assumed to indicate leaking valves, and sometimes a large amount of evaporation from the jacket, either of which circumstances may explain some rise of the expansion-line towards the end of the stroke, but it is difficult to see how they can explain the rise from cut-off which is usually observed. Now this apparent rise in the curve of expansion is exactly what would result if the apparent cut-off were too early, and this is the result of the effect that has been considered. The author has tried several diagrams, and he finds that, correcting the cut-off by formula 6, the expansion-line comes out very close indeed to the true curve.

(iii) In making these comparisons the explanation of another feature of diagrams became apparent. When the two diagrams are traced on the same card there is sometimes seen a want of symmetry about them, and almost invariably when this is the case the cut-off is shorter on the back than on the front-diagram. This would be the result of the friction of the drum, supposing the cord for the back-diagram longer than that for the front. Where this is the case the relative lengths of the cord are about 1 to 1·8.

These observations are all illustrated in Fig. 11, which represents a facsimile diagram from *Richards' Indicator*.

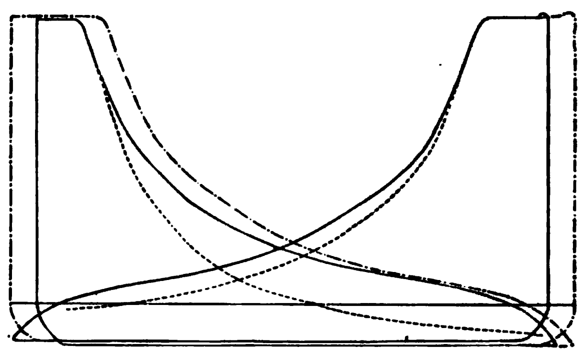


Fig. 11.

To test this diagram a tracing was taken, and reversed so that the front-diagram was superimposed on the back. It was then observed—

(*a*) That the diagrams were of different lengths, and the difference was about the same as the difference in cut-off.

(*b*) That notwithstanding the apparent cut-off in the back-diagram is to that in the front in the ratio of 2 to 3, the expansion-line of the back-diagram was exactly the same shape as that of the front.

(*c*) That if the diagrams were restored by formula 8, supposing the lengths of cords used to have been 5 feet and 9 feet, the diagrams became exactly similar, and, allowing 2 per cent. clearance, the expansion-line comes to be the true expansion-line for that cut-off. This rearrangement is shown in the dotted lines in Fig. 11, the mean pressure from which is 14 per cent. larger than from the original diagrams.

Such instances as these seem to sufficiently establish a *primâ facie* case against the confidence which appears to be at present placed in the accuracy of indicator-diagrams. But, in conclusion, the author would state that he should be very disappointed if anything in this investigation should have the effect of diminishing reliance on the indicator itself. He would have the instrument treated as other instruments have been treated, and instead of its results being assumed accurate, he would have it the object of careful study and experimental investigation, so that the limits of its wonderful perfection may be exactly known, and that reliance placed on it which such knowledge must afford.

49 A.

EXPERIMENTS ON THE STEAM-ENGINE INDICATOR.

By ARTHUR WILLIAM BRIGHTMORE, B.Sc., Stud. Inst. C.E.,
Late Berkeley Fellow in Owens College, Manchester.

THE object of these experiments was to ascertain definitely to what extent certain disturbing causes, which exist in the indicator, affect the diagram.

These disturbing causes are:—

1st. The necessary inaccuracy of the indicator springs, when cold or hot.

2nd. The effect of the inertia of the piston and parallel-motion bars on the area.

3rd. The effect of the oscillations of the spring on the diagram, and the extent to which these may be reduced without sensibly altering its area.

4th. The effect produced by the stretching of the indicator-cord. To get rid, as far as possible, of the error due to this cause, in the experiments relating to the second and third causes, a thin steel wire (B. W. G. 22) was used instead of a cord.

The following is a description of the apparatus employed:—

INDICATOR.

The indicator was an ordinary Richards indicator, made by Elliott Bros., London; having Watt's parallel-motion for magnifying the deflection of the spring. Springs by different makers were used.

ENGINE.

The engine employed was the one which is used for the Owens College workshop. It was not chosen on account of any particular adaptability for the purpose; in fact, in some of the experiments, although it fulfilled the requirements, the results were not so marked as they would have been, had the point of cut-off been earlier; but it was necessary in the experiments to have complete control over the engine, and to be able to run it with the brake on only, and the College engine presented these facilities.

It is a non-condensing engine, with 9-inch cylinder, 18 inches length of stroke, and a fly-wheel 16 feet in circumference. The point of cut-off is towards the end of the stroke. It works up to a boiler-pressure of about 47 lbs. on the square inch, and to a speed of about 150 revolutions per minute.

Reducing-Mechanism.

In order to give the paper-drum a reduced motion of the piston, the wire employed to rotate the drum was attached to a rod, one end of which turned on a pin in the cross-head, and the other end worked in a slot, fixed vertically over the middle position of the cross-head pin, as shown in Fig. 12.

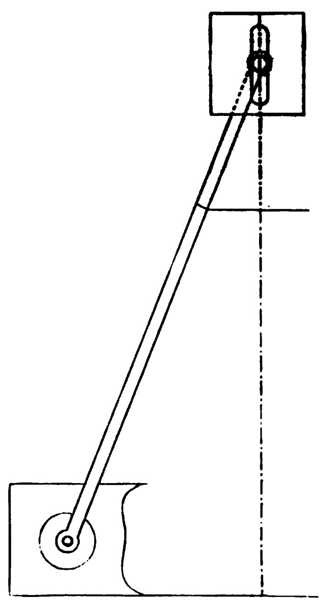


Fig. 12. Mechanism for Reducing the Motion of the Piston.

By this method of reducing the motion of the piston of the engine, the only error that comes in is due to the slight change of inclination of the wire.

Brake.

The work done was measured by the friction-brake used in the class experiments at Owens College.

It consists of small flat blocks of wood threaded on a cat-gut rope, and is passed round the fly-wheel. To one end of this rope a board, of which the other extremity rests on the ground, is fastened; and the load is placed on the board close to its attachment to the brake. The other end of the brake is attached to a spring-balance, which measures the tension on it; the arrangement is shown in Fig. 13.

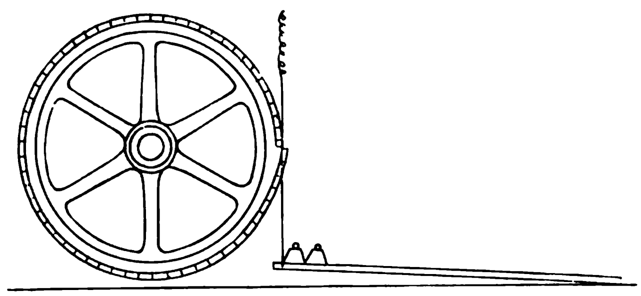


Fig. 13. FRICTION-BRAKE.

Thus, the rate of work was obtained by multiplying the difference of the tensions at the two ends by 16 feet (the circumference of the fly-wheel).

SPEED-INDICATOR (FIG. 14).

This was also a class instrument. It consists of a small paddle-wheel fixed on a vertical axis, in a small circular box containing coloured liquid.

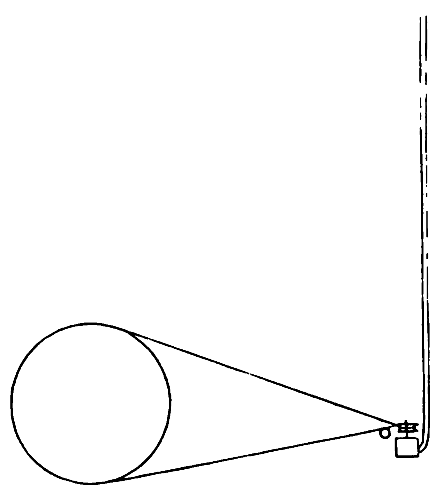


Fig. 14. SPEED-INDICATOR.

Near the bottom of this box, an upright glass tube is inserted. The paddle-wheel was rotated by a cord, driven from a pulley on the main shaft, and passing round a pulley fixed on the same vertical spindle with it.

The rotation of the paddle causes the liquid to rise in the tube to a height dependent on the speed of the engine.

Thus the scale was graduated by running the engine at constant speeds and counting.

INDICATOR-SPRINGS (FIG. 15).

Before commencing the experiments, it was necessary to test the accuracy of the indicator-springs.

To do this, the indicator was rigidly fixed in a vertical position, and pressure was applied to the centre of the indicator-piston by means of a rod, pressed upwards by one end of a long beam, balanced on a knife-edge; the weight being hung on the other end of the beam.

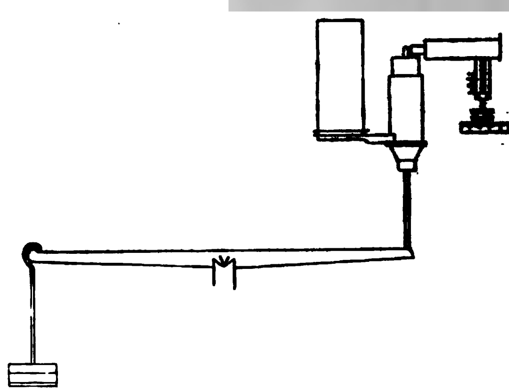


Fig. 15. APPARATUS FOR TESTING THE SPRINGS.

The deflection of the springs was measured by Professor Reynolds' small cathetometer, used in his experiments on "Thermal Transpiration," and fully described in the *Philosophical Transactions of the Royal Society*, Part II. 1879.

It consists of a microscope carried by a vertical sliding-piece moved by a very accurate screw with fifty threads to the inch, and is capable of measuring to $\frac{1}{10,000}$ inch. Thus, by continually adjusting the screw, so that some well-defined mark on the piston-rod lay on the horizontal cross-hair, and noting the reading for each particular weight, the deflections under the various pressures were arrived at.

To prevent the piston of the indicator sticking in a wrong position, owing to friction, the frame to which the indicator was attached was tapped with a light hammer each time a fresh weight was added.

Table I. gives the results of these experiments for five springs, at the ordinary temperature.

The next thing was to see what effect an increase of temperature would have on the springs. Now, the temperature of the indicator-spring never rises above 212° Fahrenheit, owing to its being open to the atmosphere, and moisture always being present in the indicator. Hence the springs were surrounded with steam at 212° Fahrenheit, by passing it through a hole in the cap of the indicator; of course the steam was at first all condensed; but by waiting until steam issued from another hole in the cap, the temperature was maintained uniformly at 212° during the experiments, which were conducted as in the previous cases. The result of these experiments for the same five springs is given in Table II.

Tables I. and II. show the uniformity of the increase of the deflection with a constant addition to the pressure on the spring. They also prove that the deflection of a spring is greater, under the same weight, the higher the temperature; hence the necessity of setting indicator-springs when hot, *i.e.*, when at the temperature of boiling water. It appears also from these Tables, that in the case of the springs experimented upon, the deflection under a given weight at 212° Fahrenheit is about 3 per cent. greater than at the ordinary temperature; therefore a diagram, taken with a spring which is perfectly correct when cold, will be 3 per cent. too large.

This is shown more clearly in Table III., which gives the mean deflection of springs under 1 lb. when cold and when hot, as calculated from Tables I. and II., and the deflection under 1 lb. as calculated from the number marked on the spring. The percentage error in the fifth column is the difference between columns three and four, and is allowed for in all the following calculations. It will be noticed from this Table, that in one case only did this error amount to 2 per cent.

Table I.—Deflection of Springs, when Cold.

Strength of Spring, lbs. to the Inch.	20		32		32*		50		80	
Weight in Scale Pan.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.
	0·6829		0·4887		0·4888		0·4832		0·4791	
		0·0246		0·0156		0·0157		0·0099		0·0060
1	0·7075		0·5043		0·5045		0·4931		0·4860	
		0·0242		0·0156		0·0154		0·0104		0·0061
2	0·7317		0·5199		0·5199		0·5035		0·4921	
		0·0245		0·0154		0·0154		0·0101		0·0063
3	0·7562		0·5353		0·5353		0·5136		0·4984	
		0·0243		0·0153		0·0152		0·0101		0·0062
4	0·7805		0·5506		0·5505		0·5237		0·5046	
		0·0240		0·0156		0·0153		0·0100		0·0062
5	0·8045		0·5662		0·5658		0·5337		0·5108	
		0·0240		0·0154		0·0152		0·0100		0·0062
6	0·8285		0·5816		0·5810		0·5437		0·5170	
		0·0245		0·0153		0·0153		0·0095		0·0062
7	0·8530		0·5969		0·5963		0·5532		0·5232	
		0·0250		0·0157		0·0156		0·0099		0·0062
8	0·8780		0·6126		0·6119		0·5631		0·5294	
		0·0250		0·0157		0·0155		0·0098		0·0062
9	0·9030		0·6283		0·6274		0·5729		0·5356	
		0·0248		0·0157		0·0153		0·0100		0·0061
10	0·9278		0·6440		0·6427		0·5829		0·5417	
		0·0249		0·0155		0·0157		0·0101		0·0060
11	0·9527		0·6595		0·6584		0·5930		0·5477	
		0·0247		0·0155		0·0154		0·0099		0·0060
12	0·9774		0·6750		0·6738		0·6029		0·5537	
		0·0246		0·0158		0·0160		0·0099		0·0059
13	1·0020		0·6908		0·6898		0·6127		0·5596	
		0·0244		0·0156		0·0156		0·0100		0·0060
14	1·0264		0·7064		0·7054		0·6227		0·5656	
		0·0243		0·0158		0·0155		0·0100		0·0061
15	1·0507		0·7222		0·7209		0·6327		0·5717	
		0·0246		0·0156		0·0161		0·0100		0·0059
16	1·0753		0·7378		0·7370		0·6427		0·5776	
		0·0238		0·0157		0·0159		0·0100		0·0061
17	1·0991		0·7535		0·7529		0·6527		0·5837	
		0·0251		0·0156		0·0154		0·0100		0·0059
18	1·1242		0·7691		0·7683		0·6627		0·5896	
		0·0244		0·0150		0·0151		0·0100		0·0062
19	1·1486		0·7841		0·7834		0·6727		0·5958	
		0·0242		0·0152		0·0154		0·0100		0·0072
20	1·1728		0·7993		0·7988		0·6827		0·6030	
		...		0·0152		0·0156		0·0102		0·0062
21	...		0·8145		0·8144		0·6929		0·6092	
		...		0·0154		0·0155		0·0101		0·0062
22	...		0·8299		0·8299		0·7030		0·6154	
		...		0·0156		0·0153		0·0103		0·0061
23	...		0·8455		0·8452		0·7133		0·6215	
		...		0·0151		0·0155		0·0100		0·0063
24	...		0·8606		0·8607		0·7233		0·6278	
		...		0·0152		0·0160		0·0101		0·0062
25	...		0·8758		0·8767		0·7334		0·6340	
		...		...		...		0·0099		0·0062
26	...		...		...		0·7433		0·6402	
		...		...		...		0·0102		0·0066
27	...		...		...		0·8535		0·6468	

* Different maker.

TABLE II.—DEFLECTION OF SPRINGS, WHEN HOT.

Strength of Spring, lbs. to the Inch.	20		32		32*		50		80	
Weight in Scale Pan.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.	Reading of Micrometer.	Deflection under 1 lb.
	0·6872		0·4941		0·4937		0·4862		0·5953	
		0·0245		0·0154		0·0162		0·0104		0·0063
1	0·7117		0·5095		0·5099		0·4966		0·6016	
		0·0251		0·0160		0·0157		0·0108		0·0065
2	0·7368		0·5255		0·5256		0·5074		0·6081	
		0·0251		0·0158		0·0157		0·0103		0·0064
3	0·7619		0·5413		0·5413		0·5177		0·6145	
		0·0249		0·0159		0·0156		0·0103		0·0062
4	0·7868		0·5572		0·5569		0·5280		0·6207	
		0·0250		0·0157		0·0157		0·0103		0·0063
5	0·8118		0·5729		0·5726		0·5383		0·6270	
		0·0251		0·0162		0·0156		0·0103		0·0063
6	0·8369		0·5891		0·5882		0·5486		0·6333	
		0·0253		0·0156		0·0157		0·0098		0·0063
7	0·8622		0·6047		0·6039		0·5584		0·6396	
		0·0258		0·0162		0·0164		0·0105		0·0064
8	0·8880		0·6209		0·6203		0·5689		0·6460	
		0·0258		0·0163		0·0159		0·0104		0·0066
9	0·9138		0·6372		0·6362		0·5793		0·6526	
		0·0256		0·0160		0·0160		0·0099		0·0062
10	0·9394		0·6532		0·6522		0·5892		0·6588	
		0·0259		0·0163		0·0158		0·0102		0·0062
11	0·9653		0·6695		0·6680		0·5994		0·6650	
		0·0258		0·0160		0·0162		0·0104		0·0064
12	0·9911		0·6855		0·6842		0·6098		0·6714	
		0·0250		0·0162		0·0158		0·0098		0·0066
13	1·0161		0·7017		0·6700		0·6196		0·6780	
		0·0250		0·0159		0·0162		0·0106		0·0065
14	1·0411		0·7176		0·7162		0·6302		0·6845	
		0·0246		0·0163		0·0163		0·0105		0·0062
15	1·0657		0·7339		0·7325		0·6407		0·6907	
		0·0259		0·0164		0·0161		0·0101		0·0063
16	1·0916		0·7503		0·7486		0·6508		0·6970	
		0·0258		(†)		0·0161		0·0104		0·0067
17	1·1174		0·7689		0·7647		0·6612		0·7037	
		0·0252		0·0160		0·0158		0·0105		0·0064
18	1·1426		0·7849		0·7805		0·6717		0·7101	
		0·0242		0·0159		0·0157		0·0105		0·0061
19	1·1668		0·8008		0·7962		0·6822		0·7162	
		...		0·0159		0·0160		0·0106		0·0065
20	...		0·8167		0·8122		0·6928		0·7227	
		...		0·0159		0·0157		0·0102		0·0062
21	...		0·8326		0·8279		0·7030		0·7289	
		...		0·0163		0·0170		0·0105		0·0063
22	...		0·8489		0·8449		0·7135		0·7352	
		...		0·0162		0·0156		...		0·0064
23	...		0·8651		0·8605		...		0·7416	
		...		0·0159		0·0160		...		0·0063
24	...		0·8810		0·8765		...		0·7479	
		...		0·0162		0·0164		...		0·0063
25	...		0·8972		0·8929		...		0·7542	
		...		...		...		...		0·0062
26	...		...		...		...		0·7602	

* Different maker. † Condensed steam let out of cylinder.

EFFECT OF INERTIA OF THE MOVING-PARTS ON THE AREA OF THE DIAGRAM.

Having ascertained the errors in the springs, the next question was to find how far the effect of inertia tends to alter the area of the diagram before the oscillations appear. To do this, diagrams were taken at various speeds and with several springs. In Table IV. the efficiences, *i.e.*, the ratios of the brake-pressures to the mean diagram-pressures, are given at the various speeds, instead of the mean pressures as calculated from the diagrams, on account of the difficulty of keeping the load on the brake exactly constant.

TABLE III.—MEAN DEFLECTIONS OF SPRINGS UNDER 1 lb.

Spring.	Experimental Deflection, cold.	Experimental Deflection, hot.	Deflection from Mark on Spring.	Percentage Error.
	Inch.	Inch.	Inch.	
20	0·0245	0·02525	0·02523	0·08
32	0·0155	0·01600	0·01580	1·25
32*	0·0155	0·01595	0·01580	0·95
50	0·0100	0·01030	0·01009	2·08
80	0·0062	0·00636	0·00630	0·94

* Different maker.

Now if the inertia affects the areas of the diagrams, the areas of the diagrams, and hence the mean diagram-pressures, will vary directly with the velocity, and inversely as the stiffness of the spring (the weight on the brake being constant); *i.e.*, the efficiencies will vary directly with the stiffness of the spring and with the inverse of the velocity. However, an examination of the Table shows no appreciable increase of the efficiency with greater stiffness of the spring, and no more decrease, as the velocity increases, than would be accounted for by the greater friction.

Table IV. is not filled in for the 20 and 32 springs at the higher speeds, because the oscillations begin to come in.

The inference is, "that in a given engine, when the ratio of the speed to the stiffness of the spring, used to indicate it, is not so great as to cause oscillations to appear in the diagram, the area is not appreciably affected by the momentum of the moving parts." This seems natural, for, after the initial disturbance on the admission of the steam to the cylinder, the motion of the spring is gradual, and hence its deflection would correspond to the pressure on it.

TABLE IV.

Speed.	Efficiencies.				
	Spring.				Mean Values.
	20	32	50	80	
44	0·94	...	0·95	...	0·945
68	0·93	0·94	0·93	...	0·933
84	...	0·93	0·93	0·93	0·930
107	...	0·93	0·94	0·93	0·933
127	...	...	0·93	0·92	0·925

OSCILLATIONS.

When the ratio of the speed of the engine to the stiffness of the spring, used to indicate it, exceeds a certain value, which is different for different engines, oscillations appear in the diagram.

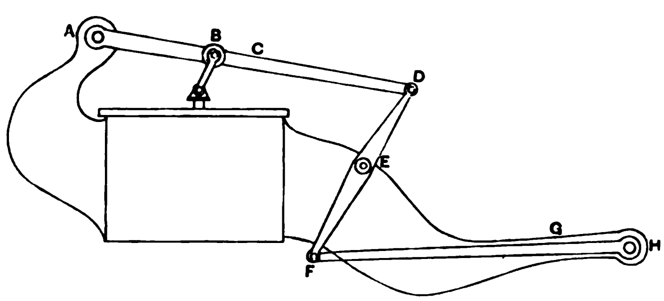


Fig. 16.

The equation which gives the time of oscillation of the spring, modified by the parallel-motion bars (Fig. 16), devised by Professor Reynolds, is, taking the axis of x vertically upwards:—

$$\frac{W'}{g}\frac{d^2x}{dt^2} = Q - ex \qquad (1),$$

where $$W' = W + (w + w_2)\frac{k^2}{a^2} + 16w_1,$$

$$Q = P - \left(W + (w + w_2)\frac{b}{a} + 4w_1\right),$$

and $e =$ the stiffness of spring.

$W =$ weight of piston + $\frac{1}{2}$ weight of spring.

$w =$ weight of rod AD (Fig. 16).

$w_1 =$ weight of rod DF.

$w_2 =$ weight of rod FH.

$P =$ whole pressure of steam on the piston.

$a = AB$.

$b = AC = GH =$ distance of centres of gravity of rods AD, FH from A and H respectively.

$k =$ radius of gyration of AD, FH, about A and H respectively.

W' is, in fact, the weight which would have to oscillate at B to be equivalent to the moving-parts, and the expression $P - Q$ represents the force which would have to be applied at B, if the parts referred to were removed, to be equivalent to them.

Equation (1) is of the well-known form for finding the time of a complete oscillation (T), and then is obtained in the ordinary way—

$$T = 2\pi\sqrt{\frac{W'}{g \cdot e}}.$$

Or calling N the number of oscillations per minute—

$$N = \frac{30}{\pi}\sqrt{\frac{g \cdot e}{W'}}.$$

It will be noticed that in equation (1), the rotation of the rod DF, which is very slight, is neglected, as also is the friction of the instrument.

In the case of the indicator employed, the values of the above constants were—

$W_{20} = 0{\cdot}10529$ lb. $\quad W_{32} = 0{\cdot}10954$ lb.

$w = 0{\cdot}00957$ lb.

$w_1 = 0{\cdot}01037$ lb.

$w_2 = 0{\cdot}00866$ lb.

$a = 0{\cdot}75$ inch.

$b = 1$ inch.

$k^2 = 1{\cdot}83$.

Whence from the above $W'_{20} = 0{\cdot}33063$ lb.

$$W'_{32} = 0{\cdot}33488 \text{ lb.}$$

and from preceding experiments $e_{20} = 475.$

$$e_{32} = 750.$$

NOTE.—The suffixes 20 and 32 refer to the springs marked 20 and 32 respectively.

Thus
$$N_{20} = 2050.$$
$$N_{32} = 2560.$$

It will be noticed on substituting for W', that the rod DF has as much influence in causing the oscillations to come in as all the other moving parts together.

To verify these results, diagrams were taken with weak springs, in order to bring in oscillations. It must be understood that the diagrams in this Paper are not intended as specimens of good diagrams, but are merely to illustrate the various points considered.

The time of oscillation of the indicator-springs may be approximately obtained from such diagrams in the following manner:—first, project the crests and hollows of the oscillations vertically down on to the atmospheric-line; next, with a radius equal to the length of the connecting-rod (reduced to the same scale as the length of the diagram), and centre on the atmospheric-line produced, project the points so obtained upon a circle described on the atmospheric-line with the length of diagram as the diameter; then the arcs of the circle intercepted between alternate intersections represent the angle turned through by the crank during the time of a complete oscillation of the spring. Hence, assuming that the crank-shaft rotates uniformly, these arcs would represent the time of a complete oscillation.

There are several reasons why the number of oscillations per minute so obtained should not quite equal the number as obtained above from theory. Firstly, the neglect of the rotation of the bar DF, and of the friction in the equation, would make a slight difference; but the most important reason is the gradual decrease of pressure in the cylinder of the engine, consequent upon the motion of the piston and initial condensation. This diminution of pressure causes the crests to lie behind, and the hollows to be in advance of their true position (Figs. 17 to 25), by an amount varying with the rate of decrease. Supposing for the moment the lag to be equal in amount for each crest, the projection of it (the lag) upon the crank-circle will include a greater arc towards the ends than in the middle of the diagram; thus, other things being the same, causing the time of oscillation to appear too great at one end of the diagram, and too small at the other end of the diagram. However, this tendency is counteracted, at least during the first half of the stroke (and it is during this period chiefly that the time of oscillation is measured), by the retardation of the velocity of oscillation, and consequently the greater effect of the reduction of pressure in causing the crests to lag as the stroke progresses. That the velocity of oscillation decreases with the distance from the point of admission is seen by integrating equation (1), where—

$$\frac{dx}{dt} = \left(\frac{Q}{e} - c\right) \cdot \sqrt{\frac{ge}{W'}} \cdot \sin \sqrt{\frac{ge}{W'}} \cdot t \quad \ldots\ldots\ldots\ldots\ldots (2)$$

where $c = \frac{1}{2}$ the distance of a hollow from the atmospheric-line.

Now $2\left(\frac{Q}{e} - c\right)$ is equal to the range of oscillation, as may be seen by again integrating equation (1), and in the case of the diagrams referred to, the range of oscillation, and hence from above, the velocity of oscillation of the spring diminished as the stroke advances, which is almost self-evident, for the time of oscillation is independent of the range, so that if the range be reduced the velocity must be reduced also.

From equation (2) it is also seen that, other things being the same, the number of oscillations in a diagram increases with the stiffness of the spring, hence the counteracting effect, just referred to, would be less marked as the stiffness of the spring used is increased, so that for this reason the number of oscillations per minute as obtained from a diagram would be nearer the truth the weaker the spring.

Again, the number of oscillations per minute will probably be nearer the truth the greater the speed of the engine; for the number of oscillations in

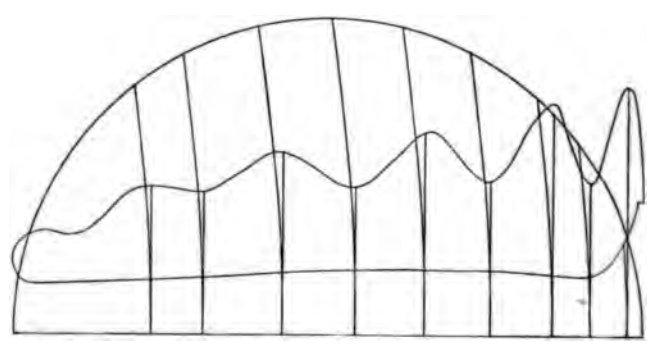


Fig. 17. Front-end diagram taken with 20 spring at 144 revolutions.

a diagram is smaller the greater the speed of the engine, because the time of oscillation of the spring is independent of the speed of the engine, and hence the ratio of the velocity of oscillation to the rate of reduction of pressure is less the higher the speed of the engine, hence the counteracting effect referred to is greater. These two points are illustrated in the diagrams, Figs. 17 to 25, and the accompanying Table V.

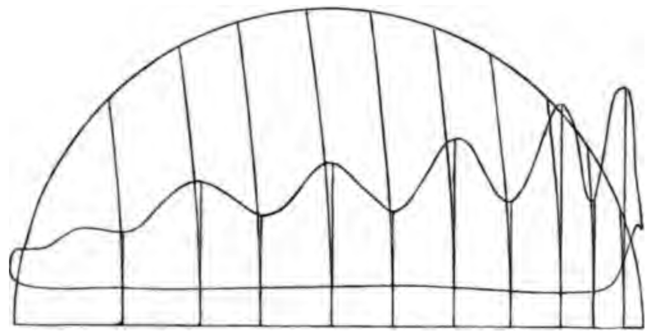


Fig. 18. Front-end diagram taken with 20 spring at 127 revolutions.

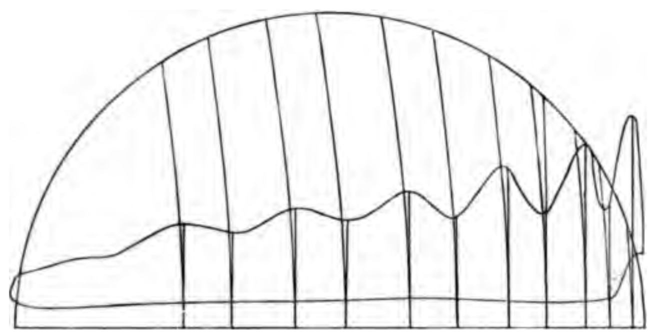


Fig. 19. Front-end diagram taken with 20 spring at 107 revolutions.

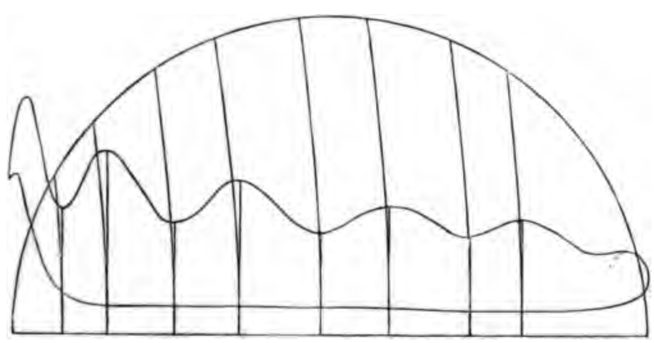


Fig. 20. Back-end diagram taken with 20 spring at 144 revolutions.

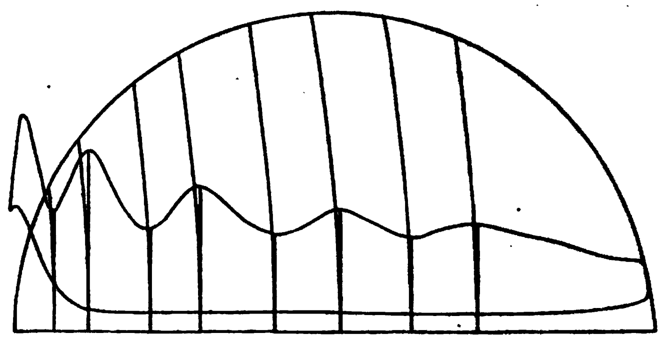


Fig. 21. Back-end diagram taken with 20 spring at 127 revolutions.

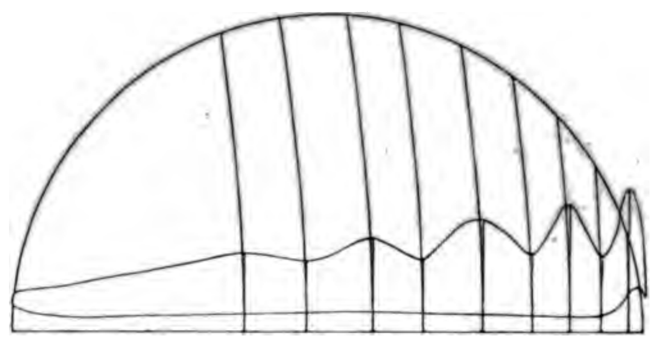


Fig. 22. Front-end diagram taken with 32 spring at 144 revolutions.

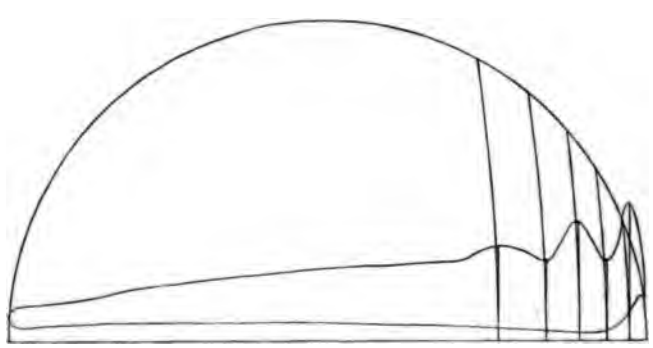


Fig. 23. Front-end diagram taken with 32 spring at 127 revolutions.

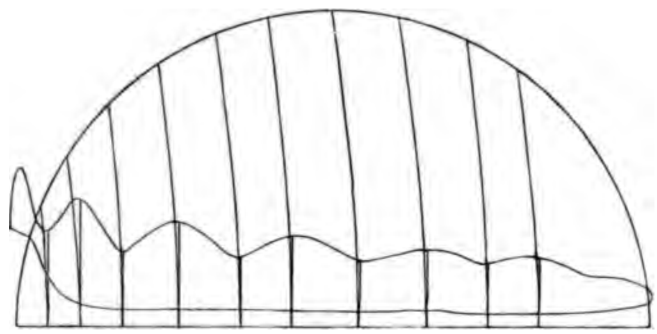


Fig. 24. Back-end diagram taken with 32 spring at 144 revolutions.

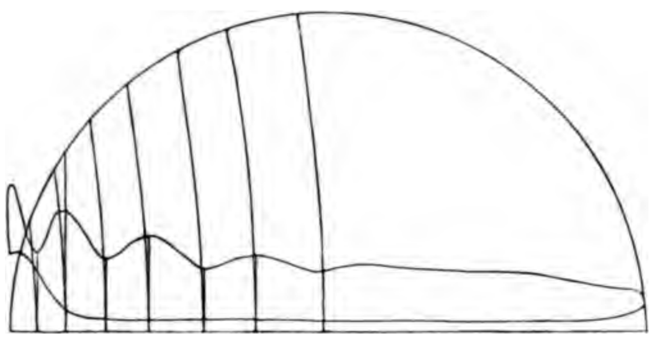


Fig. 25. Back-end diagram taken with 32 spring at 127 revolutions.

TABLE V.

Speed. Revolutions per minute.	End.	Spring.	Number of Oscillations.		Difference per cent.
			From Diagram.	From Formula.	
144 (Fig. 17)	Front	20	1,950	2,050	5·0
127 (Fig. 18)	„	20	1,920	„	6·5
107 (Fig. 19)	„	20	1,883	„	8·5
144 (Fig. 20)	Back	20	1,950	„	5·0
127 (Fig. 21)	„	20	1,930	„	6·0
144 (Fig. 22)	Front	32	2,370	2,560	7·5
127 (Fig. 23)	„	32	2,300	„	10·0
144 (Fig. 24)	Back	32	2,300	„	10·0
127 (Fig. 25)	„	32	2,300	„	10·0

In calculating the oscillations from the diagrams a mean value was taken. The distance to which the oscillations extend depends on the range of the first one, and on the friction of the pencil. The range of the first oscillation is great if the period of a semi-oscillation nearly coincides with the time the steam takes to attain its maximum pressure on admission; this happens when the engine is running fast. It is small when the time of attaining the greatest pressure of steam and the time of a semi-oscillation are not nearly equal. Thus, when the steam is wire-drawn on entering the cylinder of an engine, that engine would give a better diagram at high speeds than if this were not the case.

Again, if the steam be throttled on entering the indicator, the time of the steam attaining its maximum pressure in the indicator-cylinder will be lengthened; hence the extent of the first oscillation will be reduced, and therefore the oscillations in the diagram will be reduced; but the diagram so obtained does not give a correct idea of the work done, but is too small in proportion to the amount of throttling.

The effect of the friction of the pencil in lessening the extent of the oscillations varies with the pressure on the pencil. When the oscillations are thus reduced by pressing the pencil on the paper an indefiniteness is introduced into the results, owing to the pencil sticking either too high or too low, and the results cannot be relied on.

To illustrate this point diagrams were taken under the same conditions, of which the results are given in Table VI.

In the case of the weaker springs, 20 lbs. and 32 lbs., the pencil was pressed on the diagram-paper so as to reduce the oscillations. Diagrams were taken with stiffer springs, in which oscillations do not perceptibly enter, to check the results so obtained.

TABLE VI.—FRONT-END EFFICIENCIES.

Speed.	20 Spring (pencil pressed).	32 Spring (pencil pressed).	50 Spring.	80 Spring.
69	0·932	0·927	0·959	0·958
87	0·931	0·942	0·954	0·954
108	0·918	0·907	0·955	0·954
Mean efficiencies	0·927	0·925	0·956	0·955

Table VI. shows that in those experiments in which the pencil was pressed on to the paper the results are too small by more than 3 per cent. No doubt if the engine had cut-off earlier, and been working with a higher pressure of steam, the results would have been still more discordant.

Probably the most accurate method of arriving at the mean pressure when the oscillations extend a good way into the diagram, at least when the cut-off occurs late in the stroke as in the present case, is to draw a line midway between the crests and hollows, and to the value for the mean pressure obtained by taking this line add an amount, which in the case of indicators similar to the one employed in these experiments is 0·35 lb.

To see the reason for this, referring back to equation (1), and integrating it twice—

$$x = \frac{Q}{e} + \left(c - \frac{Q}{e}\right) \cos \sqrt{\frac{ge}{W'}} \, . \, t.$$

Substituting in this $t = \pi \sqrt{\frac{W'}{ge}}$ (time of half oscillation)

$$x = \frac{2Q}{e} - c,$$

i.e., Q is the arithmetical mean of ex and ec.

Substituting in this expression the value for Q, and taking the area of the indicator piston as 0·5 square inch, the following value for the intensity of pressure (p) is obtained:—

$$p = e(x + c) + 2\left(W + (w + w_2)\frac{b}{a} + 4w_1\right).$$

Hence if a line midway between the crests and hollows be taken as representing the pressure, the mean pressure so obtained will be too small by the amount of the second term on the right, which for the indicator employed = 0·35 lb. This would be negligible for any considerable pressure.

It was found that with the indicator used, a diagram tolerably free from oscillations could be taken from the engine up to a speed of about 90 revolutions per minute, with a spring of 20 lbs. to the inch. Hence, since the time the steam takes to attain its maximum pressure in the cylinder varies with the speed of the engine (in different engines it would also vary with the arrangement of the slide-valve), it might be expected to obtain a diagram tolerably free from oscillations at a speed of from 400 to 500 revolutions per minute, with an indicator having a parallel-motion in which the rod corresponding to DF is absent, and in which the other moving-parts are as light again as in the present case. This would be the case with an indicator of smaller diameter, in which a much stronger spring could be used for the same weight. For much higher speeds than this, unless the relative time occupied in attaining the maximum pressure increased with the speed, it would appear that the diagrams would be affected to a great but unknown extent by the oscillations of the spring.

Vitiation of the Diagram by the Stretching of the Indicator-Cord.

The effect of the stretching of the cord varies greatly with the shape of the diagram, and with the state of lubrication of the paper drum. Owing to the late cut-off, the engine employed in the experiments was not well suited for showing this effect. However, in some experiments, when the paper-drum wanted oiling, the diagram given with the cord was more than 7 per cent. smaller than that given with the steel wire. The effect is in all cases to reduce the area, though not necessarily to reduce the mean pressure calculated from it.

To ascertain if the diagrams from the engine in question would show much difference when taken with cord and with wire, the experiments summarised in Table VII. were made. The lengths and efficiencies given are the mean of the front- and back-end diagrams.

Table VII.

Speed.	Wire.		String.	
	Length.	Efficiency.	Length.	Efficiency.
	Inches.		Inches.	
68	5·11	0·93	4·78	0·94
84	5·11	0·93	4·80	0·94
107	5·13	0·94	4·80	0·94
127	5·12	0·93	4·80	0·97

Although the efficiency as calculated from the two sets of diagrams is inconsiderable, yet the difference in their lengths points to a large difference in their areas.

The difference in the tension of the indicator-cord at various parts of the stroke may be shown by considering the equation of motion of the indicator-drum.

This equation during the outward stroke is

$$\frac{I \cdot d^2\delta}{dt^2} = Ta - M_s - M_f.$$

where I = the moment of inertia of the drum about its axis.

T = the tension in the cord.

a = radius of drum.

M_s = moment of resistance of the drum-spring about the axis of drum.

M_f = moment of friction about the same line.

Hence $$T = \frac{1}{a}\left(I\frac{d^2\delta}{dt^2} + M_s + M_f\right).$$

$\frac{d^2\delta}{dt^2}$, the angular acceleration of the drum about its axis, is a maximum to begin with, and continues to decrease during the stroke, becoming zero near the middle of the stroke.

M_s is constant during the stroke.

M_f is a maximum on starting, then suddenly decreases and then varies directly with some power of the velocity, increasing therefore until about the middle of the stroke, and then diminishing.

Thus it is evident that during the outward stroke the tension T is a maximum to begin with, decreases rapidly about the middle of the stroke, and more slowly towards the end.

At the end of the stroke the friction suddenly changes sign, thus causing a sudden diminution in the tension at the commencement of the inward stroke; afterwards the tension increases rapidly about the middle of the stroke, and more slowly towards the end.

Hence it might be expected that that part of a diagram taken during the outward stroke would be shortened to commence with, then slightly stretched, and slightly shortened at the end; and that that part taken during the inward stroke, would be first shortened, then lengthened a little, and slightly shortened towards the end, almost as in the case of the outward stroke.

To show that this actually takes place, an arrangement was devised by Professor Reynolds, the object of which was to prick holes in the diagram corresponding to eleven equidistant positions of the piston. For this purpose a Grove battery (D Fig. 26) of five cells, in conjunction with a Ruhmkorff coil, was used. But in order to get the holes pricked in their proper positions, instead of the ordinary arrangement for making and breaking contact, the following plan was adopted, the hammer of the coil being held back. The wire from one pole of the battery was connected with one of the binding-screws (H) of the primary coil as usual, but the wire from the other pole of the battery was connected with the engine. A wire from the other binding-screw (*G*) of the primary coil was attached to the contact-breaker (*B*). This consisted of a smooth piece of wood, into which eleven pieces of wire were inserted at equal distances, and filed level with the wood, the

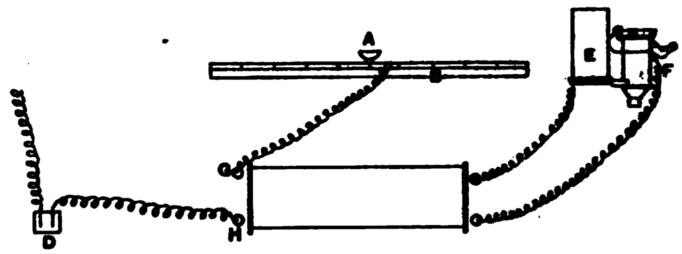


Fig. 26. Electrical Apparatus for showing the Distortion of a Diagram by the Indicator-Cord.

distance between the first and the last wires being the length of the stroke of the engine. The contact-breaker was fixed on the lower slide bar, so that the central wire should be at the middle of the stroke, and so that a pointer (*A*), which was secured to the cross-head, should slide on the smooth piece of wood. Hence every time the pointer crossed a wire on the contact-breaker the circuit of the primary current was complete, and a spark of the induced current passed through the diagram-paper. To bring this about one wire of the induced current was connected with the metallic drum (*E*), and the other to a cup of mercury (*F*), into which the metallic pencil dipped, thus completing the circuit of the induced current when the pencil touched the paper.

In the diagrams, Figs. 27 to 34, which were taken in this manner, the position of the pricked holes, corresponding to the eleven equidistant positions of the piston, are indicated by small circles. The relative positions of these circles show which parts of the diagrams are lengthened, and which are shortened. An examination shows that the effect is not merely to shorten

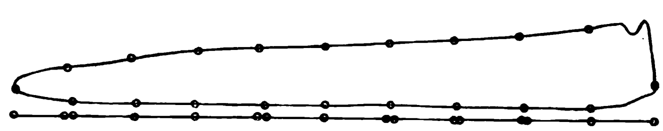


Fig. 27. Front-end pricked diagram taken with wire at 107 revolutions.

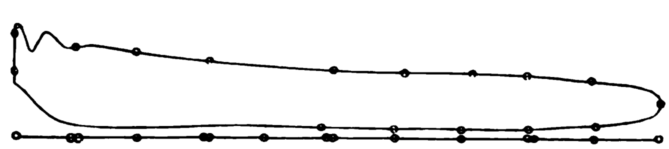


Fig. 28. Back-end pricked diagram taken with wire at 107 revolutions.

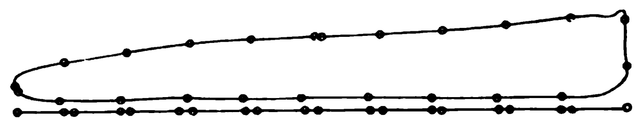


Fig. 29. Front-end pricked diagram taken with string at 107 revolutions.

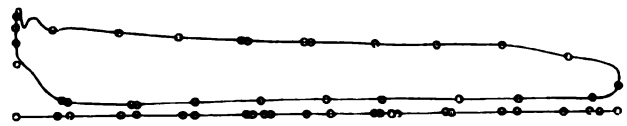


Fig. 30. Back-end pricked diagram taken with string at 107 revolutions.

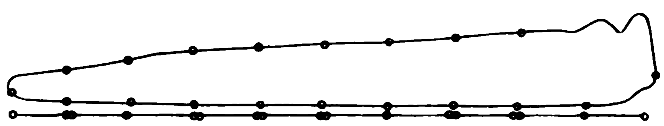


Fig. 31. Front-end pricked diagram taken with wire at 127 revolutions.

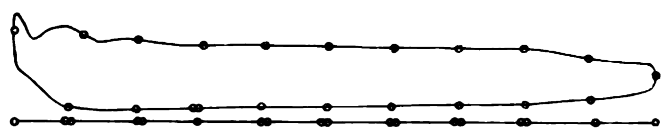


Fig. 32. Back-end pricked diagram taken with wire at 127 revolutions.

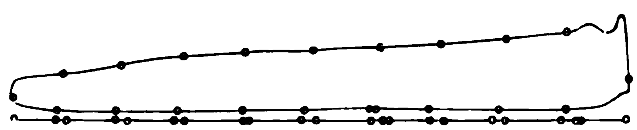


Fig. 33. Front-end pricked diagram taken with string at 127 revolutions.

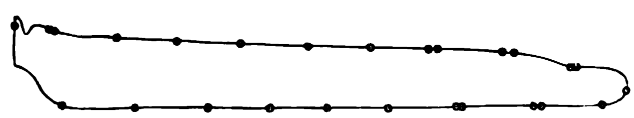


Fig. 34. Back-end pricked diagram taken with string at 127 revolutions.

the ends and lengthen the middle of the diagrams, but also to distort them, *i.e.*, to cause corresponding points in their upper and lower parts not to lie in the same vertical line. The amount of this distortion is shown by the distance between corresponding points on the atmospheric-line. It will also be noticed that even in the diagrams taken with wire instead of with cord this distortion is not altogether absent. The indefiniteness in the stretching of the cord is shown by some of the points being marked twice.

In high-speed diagrams of short length these effects would cause a marked modification in their form when taken with cord.

At high speeds, when the spring of the drum is not stiff enough to keep the cord tight near the centre of the stroke, and the velocity is greatest, a shortening of the middle portion of the diagram, taken during the inward stroke, and a lengthening of the end, would result.

These considerations show that in indicators intended to take diagrams from engines running at high speeds, the drum, as well as all the other moving-parts, should be as light as possible.

50.

ON THE DILATANCY OF MEDIA COMPOSED OF RIGID PARTICLES IN CONTACT. WITH EXPERIMENTAL ILLUSTRATIONS*.

[*From the* "Philosophical Magazine" *for December*, 1885.]

IDEAL rigid particles have been used in almost all attempts to build fundamental dynamical hypotheses of matter: these particles have generally been supposed smooth.

Actual media, composed of approximately rigid particles, exist in the shape of sand, shingle, grain, and piles of shot; all which media are influenced by friction between the particles.

The dynamical properties of media, composed of ideal smooth particles *in a high state of agitation*, have formed the subject of very long and successful investigations, resulting in the dynamical theory of fluids. Also, the limiting conditions of equilibrium of such media as sand, have been made the subject of theoretical treatment by the aid of certain assumptions.

These investigations, however, by no means constitute a complete theory of granular masses; nor does it appear that any attempts have been made to investigate the dynamical properties of a medium consisting of smooth hard particles, held in contact by forces transmitted through the medium. It has sometimes been assumed that such a medium would possess the properties of a liquid, although in the molecular hypothesis of liquids now accepted, the particles are assumed to be in a high state of motion, holding each other apart by collisions; such motion being rendered necessary to account for the property of diffusion.

* This Paper was read before Section A of the British Association at the Aberdeen Meeting, September 10, 1885, and again before Section B, at the request of the Section, September 15.

Without attempting anything like a complete dynamical theory, which will require a large development of mathematics, I would point out the existence of a singular fundamental property of such granular media, which is not possessed by known fluids or solids. On perceiving something which resembles nothing within the limits of one's knowledge, a name is a matter of great difficulty. I have called this unique property of granular masses "dilatancy," because the property consists in a definite change of bulk, consequent on a definite change of shape or distortional strain, any disturbance whatever causing a change of volume and generally dilatation.

In the case of fluids, volume and shape are perfectly independent; and although in practice it is often difficult to alter the shape of an elastic body without altering its volume, yet the properties of dilatation and distortion are essentially distinct, and are so considered in the theory of elasticity. In fact there are very few solid bodies which are to any extent dilatable at all.

With granular media, the grains being sensibly hard, the case is, according to the results I have obtained, entirely different. So long as the grains are held in mutual equilibrium by stresses transmitted through the mass, every change of relative position of the grains is attended by a consequent change of volume; and if in any way the volume be fixed, then all change of shape is prevented.

In speaking of a granular medium, it is assumed to be in such a condition that the position of any internal particle becomes fixed, when the positions of the surrounding particles are fixed.

This condition is very generally fulfilled, but not always where there is friction; without friction it would be always fulfilled.

From this assumption it at once follows, that no grain in the interior can change its position in the mass by passing between the contiguous grains without disturbing these; hence, whatever alterations the medium may undergo, the same particle will always be in the same neighbourhood.

If, then, the medium is subject to an internal strain, the shapes of the internal groups of molecules will all be altered, the shape of each elementary group being determined by the shape of the surrounding particles. This will be rendered most intelligible by considering instances; that of equal spheres is the most general, and presents least difficulty.

A group of such spheres being arranged in such a manner that, if the external spheres are fixed, the internal ones cannot move, any distortion of the boundaries will cause an alteration of the mean density, depending on the distortion and the arrangement of the spheres. For example:—

If arranged as a pile of shot as in Fig. 2, which is an arrangement of tetrahedra and octahedra, the density of the media is $\frac{1}{\sqrt{2}}\frac{\pi}{3}$, taking the density of the sphere as unity.

If arranged in a cubical formation, as in Fig. 1, the density is $\frac{\pi}{6}$, or $\sqrt{2}$ times less than in the former case.

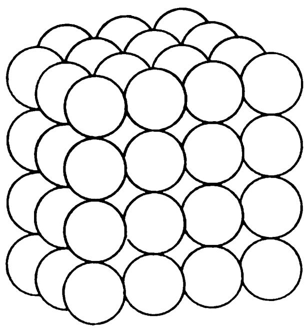


Fig. 1.

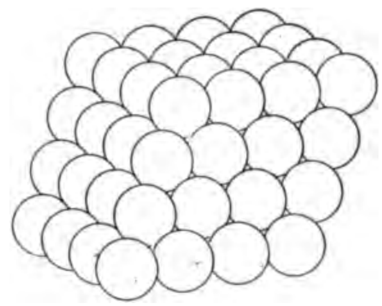


Fig. 2.

These arrangements are both controlled by the bounding spheres; and in either case the distortion necessitates a change of volume.

Either of these forms can be changed into the other by changing the shape of the bounding surface.

In both these cases the structure of the group is crystalline, but that is on account of the plane boundaries.

Practically, when the boundaries are not plane, or when the grains are of various sizes or shapes, such media consist of more or less crystalline groups having their axes in different directions, so that their mean condition is amorphous.

The dilatation consequent on any distortion for a crystalline group may be definitely expressed. When the mean condition is amorphous, it becomes difficult to ascertain definitely what the relations between distortion and dilatation are. But if, when at maximum density, the mean condition is not only amorphous but isotropic, a natural assumption seems to be, that any small contraction from the condition of maximum density in one direction, means an equal extension in two others at right angles.

As such a contraction in one direction continues, the condition of the medium ceases to be isotropic, and the relation changes until dilatation ceases. Then a minimum density is reached; after this, further contraction in the same direction causes a contraction of volume, which continues until a maximum density is reached. Such a relation between the contraction in one direction, and the consequent dilatation, would be expressed by

$$e - 1 = e_1 \sqrt{\sin^2 \frac{a}{e_1}};$$

e being the coefficient of dilatation, a that of contraction, and e_1 the maximum dilatation; the positive root only to be taken.

The amorphous condition of minimum volume is a very stable condition; but there would be a direct relation between the strains and stresses in any other condition if the particles were frictionless and rigid.

If the particles were rigid, the medium would be absolutely without resilience, and hence the only energy of which it would be susceptible would be kinetic energy; so that, supposing the motion slow, the work done upon any group in distorting it would be zero. Thus, supposing a contraction in one direction and expansion at right angles, then if p_x be the stress in the direction of contraction, and p_y, p_z the stress at right angles, a being the contraction, b and c expansions,

$$p_x a + p_y b + p_z c = 0;$$

or, supposing $b = c$, $p_y = p_z$,

$$p_x a + p_y (a + c) = 0.$$

With friction the relation will be different; the friction always opposes strain, *i.e.* tends to give stability.

It is a very difficult question to say exactly what part friction plays; for although we may perhaps still assume without error,

$$\frac{p_y}{p_x} = \frac{1 - \sin\phi}{1 + \sin\phi},$$

where ϕ is the angle of repose, we cannot assume that $\tan\phi$ has any relation to the actual friction between the molecules.

The extreme value of ϕ is a matter of arrangement; as in the case of shot, which would pile equally well although without friction.

Supposing the grains rigid, the relations between distortion and dilatation are independent of friction; that is to say, the same distortion of any bounding surfaces must mean the same internal distortion whatever the friction may be.

The only possible effect of friction would be to render the grains stable under circumstances under which they would not otherwise be stable; and hence we might, with friction, be able to bring about an alteration of the boundaries other than the alteration possible without friction; and thus we ight possibly obtain a dilatation due to friction. How far this is the case an be best ascertained by experiment.

In the case of a granular medium, friction may always be relaxed by elieving the mass of stress, and any stability due to this cause would be hown by shaking the mass when in a condition of no stress.

But before applying this test, it is necessary to make perfectly sure that uring the shaking the boundary spheres do not change position.

Another test of the effect of friction is, by comparing the relative ilatation and distortion with different degrees of friction. If the dilatation ere in any sense a consequence of friction, it would be greater when the oefficient of friction between the spheres was greater. Where the granular ass is bounded by solid surfaces, the friction of the grains against these urfaces will considerably modify the results.

The problem presented by frictionless balls is much simpler than that resented in the case of friction. In the former case the theoretical problem ay be attacked with some hope of success. With friction the property is ost easily studied by experiment.

As a matter of fact, if we take means to measure the volume of a mass f solid grains more or less approximately spheres, the property of dilatancy evident enough, and its effects are very striking, affording an explanation f many well-known phenomena.

If we have in a canvas bag any hard grains or balls, so long as the bag is ot nearly full it will change its shape as it is moved about; but when the ack is approximately full, a small change of shape causes it to become erfectly hard. There is perhaps nothing surprising in this, even apart from miliarity; because an inextensible sack has a rigid shape when extended the full, any deformation diminishing its capacity, so that contents which id not fill the sack at its greatest extension fill it when deformed. n careful consideration, however, many curious questions present themelves.

If, instead of a canvas bag, we have an extremely flexible bag of indiaubber, this envelope, when filled with heavy spheres (No. 6 shot), imposes o sensible restraint on their distortion; standing on the table it takes early the form of a heap of shot. This is apparently accounted for by the ct that the capacity of the bag does not diminish as it is deformed. In his condition it really shows us less of the qualities of its granular contents han the canvas bag. But as it is impervious to fluid, it will enable me to easure exactly the volume of its contents.

Filling up the interstices between the shot with water, so that the bag is quite full of water and shot, no bubble of air in it, and carefully closing the mouth, I now find that the bag has become absolutely rigid in whatever form it happened to be when closed.

It is clear that the envelope now imposes no distortional constraint on the shot within it, nor does the water. What, then, converts the heap of loose shot into an absolutely rigid body? Clearly the limit which is imposed on the volume by the pressure of the atmosphere.

So long as the arrangement of the shot is such that there is enough water to fill the interstices, the shot are free, but any arrangement which requires more room, is absolutely prevented by the pressure of the atmosphere.

If there is an excess of water in the bag when the shot are in their maximum density, the bag will change its shape quite freely for a limited extent, but then becomes instantly rigid, supporting 56 lb. without further change. By connecting the bag with a graduated vessel of water, so that the quantity which flows in and out can be measured, the bag again becomes susceptible of any amount of distortion.

Getting the bag into a spherical form, and its contents at maximum density, and then squeezing it between two planes, the moment the squeezing begins the water begins to flow in, and flows in at a diminishing rate until it ceases to draw more water.

The material in the bag is in a condition of minimum density under the circumstances. This does not mean that all the parts are in a condition of minimum density, because the distortion is not the same in all the parts; but some parts have passed through the condition of maximum, while others have not reached it, so that on further distortion the dilatations of the latter balance the contractions of the former. If we continue to squeeze, water begins to flow out until about half as much has run out as came in; then again it begins to flow in. We cannot by squeezing get it back into a condition of uniform maximum density, because the strain is not homogeneous. This is just what would occur if the shot were frictionless; so that it is not surprising to find that, using oil instead of water, or, better (on account of the india-rubber), a strong solution of soap and water, which greatly diminishes the friction, the results are not altered.

On measuring the quantities of water, we find that the greatest quantity drawn in is about 10 per cent. of the volume of the bag; this is about one-third of the difference between the volumes of the shot at minimum and maximum density.

$\frac{1}{\sqrt{2}}$: 1, or 30 per cent. of the latter.

On easing the bag it might be supposed that the shot would return to their initial condition. But that does not follow: the elasticity of form of the bag is so slight compared with its elasticity of volume, that restitution will only take place as long as it is accompanied with contraction of volume.

So long as the point of maximum volume has not been reached, approximate restitution follows quite as nearly as could be expected, considering that friction opposes restitution. But when the squeezing has been carried past the point of maximum volume, then restitution requires expansion; and this the elasticity of shape is not equal to accomplish, so that the bag retains its flattened condition. This experiment has been varied in a great variety of ways.

The very finest quartz sand, or glass balls $\frac{3}{4}$ inch in diameter, all give the same results. Sand is, on the whole, the most convenient material, and its extreme fineness reduces any effect of the squeezing of the india-rubber between the interstices of the balls at the boundaries; which effect is very apparent with the balloon bags, and shot as large as No. 6.

A well-marked phenomenon receives its explanation at once from the existence of dilatancy in sand. When the falling tide leaves the sand firm, as the foot falls on it the sand whitens, or appears momentarily to dry round the foot. When this happens the sand is full of water, the surface of which is kept up to that of the sand by capillary attraction; the pressure of the foot causing dilatation of the sand, more water is required, which has to be obtained either by depressing the level of the surface against the capillary attraction, or by drawing water through the interstices of the surrounding sand. This latter requires time to accomplish, so that for the moment the capillary forces are overcome; the surface of the water is lowered below that of the sand, leaving the latter white or dryer until a sufficient supply has been obtained from below, when the surface rises and wets the sand again. On raising the foot it is generally seen that the sand under the foot and around becomes momentarily wet; this is because, on the distorting forces being removed, the sand again contracts, and the excess of water finds momentary relief at the surface.

Leaving out of account the effect of friction between the balls and the envelope, the results obtained with actual balls, as regards the relation between distortion and dilatation, appear to be the same as would follow if the balls were smooth.

The friction at the boundaries is not important as long as the strain over the boundaries is homogeneous, and particularly if the balls indent themselves into the boundaries, as they do in the case of india-rubber. But with a plane surface, the balls at the boundaries are in another condition from the balls within. The layer of balls at the surface can only vary its density from $2/\sqrt{3}$ to 1. This means that the layer of balls at a surface can slide between that surface and the adjacent layer, causing much less dilatation than would be caused by the sliding of an internal layer within the mass. Hence, where two parts of the mass are connected by such a surface, certain conditions of strain of the boundaries may be accommodated by a continuous stream of balls adjacent to the surface. This fact made itself evident in two very different experiments.

In order to examine the formation which the shot went through, an ordinary glass funnel was filled with shot and oil, and held vertical while more shot were forced up the spout of the funnel. It was expected that the shot in the funnel would rise as a body, expanding laterally so as to keep the funnel full. This seems to have been the effect at the commencement of the experiment; but after a small quantity had passed up it appeared, looking at the side of the funnel, that the shot were rising much too fast, for which, on looking into the top of the funnel, the reason became apparent. A sheet of shot adjacent to the funnel was rising steadily all round, leaving the interior shot at the same level with only a slight disturbance.

In another experiment one india-rubber ball was filled with sand and water; at the centre of this ball was another much smaller ball, communicating through the sides of the outer envelope by means of a glass pipe with an hydraulic pump. It was expected that, on expanding the interior ball by water, the sand in the outer ball would dilate, expanding the outer ball and drawing more water into the intervening sand. This it did, but not to the extent expected. It was then observed that the outer envelope, instead of expanding, generally bulged in the immediate neighbourhood of the point where the glass tube passed through it; showing that this tube acted as a conductor for the sand from the immediate neighbourhood of the interior ball to the outer envelope, just as the glass sides of the funnel had acted for the shot.

As regards any results which may be expected to follow from the recognition of this property of dilatancy,—

In a practical point of view, it will place the theory of earth-pressures on a true foundation. But inasmuch as the present theory is founded on the angle of repose, which is certainly not altered by the recognition of dilatancy, its effect will be mainly to show the real reason for the angle of repose.

The greatest results are likely to follow in philosophy, and it was with a view to these results that the investigation was undertaken.

The recognition of this property of dilatancy places a hitherto unrecognized mechanical contrivance at the command of those who would explain the fundamental arrangement of the universe, and one which, so far as I have been able to look into it, seems to promise great things, besides possessing the inherent advantage of extreme simplicity.

Hitherto no medium has ever been suggested which would cause a statical force of attraction between two bodies at a distance. Such attraction would be caused by granular media in virtue of this dilatancy and stress. More than this, when two bodies in a granular medium under stress are near together, the effect of dilatancy is to cause forces between the bodies, in very striking accordance with those necessary to explain coherence of matter.

Suppose an outer envelope of sufficiently large extent, at first not absolutely rigid, filled with granular media, at its maximum density. Suppose one of the grains of the media commences to grow into a larger sphere; as it grows, the surrounding medium will be pushed outwards radially from the centre of the expanding sphere. Considering spherical envelopes following the grains of the medium, these will expand as the grains move outwards. This fixes the distortion of the medium, which must be contraction along the radii, and expansion along all tangents.

The consequent amount of dilatation depends on the relation of distortion and dilatation, and on the arrangement of the grains in the medium. At first the entire medium will undergo dilatation, which will diminish as the distance from the centre increases. As the expansion goes on, the medium immediately adjacent to the sphere will first arrive at a condition of minimum density; and for further expansion this will be returning to a maximum density, while that a little further away will have reached a minimum. The effect of continued growth will therefore be, to institute concentric undulations of density from maximum to minimum density, which will move outwards; so that after considerable growth, the sphere will be surrounded with a series of envelopes of alternately maximum and minimum density, the medium at a great distance being at maximum density. At a definite distance from the centre of the sphere not more than

$$1{\cdot}4R,$$

where R is the radius of the sphere, the density will be a minimum, and between this and the sphere there may be a number of alternations, depending on the relative diameters of the grains and the spheres.

The distance between these alternations will diminish rapidly as the sphere is approached. The distance of the next maximum is $1{\cdot}2R$, the next minimum is given by $1{\cdot}09R$, and the next maximum $1{\cdot}06R$.

The general condition of the medium around a sphere which has expanded in the medium, is shown in Fig. 3, which has been arrived at on the supposition that the sphere is large compared with the grains.

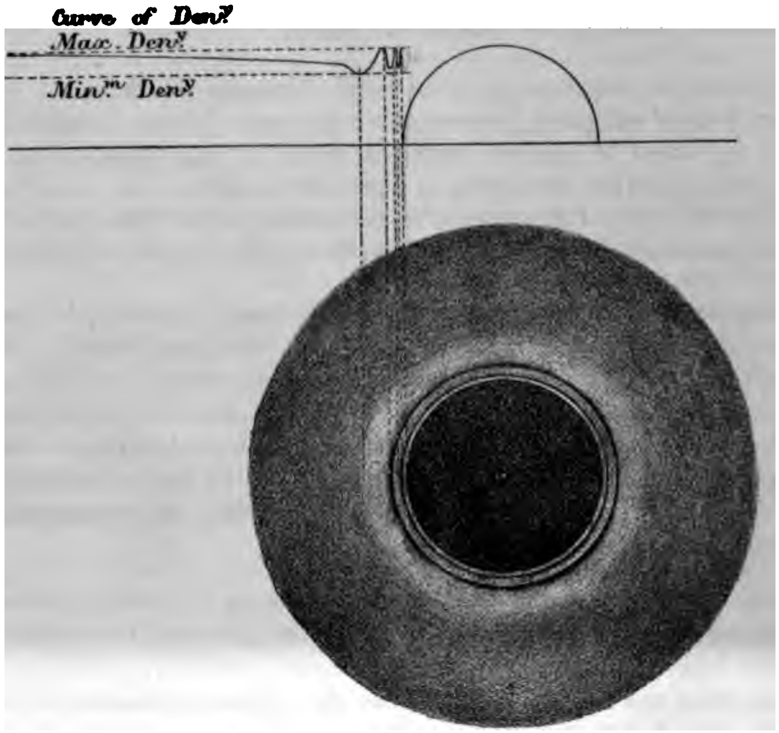


Fig. 3.

From a radius about $1{\cdot}4R$ outwards the density gradually increases, reaching a maximum density at infinity; and at all distances greater than $1{\cdot}8R$ the law is expressed by

$$\frac{de}{dr} = \frac{1}{r^n},$$

where n has some value greater than 3, depending on the structure of the medium.

Within the distance $1{\cdot}4R$ the variation is periodic, with a rapidly diminishing period. In this condition, supposing the medium of unlimited extent and the sphere smooth, the sphere may move without causing further expansion, merely changing the position of the distortion in the medium; for the grains, slipping over the sphere, would come back to their original positions. It thus appears that smooth bodies would move without resistance, if the relation between the size of the grains and bodies is such, that the energy due to the relative motion of the grains in immediate proximity may e neglected. The kinetic energy of the motion of the medium would be roportional to the volume of the ball, multiplied by the density of the nedium, and the square of the velocity.

But the momentum might be infinite, supposing the medium infinite in xtent, in which case a single sphere would be held rigidly fixed.

If we suppose two balls to expand instead of one, and suppose the disortion of the medium for one ball to be the same as if the other were ot there, the result will be a compound distortion. Since, however, the ilatation does not bear a linear relation to the distortion, the dilatation esulting from the compound distortion will not be the sum of the dilatations r the separate distortions, unless we neglect the squares and products of he distortions as small.

Supposing the bodies so far apart that one or other of the separate istortions caused at any point is small, then, retaining squares and products, t appears that the resultant dilatation at any point will be less than the um of the separate dilatations, by quantities which are proportional to the roducts of the separate distortions.

The integrals of these terms through the space bounded by spheres of adii R and L, are expressed by finite terms, and terms inversely proporional to L, which latter vanish if L is infinite. Thus, while the total separate ilatations are infinite, the compound dilatations differ from the sum of the eparate by finite terms, and these are functions of the product of the olumes, and the reciprocal of the distance.

Assuming stress in the medium, the difference in the value of these nite terms for two relative positions of the bodies, multiplied by the tresses, represents an amount of work which must be done by the bodies n the medium in moving from one position to another.

To get rid of the difficulty of infinite extent of medium, if for the moment re assume the envelope sufficiently large and imposing a normal pressure pon the medium, then, since the work done will be proportional to the ilatation, the force between the bodies will be proportional to the rate at hich this dilatation varies with the distance between them.

The force between the bodies would depend on the character of the lasticity, as well as on the dilatation.

It is not necessary to assume the outer envelope elastic; this may be bsolutely rigid, and one or both the balls elastic.

In such case the two balls are connected by a definite kinematic relation. s they approach they must expand, doing work which is spent in producing energy of motion; as they recede, the kinetic energy is spent in the work of compressing the balls.

As already stated, the momentum of the infinite medium for a single ball in finite motion may be infinite, and proportional to the product of the volume of the ball by the velocity; but with two balls moving in opposite directions, with velocities inversely as the masses, the momentum of the system is zero. Therefore such motion may be the only motion possible in a medium of infinite extent.

When the distance between the balls is of the same order as their dimensions, the law of attraction changes with the law of the compound dilatations, and becomes periodic, corresponding to the undulations of density surrounding the balls. Thus, before actual contact was reached, the balls would suffer alternate repulsion and attraction, with positions of equilibrium more or less stable between, as shown in Figs. 4 and 5.

Fig. 4.

Fig. 5.

We have thus a possible explanation of the cohesion and chemical combination of molecules, which I think is far more in accordance with actual experience than anything hitherto suggested.

It was the observation of these envelopes of maximum and minimum density, which led me to look more fully into the property of dilatancy.

The assumed elasticity of the surrounding envelope, or of the balls, has only been introduced to make the argument clear.

The medium itself may be supposed to possess kinetic elasticity arising from internal distortional motion, such as would arise from the transmission of waves, in which the motion of the medium is in the plane of their fronts.

The fitness of a dilatant medium to transmit such waves is only less striking than its property of causing attraction, because in the first respect it is not unique.

But, as far as I can see, such transmission is not possible in a medium :omposed of uniform grains. If, however, we have comparatively large grains iniformly interspersed, then such transmission becomes possible. If, notwith-tanding the large grains, the medium is at maximum density, the large grains will not be free to move without causing further dilatation; and it eems that the medium would transmit distortional vibrations, in which the listortions of the two sets of grains are opposite.

Such waves, although the motion would be essentially in the plane of the vave, would cause dilatation, just as waves in a chain cause contraction in he reach of the chain. They would in fact impart elasticity to the medium, xactly as, in the case of a slack chain having its ends fixed but otherwise iot subject to forces, any lateral motion imparted to the chain will cause ension, proportional to the energy of disturbance divided by the slackness r free length of chain.

Distortional waves therefore, travelling through dilatant material which loes not quite occupy the space in which it is confined when at maximum lensity, would render the medium uniformly elastic to distortion, but not in he same degree to compression or extension. The tension caused by such vaves would depend on the gross energy of motion of the waves, divided y the total dilatation from maximum density consequent on the wave-notion. All such waves, whatever might be their length, would therefore nove with the same velocity.

If, when rendered elastic by such waves, the medium were thrown into a tate of distortion by some external cause, this would diminish the possible ilatation caused by the waves. Thus work would have to be done on the nedium in producing the external distortion, which would be spent in in-reasing the energy of the waves. For instance, the separation of two bodies n such a medium, which, as already shown, would increase the statical istortion, would increase the energy of the waves and *vice versâ*.

As far as the integrations have been carried for this condition of elasticity, t appears, with a certain arrangement of large and small grains, that the orces between the bodies would be proportional to the product of the olumes divided by the square of the distance; *i.e.* that the state of stress f the medium may be the same as Maxwell has shown must exist in the ther to account for gravity. We have thus an instance of a medium, ransmitting waves similar to heat-waves, and causing force between bodies imilar to the forces of gravitation and cohesion, in such a manner as to onstitute a conservative system. More than this, by the separation of the wo sets of grains, there would result phenomena similar to those resulting rom the separation of the two electricities. The observed conducting power f a continuous surface for the grains of a medium, closely resembles the conduction of electricity. And such a composite medium would be susceptible of a state in which the arrangement of the two sets of grains were thrown into opposite distortions, which state, so far as it has yet been examined, appears to coincide with the state of a medium necessary to explain electrodynamic and magnetic phenomena according to Maxwell's theory.

In this short sketch of the results which it appears to me may follow from the recognition of the property of dilatancy, I have not attempted to follow the exact reasoning even so far as I have carried it.

In the preliminary acceptance of a theory, the mind must be guided rather by a general view of its adaptability, than by its definite accordance with some out of many observed facts. And as it seems, after a preliminary investigation, that in space filled with discrete particles, endowed with rigidity, smoothness, and inertia, the property of dilatancy would cause amongst other bodies, not only one property, but all the fundamental properties of matter, I have, in pointing out the existence of dilatancy, ventured to call attention to this dilatant or kinematic theory of ether, without waiting for the completion of the definite integrations, which must take long, although it is by these that the fitness of the hypotheses must be eventually tested.

51.

EXPERIMENTS SHOWING DILATANCY, A PROPERTY OF GRANULAR MATERIAL, POSSIBLY CONNECTED WITH GRAVITATION.

[*From the* "Proceedings of the Royal Institution of Great Britain."]

(*Read* February 12, 1886.)

IN commencing this discourse, the author said, My principal object to-night is to show you certain experiments which I have ventured to think would interest you on account of their novelty, and of their paradoxical character. It is not, however, solely or chiefly on account of their being curious that I venture to call your attention to them. Let them have been never so striking, you would not have been troubled with them, had it not been that they afford evidence of a fact of real importance in mechanical philosophy.

This newly recognised property of granular masses, which I have called dilatancy, will, it may be hoped, be rendered intelligible by the experiments, but it was not by these experiments that it was discovered.

This discovery, if I may so call it, was the result of an attempt to conceive the mechanical properties a medium must possess, in order that it might fulfil the functions of an all-pervading ether—not only in transmitting waves of light, and refusing to transmit waves like those of sound, but in causing the force of gravitation between distant bodies, and actions of cohesion, elasticity, and friction between adjacent molecules, together with the electric and magnetic properties of matter, and at the same time allowing the free motion of bodies.

It will be well known to those who attend the lectures in this room, that although a vast increase has been achieved in knowledge of the actions called the physical properties of matter, we have as yet no satisfactory explanation as to the *prima causa* of these actions themselves; that to explain the transmission of light and heat, it has been found necessary to assume space filled with material possessing the properties of an elastic jelly, the existence of which, though it accounts for the transmission of light, has hitherto seemed inconsistent with the free motion of matter, and failed to afford the slightest reason for the gravitation, cohesion, and other physical properties of matter. To explain these, other forms of ether have been invented, as in the corpuscular theory and the celebrated hypothesis of La Sage, the impossibilities of which hypotheses have been finally proved by the late Professor Maxwell, to whom we owe so much of our definite knowledge of the fundamental physics. Maxwell insisted on the fact, that even if each of the physical properties could be explained by a special ether, it would not advance philosophy, as each of these ethers would require another ether to explain its existence, *ad infinitum*. Maxwell clearly contemplated the existence of one medium, but it was a medium which would cause not one but all the physical properties of matter. His writings are full of definite investigations as to what the mechanical properties of this ether must be, to account for the laws of gravitation, electricity, magnetism, and the transmission of light, and he has proved very clear and definite properties, although, as he distinctly states, he was unable to conceive a mechanism which should possess these properties.

As the result of a long-continued effort to conceive a mechanical system possessing the properties assigned by Maxwell, and further, which would account for the cohesion of the molecules of matter, it became apparent that the simplest conceivable medium—a mass of rigid granules in contact with each other—would answer not one but all the known requirements, provided the shape and mutual fit of the grains were such, that while the grains rigidly preserved their shape, the medium should possess the apparently paradoxical, or anti-sponge property, of swelling in bulk as its shape was altered.

I may here remark, that if ether is atomic or granular, that it should be a mass of grains holding each other in position by contact, like the grains in the sack of corn, is one of only two possible conceptions; the other being that of La Sage, or the corpuscular theory that the grains are free like bullets, moving in space in all directions.

Nor, in spite of its paradoxical sound, is there any great difficulty of conceiving the swelling in bulk. When the grains are in contact, it appears at once that the mechanical properties of the medium must be to some extent affected by the shape and fit of the grains. And having arrived at the conclusion, that in order to act the part of ether, this shape and fit must be such that the mass could not change its shape, without changing its volume or space occupied, the next thing was to see what possible shape could be given to the grains, so that while these rigidly preserved their shape, the medium might possess this property of dilatancy.

It was obvious that the grains must so interlock, that when any change of shape of the mass occurred, the interstices between the grains should increase. This would be possessed by grains shaped to fit into each other's interstices in one particular arrangement.

In an ordinary mass of brickwork or masonry well bonded without mortar, the blocks fit so as to have no interstices; but if the pile be in any way distorted, interstices appear, which shows that the space occupied by the entire mass has increased. (*Shown by a model.*)

At first it appeared that there must be something special and systematic, as in the brick wall, in the fit of the grain of ether, but subsequent consideration revealed the striking fact, *that a medium composed of grains, of any possible shape, possessed this property of dilatancy, so long as one important condition was satisfied.*

This condition is, that the medium should be continuous, infinite in extent, or that the grains at the boundary should be so held as to prevent a rearrangement commencing. All that is wanted is a mass of hard smooth grains, each grain being held by the adjacent grains, and the grains on the outside prevented from rearranging.

Smooth hard spheres arranged as an ordinary pile of shot are in their closest order, the interstices occupying a space about one-third that occupied by the spheres themselves. By forcing the outside shot so as to give the pile a different shape, the inside spheres are forced by those on the outside, and the interstices increase. Thus by shaping the outside of the pile, the interstices may be increased to any extent, until they occupy about nine-tenths of the volume of the spheres: this is the most open formation. A further change of shape in the same direction causes a contraction of the interstices, until a minimum volume is reached, and then again an expansion, and so on. The point to be realised is, that in any of these arrangements, if the whole of the spheres on the outside of the group are fixed, those inside will be fixed also. (*Shown by a model.*)

An interior portion of a mass of smooth hard spheres therefore cannot have its shape changed by the surrounding spheres, without altering the room it occupies, and the same is true for any granular mass, whatever be the shape of the grains.

Considering the generality of this conclusion, the non-discovery of this property as existing in tangible matter, requires a word of explanation.

The physical properties of elasticity, adhesion, and friction, so far render the molecules of ordinary matter incapable of behaving as a system of parts with the sole property of keeping their shape, and so prevent evidence of dilatancy in solids and fluids. This is quite consistent with dilatancy in the ether, for the properties of elasticity, cohesion, and friction, in tangible matter, are due to the presence of the ether, so that it would be illogical for the elementary atoms of the ether to possess these properties.

This, although a sufficient reason why dilatancy has not been recognised as a property of solid and fluid matter, does not explain its non-existence in masses of solid, hard, free grains, as of corn, shot, and sand. To understand why it has not been observed in these, it must be remembered that, to ordinary observation, these present only an outside appearance, and that the condition essential for dilatancy, that the outside grains should not be free to rearrange, is seldom fulfilled. Also these granular forms of matter, though commonplace, have not been the subjects of physical research, and hence such evidence as they do afford has escaped detection.

Once, however, having recognised dilatancy as a universal property of granular masses, it was obvious that if evidence of it was to be sought from tangible matter, it must be sought in what have hitherto been the most commonplace and least interesting arrangements. That an important geometrical and mechanical property of a material system should have been hidden for thousands of years, even in sand and corn, is such a striking thought, that it required no little faith in mechanical principles to undertake the search for it, and although finding nothing but what was strictly in accordance with the conclusions previously arrived at, the evidence obtained of this long-hidden property was as much a matter of visual surprise to the lecturer, as it can be to any of the audience.

To render the dilatancy of a granular mass evident, it was necessary to accomplish two things: (1) the outside grains must be controlled so that they could not rearrange, and this without preventing change of shape and bulk of the mass; (2) the changes of bulk or volume of the mass, or of the interstices between the grains, must be rendered evident by some method of measurement which did not depend on the shape of the mass.

A very simple means—*a thin india-rubber* envelope or boundary—answered both these purposes to perfection. The thin india-rubber closed over the outside grains sufficiently to prevent their change of position, and the impervious character of the bag allowed of a continuous measure of the volume of the contents, by measuring the quantity of air or water necessary to fill the interstices.

Taking an india-rubber bag which will hold six pints of water, without stretching, and having only a small tubular aperture, getting it quite dry, and putting into it six pints of dry sea sand, such as will run in an hour-glass, sharp river sand, dry corn, shot or glass marbles, it presents no very striking appearance, but all the same when filled with any of these materials, it cannot have its form changed, as by squeezing between two boards, without changing its volume. These changes of volume are not sufficient to be noticeable while the squeezing is going on, but they may be rendered apparent. It is sufficient to do this with the bag full of clean dry Calais sand, such as is used in an hour-glass.

The tube from the bag is connected with a mercurial pressure-gauge, so that the bag is closed by the mercury.

The actual volume occupied by the quartz grains is four and a half pints. The remaining space, one and a half pints, is occupied by the interstices between the grains in their closest order; these interstices are full of air, so that three-quarters of the bag are occupied by quartz, and one-quarter by air. Since the bag is closed, and no more air can get in, if interstices are increased from one pint and a half to two pints, the air must expand, and its pressure will fall from that of the atmosphere to three-quarters of an atmosphere. As soon as squeezing begins, the mercury rises on the side connected with the bag, and steadily rises as the bag flattens, until it has risen seven inches, showing that the bag has increased in capacity by half a pint, or one-twelfth of its initial capacity.

That by squeezing a porous mass like sand we should diminish the pressure of the air in the pores is paradoxical, and shows the anti-sponginess of the granular material; had there been a sponge in the bag, the pressure of the air would have increased with the squeezing.

This experiment has been mainly introduced to prevent a possible impression that the fluid filling the interstices has anything to do with the dilatation besides measuring it.

Water affords a more definite measure of volume than air.

Taking a small india-rubber bottle with a glass neck full of shot and water, so that the water stands well into the neck. If instead of shot the bag were full of water, or had anything of the nature of a sponge in it, when the bag was squeezed the water would be forced up the neck. With the shot the opposite result is obtained; as I squeeze the bag, the water decidedly shrinks in the neck.

This experiment, which you see is on a very small scale, was not designed to show to an audience; it was the original experiment which was made for my own satisfaction, when the idea of dilatancy first presented itself. The result, but for the knowledge of dilatancy, would appear paradoxical, not to say magical. When we squeeze a sponge between two planes, water is squeezed out; when we squeeze sand, shot, or granular material, water is drawn in.

Taking a larger apparatus, a bag which holds six pints of sand, the interstices of which are full of water without any air—the glass neck being graduated so as to measure the water drawn in. On squeezing the bag with a large pair of pincers, a pint of water is drawn from the neck into the bag. This is the maximum dilatation; the grains of sand are now in the most open order into which they can be brought by this squeezing; further squeezing causes them to take closer order, the interstices diminish, and the water runs out into the vessel, and for still further squeezing is drawn back again, showing that as the change of form continues, the medium passes through maximum and minimum dilatations.

This experiment may be repeated with granules of any size or shape, provided they are hard, and shows the universality of dilatancy.

Although not more definite, perhaps more striking evidence of dilatancy is afforded by the means which the non-expansibility of water affords of limiting the volume of the bag. An impervious bag full of sand and water without air cannot have its contents enlarged without creating a vacuum inside it—the interstices of the sand are therefore strictly limited to the volume of the water inside it, unless forces are brought to bear sufficient to overcome the pressure of the atmosphere and create a vacuum. Since then, owing to this property of dilatancy, the shape of a granular mass at its greatest density cannot change without enlarging the interstices, if we prevent this enlargement by closing the bag we prevent change of shape.

Taking the same bag, the sand being at its closest order—and closing the neck so that it cannot draw more water. A severe pinch is put on the bag, but it does not change its shape at all; the shape cannot alter without enlarging the interstices, which cannot enlarge without drawing more water, and this is prevented. To show that there is an effort to enlarge going on, it is only necessary to open a communication with a pressure-gauge, as in the experiment with air. The mercury rises on the side of the bag, showing when the pinch is hardest (about 200 lbs. on the planes) that the pressure in the bag is less by 27 inches of mercury than the pressure of the atmosphere; a little more squeezing and there is a vacuum in the bag. Without a knowledge of the property of dilatancy such a method of producing a vacuum would sound somewhat paradoxical. Opening the neck to allow the entrance of water, the bag at once yields to a slight pressure, changing shape, but this change at once stops when the supply is cut off, preventing further dilatation.

In these experiments neither the thickness of the bag, nor the character of the fluid, has anything to do with the dilatation of the contents, considered as forming an interior group of a continuous medium, the bag merely controlling the outside members as they would be controlled by surrounding grains, and the fluid merely measuring or limiting the volume of the interstices.

It has, however, been absence of such control of the outside grains, and such means of measuring the volume of the interstices, that has prevented the dilatancy revealing itself as a general mechanical property of granular material; as a *mechanical* property, because dilatancy has long been known to those who buy and sell corn. It is seldom left for the philosopher to discover anything which has a direct influence on pecuniary interests; and when corn was bought and sold by *measure*, it was in the interest of the vendor to make the interstices as large as possible, and of the vendee to make them as small; of the vendor to make the corn lie as lightly as possible, and of the vendee to get it as dense as possible. These interests are obvious; but the methods of getting corn dense and light are *paradoxical* when compared with the methods for other material. If we want to get any elastic material light we shake it up, as a pillow or a feather bed, or a basket of dried fruit; to get these dense we squeeze them into the measure. With corn it is the reverse; it is no good squeezing it to get it dense; if we try to press it into the measure we make it light—to get it dense we must shake it—which, owing to the surface of the measure being free, causes a rearrangement in which the grains take the closest order.

At the present day the measure for corn has been replaced by the scales, but years ago corn was bought and sold by measure only, and measuring was then an art which is still preserved. It is understood that the corn is to be measured light, and the method employed is now seen to have made use of the property of dilatancy. The measure is filled over full and the top struck with a round pin called the strake or strickle. The universal art is to put the strake end on into the measure before commencing to fill it. Then when heaped full, to pull the strake gently out and strike the top; if now the measure be shaken it will be seen that it is only nine-tenths full.

Sand presents many striking phenomena well known but not hitherto explained, which are now seen to be simply evidence of dilatancy.

Every one who walks on the strand must have been painfully struck with the difference in the firmness and softness of the sand at different times; letting alone when it is quite dry and loose. At one time it will be so firm and hard that you may walk with high heels without leaving a footprint; while at others, although the sand is not dry, one sinks in so as to make walking painful. Had you noticed you would have found that the sand is firm as the tide falls, and becomes soft again after it has been left dry for some hours. The reason for this difference is exactly the same as that of the closed bags with water and air in the interstices of the sand. The tide leaves the sand, though apparently dry on the surface, with all its interstices perfectly full of water, which is kept up to the surface of the sand by capillary attraction; at the same time the water is percolating through the sand from the sands above, where the capillary action is not sufficient to hold the water. When the foot falls on this water-saturated sand, it tends to change its shape, but it cannot do this without enlarging the interstices—without drawing in more water. This is a work of time, so that the foot is gone again before the sand has yielded. If you stand still, you will find that your feet sink more or less, and that when you move, the sand becomes wet all round the space you stood on, which is the excess of water you have drawn in, set free by the sand regaining its densest form.

One phenomenon attending walking on firm sand is very striking; as the foot falls, the sand all round appears to shoot white or dry momentarily, soon becoming dark again. This is the suction into the enlarging interstices below the foot, which for the moment depresses the capillary surface of the water below that of the sand.

After the tide has left the sand for a sufficient time, the greater part of the water has run out of the interstices, leaving them full of air, which by expanding allows the interstices to enlarge, and the foot to sink in far enough to make walking unpleasant.

If we walk on sand under water, it is always more or less soft, for the interstices can enlarge, drawing in water from above.

The firmness of the sand is thus seen to be due to the interstices being full of water, and to the capillary action or surface tension of the water at the surface of the sand. This capillary action will hold the water up in the sand for some inches or feet, according to the fineness of the sand. This is shown by a somewhat striking experiment. If sand running in a stream from a small hole in the bottom of a vessel, as in an hour-glass, fall into a vessel containing a slight depth of water, the sand at first forms an island, which rises above the water. The sand which then falls on the top of this island is dry as it falls, but capillary action draws up the water which fills the interstices and gives the sand coherence. The island grows vertically, very fast, and assumes the form of a column, sometimes with branches like a tree or a fern, some inches or even a foot high. The strength of these consists in the surface tension of the water preventing air from being drawn in to enlarge the interstices, which therefore cannot change shape; it is therefore another evidence of dilatancy.

By substituting an impervious envelope for the surface of water, firmness of sand saturated with water may be rendered very striking.

Thin india-rubber balloons, which may be easily expanded with the mouth, afford an almost transparent envelope.

Taking one containing about six pints of sand and water, closed without air, there being more water than will fill the interstices at the densest, but not enough to allow them to enlarge to the full extent. When standing on the table, the elasticity of the envelope gives it a rounded shape. The sand has settled down to the bottom, and the excess of water appears above the sand, the surface of which is free. The bag may be squeezed and its shape altered, apparently as though it had no firmness, but this is only so long as the surface is free. But taking it between two vertical plates and squeezing, at first it submits, apparently without resistance, when all at once it comes to a dead stop. Turning it on to its side, a 56-lb. weight produces no further alteration of shape; but on removing the weight, the bag at once returns to its almost rounded shape.

Putting the bag now between two vertical plates, and slightly shaking while squeezing, so as to keep the sand at its densest, while it still has a free surface, it can be pressed out until it is a broad flat plate. It is still soft as long as it is squeezed, but the moment the pressure is removed, the elasticity of the bag tends to draw it back to its rounded form, changing its shape, enlarging the interstices, and absorbing the excess of water; this is soon gone, and the bag remains a flat cake with peculiar properties. To pressures on its sides it at once yields, such pressures having nothing to overcome but the elasticity of the bag, for change of shape in that direction causes the sand to contract. To radial pressures on its rim, however, it is perfectly rigid, as such pressures tend further to dilate the sand; when placed on its edge, it bears one cwt. without flinching.

If, however, while supporting the weight it is pressed sufficiently on the sides, all strength vanishes, and it is again a rounded bag of loose sand and water.

By shaking the bag into a mould, it can be made to take any shape; then, by drawing off the excess of water and closing the bag, the sand becomes perfectly rigid, and will not change its shape without the envelope be torn; no amount of shaking will effect a change. In this way bricks can be made of sand or fine shot full of water and the thinnest india-rubber envelope, which will stand as much pressure as ordinary bricks without change of shape; also permanent casts of figures may be taken.

I have now shown, as fully as time will allow, the experiments which afford evidence of the existence of the property of dilatancy, and how it explains natural phenomena hitherto but little noticed.

Beyond affording evidence of the existence of the property dilatancy, these experiments have no direct connection with gravitation or the physical properties of matter.

These properties cannot be deduced by direct experiment on granular material, for the simple reason that the grains of the medium which constitutes the ether must be free from friction, while the grains with which we work are subject to friction. These properties can only be deduced by mathematical reasoning, into which I will not drag you to-night. I will merely point out two or three facts, which may serve to convey an idea of how dilatancy should have such a bearing on the foundation of the universe.

If you look at this diagram, you see it represents a ball surrounded by a continuous mass of grain, the density of the grains being indicated by the depth of colour. If that ball were to grow in volume, it would have to push out the medium on all sides, and in that way it would distort the groups of grains, or change their form, causing the interstices to increase; those nearer the ball would be distorted more than those further away. Then the interstices of these would grow the most rapidly, and those adjacent to the ball would first come to their openest order for further growth; these would contract somewhat, those a little further away would reach the openest order, and if the process of growth steadily continued, we should have a series of undulations of density, commencing at the ball and moving outwards; the first of these waves of open order would not, however, get beyond half the diameter of the ball away. The diagram represents the interstices that would result, if a single grain of the material had grown to the size of the ball, pushing the medium out before it. It is not necessary that the ball should have grown, to produce this result; however the ball were originally placed, if it were moved away from its original place, it would assume this arrangement, and with this arrangement it would be free to move. Now, although I cannot attempt to enter upon the relation between the density of the medium, and the force of attraction between two bodies in it, I may call your attention to this fact, that the dilatation as calculated, varies exactly as the force of gravitation, inversely as the square of the distance from an infinite distance till close to the ball, and then goes through several undulations, corresponding exactly to the variations in the attraction of bodies necessary to explain the elasticity and cohesion of molecules. As is shown in the other diagrams, these undulations in density, which may be experimentally produced, not only appear to afford a clear explanation of cohesion, but are the only suggestion of an explanation ever made. And further, similar undulations have been found necessary to explain one of the phenomena of light. My reason for calling your attention to them was partly an experiment, which, although not the most striking, is the most advanced experiment in the direction of dilatancy.

The apparatus is that represented in the diagram; the medium is contained in the large elastic bag; in the middle of this bag is a small hollow elastic ball, which can be expanded by water forced in through a tube passing through the medium and outside ball; the quantity of water which passes in is measured by a mercury gauge, the water being forced in by the pressure of the mercury. The medium between the two balls is sand and water, and is connected with a gauge, the water drawn from which measures the dilatation.

The full pressure of 30 inches is on the interior ball, but produces no expansion, because the medium outside cannot dilate, as the supply of water is now cut off; opening the tap to admit water to the outer ball, it at once draws water. It has now drawn 3 oz.; in the meantime the mercury has fallen, showing that an ounce and a half was admitted to the interior ball, the expansion of which drew the water into the outer envelope. This experiment is not striking, but it is definite, and enables us to measure the dilatation consequent on a given distortion.

It is impossible for me to go further into this explanation, so I will merely state that the ability of the grains of a medium to slide over a smooth surface has been experimentally shown to produce phenomena closely resembling the conduction of electricity, to complete which it is only necessary to construct the medium of two different sorts of grains, different in size or different in shape, the separation of which would afford the two electricities, and be a simple way out of the difficulty hitherto found in explaining the non-exhaustibility of the electricity in a body. Hitherto the two electric fluids have been supposed to reside together in the matter of the machine, which, however much has been withdrawn, has never shown signs of exhaustion. In the dilatant hypothesis, these electricities are the two constituents of the ether which the machine separates, and it is worth noticing that the ordinary electrical machine resembles in all essential particulars the machines used by seedsmen for separating two kinds of seed, trefoil and rye-grass, which grow together: as long as there is a supply of the mixture, the machine is never exhausted.

This dilatant hypothesis of ether is very promising, although it cannot be put forward as proved until it has been worked out in detail, which will take long. In the meantime it is put forward mainly to excite interest in the property of dilatancy, to the discovery of which it has led. This property, now that it has once been recognised, is quite independent of any hypothesis, and offers a new field for philosophical and mathematical research quite independent of the ether.

52.

ON THE THEORY OF LUBRICATION AND ITS APPLICATION TO MR BEAUCHAMP TOWER'S EXPERIMENTS, INCLUDING AN EXPERIMENTAL DETERMINATION OF THE VISCOSITY OF OLIVE OIL.

[*From the* "Philosophical Transactions of the Royal Society," Part I., 1886.]

SECTION I.—INTRODUCTORY.

1. **LUBRICATION, or the action of oils and other viscous fluids to diminish friction and wear between solid surfaces, does not appear to have hitherto** formed a subject for theoretical treatment. Such treatment may have been prevented by the obscurity of the physical actions involved, which belong to a class as yet but little known, namely, the boundary or surface actions of fluids; but the absence of such treatment has also been owing to the want of any general laws discovered by experiment.

The subject is of such fundamental importance in practical mechanics, and the opportunities for observation are so frequent, that it may well be a matter of surprise that any general laws should have for so long escaped detection.

Besides the general experience obtained, the friction of lubricated surfaces has been the subject of much experimental investigation by able and careful experimenters. But, although in many cases empirical laws have been propounded, these fail for the most part to agree with each other and with the more general experience.

2. The most recent investigation is that of Mr Beauchamp Tower, undertaken at the instance of the Institution of Mechanical Engineers. Mr Tower's first report was published November, 1883, and his second report in 1884 (*Proc. Inst. Mechanical Engineers*).

In these reports Mr Tower, making no attempt to formulate, states the results of experiments apparently conducted with extreme care and under very various and well-chosen circumstances. Those results which were obtained under the ordinary conditions of lubrication so far agree with the results of previous investigators as to show a want of any regularity. But one of the causes of this want of regularity, irregularity in the supply of the lubricant, appears to have occurred to Mr Tower early in his investigation, and led him to include amongst his experiments the unusual circumstances of surfaces completely immersed in oil. This was very fortunate, for not only do the results so obtained show a great degree of regularity, but while making these experiments he was accidentally led to observe a phenomenon which, taken with the results of his experiments, amounts to a crucial proof that in these experiments with the oil bath the surfaces were completely and continuously separated by a film of oil; this film being maintained by the motion of the journal, although the pressure in the oil at the crown of the bearing was shown by actual measurement to be as much as 625 lbs. per sq. inch above the pressure in the oil bath.

These results obtained with the oil bath are very important, notwithstanding that the condition is not common in practice. They show that with perfect lubrication a definite law of variation of the friction with the pressure and velocity holds for a particular journal and brass. This strongly implies that the irregularity previously found was due to imperfect lubrication. Mr Tower has brought this out:—Substituting for the bath an oily pad, pressed against the free part of the journal, and making it so slightly greasy that it was barely perceptible to the touch, he again found considerable regularity in the results; these, however, were very different from those with the bath. Then with intermediate lubrication he obtained intermediate results, of which he says:—"Indeed, the results, generally speaking, were so uncertain and irregular that they may be summed up in a few words. The friction depends on the quantity and uniform distribution of the oil, and may be anything between the oil bath results and seizing, according to the perfection or imperfection of the lubrication."

3. On reading Mr Tower's report it occurred to the author as possible that, in the case of the oil bath, the film of oil might be sufficiently thick for the unknown boundary actions to disappear, in which case the results would be deducible from the equations of hydrodynamics. Mr Tower appears to have considered this, for he remarks that according to the theory of fluid friction the resistance would be as the square of the velocity, whereas in his results it does not increase according to this law. Considering how very general the law of resistance as the square of the velocity is with fluids, there is nothing remarkable in the assumption of its holding in such a case.

But the study of the behaviour of fluid in very small channels, and particularly the recent determination by the author of the critical velocity at which this law changes from that of the square of the velocity to that of the simple ratio, shows that with such highly viscous fluids as oils, such small spaces as those existing between the journal and its bearing, and such limited velocity as that of the surface of the journal, the resistance would vary, *cœteris paribus*, as the velocity. Further, the thickness of the oil film would not be uniform and might be affected by the velocity, and as the resistance would vary, *cœteris paribus*, inversely as the thickness of the film, the velocity might exert in this way a secondary effect on the resistance; and, further still, the resistance would depend on the viscosity of the oil, and this depends on the temperature. But as Mr Tower had been careful to make all his experiments in the same series with the journal at a temperature of 90° Fahr., it did not at first appear that there could be any considerable temperature effect in his results.

4. The application of the hydrodynamical equations to circumstances similar in so far as they were known to those of Mr Tower's experiments, at once led to an equation between the variation of pressure over the surface and the velocity, which equation appeared to explain the existence of the film of oil at high pressure. This equation was mentioned in a paper read before Section A. of the British Association at Montreal, 1884. It also appears from a paragraph in the President's Address (*Brit. Assoc. Rep.*, 1884, p. 14) that Professor Stokes and Lord Rayleigh had simultaneously arrived at a similar result. At that time the author had no idea of attempting its integration. On subsequent consideration, however, it appeared that the equation might be transformed so as to be approximately integrated, and the theoretical results thus definitely compared with the experimental.

5. The result of this comparison was to show that with a particular journal and brass, the mean thickness of the film of oil would be sensibly constant, and hence, if the viscosity was constant, the resistance would increase directly as the speed. As this was not in accordance with Mr Tower's experiments, in which the resistance increased at a much slower rate, it appeared that either the boundary actions became sensible, or that there must have been a rise in the temperature of the oil which had escaped the thermometers used to measure the temperature of the journal.

That there would be some excess of temperature in the oil film, on which all the work of overcoming the friction is spent, is certain; and after carefully considering the means of escape of this heat, it seems probable that there would be a difference of several degrees between the oil bath and the film of oil.

This increase of temperature would be attended by a diminution of viscosity, so that, as the resistance and temperature increased with the velocity, the viscosity would diminish and cause a departure from the simple ratio.

6. In order to obtain a quantitative estimate of these secondary effects, it was necessary to know exactly the relation between the viscosity and temperature of the lubricant used. For this purpose an experimental determination was made of the viscosity of olive oil at different temperatures as compared with the known viscosity of water. From the results of these experiments an empirical formula has been deduced, by means of which definite expressions have been obtained for the approximate variation of the viscosity with the speed and load. Taking these variations of viscosity into account, the results obtained from the hydrodynamical theory are brought into complete accordance with these experiments of Mr Tower. Thus we have not only an explanation of the very novel phenomena brought to light by these experiments, and what appears to be an important verification of the assumptions on which the theory of hydrodynamics is founded, but we also find, what is not shown in the experiments, how the various circumstances under which the experiments have been made affect the results.

7. Two circumstances particularly are brought out in the theory as principal circumstances which seem to have hitherto entirely escaped notice, even that of Mr Tower.

One of these is the difference in the radii of the journal and of the brass or bearing.

It is well known that the fitting between the journal and its bearing produces a great effect on the carrying power of the journal, but this fitting is rather supposed to be a matter of smoothness of surface than a degree of correspondence in radii. The radius of the bearing must always be as much larger than that of the journal as is necessary to secure an easy fit; but more than this, I think, has never been suggested.

Now it appears from the theory that if viscosity were constant the friction would be inversely proportional to the difference in radii of the journal and the bearing, and this although the arc of contact is less than the semicircumference. Taking the temperature into account, it appears, from the comparison of the theoretical results with the experimental, that at a temperature of 70°·5 Fahr. the radius of one of the brasses used was ·00077 inch greater than that of the journal, while at a temperature of 70° Fahr. that of the other was ·00084 inch, or 9 per cent. larger than the first.

These two brasses were probably both bedded to the journal in the same way, and had neither of them been subjected to any great amount of wear, so that there is nothing surprising in their being so nearly the same fit. It would be extremely interesting to find whether prolonged wear of the brass tends to preserve or destroy the fit. This does not appear from Mr Tower's experiments. It does appear, however, that with an increase of temperature the brass expands more than the journal, and that its radius increases as the load increases in a very definite manner.

Another circumstance brought out by the theory, and remarked on both by Lord Rayleigh and the author at Montreal, but not before expected, is that the point of nearest approach of the journal to the brass is not by any means in the line of the load, and, what is still more contrary to common supposition, is on the *off** side of the line of load.

This circumstance, the reason for which is rendered perfectly clear by the conditions of equilibrium, at once accounts for a singular phenomenon mentioned by Mr Tower, viz., that the journal having been run in one direction until the initial tendency to heat had entirely disappeared, on being reversed it immediately began to heat again; but this effect stopped when the process had been often repeated. The fact being that running in one direction the brass had been worn to the journal only on the *off* side for that direction, so that when the motion was reversed the new *off* side was like a new brass.

7 A. The circumstances which determine the greatest load which a bearing will carry with complete lubrication, *i.e.*, with the film of oil extending between brass and journal throughout the entire arc, are definitely shown in the theory.

The effect of increasing the load beyond a certain small value, being to cause the brass to approach nearer to the journal at a point H, which moves

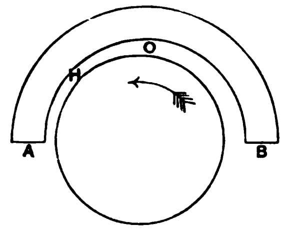


Fig. 1.

from A towards O as the load increases, and when the load is such that the least separating distance is about half the difference of radii, the angular

* "On" and "off" sides of the line of load are used by Mr Tower to express respectively the sides of approach and succession, as B and A in the figure, the arrow indicating the direction of rotation.

position of H is 40° to the off side of O, the middle of the brass. At this point the pressure in the oil film is everywhere greater than at A and B, the extremities of the brass, but when the load further increases the pressure towards A on the off side becomes smaller or negative. This, when sufficient, will cause rupture in the oil film, which will then only extend between the brass and journal over a portion of the whole arc, and a smaller portion as the load increases. Thus, since the amount of negative pressure which the oil will bear, depends on circumstances which are uncertain, the limit of the safe load for complete lubrication is that which causes the least separating distance to be half the distance of radii of the brass and journal.

The rupture of the oil film does not take place at the point of nearest approach, and hence the brass may still be entirely separate from the journal, and could the integrations be effected it would be possible to deal as definitely with this condition as with that of complete lubrication; but these difficulties have limited the actual application of the theory to complete lubrication. This however by no means requires an oil bath, but merely sufficient oil on the journal.

What happens when the supply of oil is limited, *i.e.*, insufficient for complete lubrication, cannot be definitely expressed without further integrations; but sufficient may be seen to show that the brass will still be completely separated from the journal, although the separating film will not touch the brass, except over a limited area; but in this case it is easy to show by general reasoning that in the one extreme, where the supply of oil is limited, the friction increases directly as the load and is independent of the velocity, while in the other, where the oil is abundant, the circumstances are those of the oil bath.

The effect of the limited length of the journal is also apparent in the equations, as is also the effect of necking the shaft to form the journal, so that the ends of the brass are against flanges on the shaft.

The theory is perfectly applicable to cases in which the direction of the load on the bearing varies, as with the crank pin and with the bearings of the crank shaft of the steam-engine; but these cases have not been considered, as there are no definite experiments to compare.

8. Although in the main the present investigation has been directed to the circumstances of Mr Tower's experiments, viz., a cylindrical journal revolving in a cylindrical brass, it has, on the one hand, been found necessary to proceed from the general equations of equilibrium of viscous fluids, and, on the other hand, to consider somewhat generally the physical propert·· ·ᶠ viscosity and its dependence on temperature.

The property of viscosity has been discussed at length in Section II.; which section also contains the account of an experimental investigation as to the viscosity of olive oil.

The general theory deduced from the hydrodynamical equations for viscous fluids, with the methods of application, is given in Sections IV., V., VI., VII., and VIII.

As there are some considerations which cannot be taken into account in the more general method, which method also tends to render obscure the more immediate purpose of the investigation, a preliminary discussion of the problem, illustrated by aid of the graphic method, has been introduced as Section III. Finally, the definite application of the theory to Mr Tower's experiments is given in Section IX.

SECTION II.—THE PROPERTIES OF LUBRICANTS.

9. *The Definition of Viscosity.*

In distinguishing between solid and fluid matter, it is customary to define fluid as a state of matter incapable of sustaining tangential or shearing stress. This definition, however, as is well known, is only true as applied to *actual* fluids when at rest. The resistance encountered by water and all known fluids flowing steadily along parallel channels, affords definite proof that in certain states of motion all actual fluids will sustain shearing stress. These actual fluids are, therefore, called in the language of mathematics imperfect or viscous fluids.

In order to obtain the equations of motion of such fluids, it has been necessary to define clearly the property of viscosity. This definition has been obtained from the consideration that to cause shearing stress in a body it is necessary to submit it to forces tending to change its shape. Forces tending to cause a general motion, whether linear, revolving, uniform expansion, or uniform contraction, call forth no shearing stress.

Using the term *distortion* to express change of shape, apart from change of position, uniform expansion, or contraction, the viscosity of a fluid is defined as the shearing stress caused in the fluid while undergoing distortion, and the shearing stress divided by the rate of distortion is called the coefficient of viscosity, or, commonly, the viscosity of the fluid.

This is best expressed by considering a mass of fluid bounded by two parallel planes at a distance a, and supposing the fluid between these planes to be in motion in a direction parallel to these surfaces with a velocity which varies uniformly from 0 at one of these surfaces to u at the other. Then the rate of distortion is

$$\frac{u}{a}$$

and the shearing stress on a plane parallel to the motion is expressed by

$$f = \mu \frac{u}{a} \qquad \text{.................................(1)},$$

μ being the coefficient of viscosity or the modulus of the resistance to distortional motion.

10. *The Character of Viscosity.*

In dealing with ideal fluids, it is of course allowable to consider μ as being zero or having any conceivable value; but practically, as regards natural philosophy, the value of any such considerations depends on whether the calculated behaviour of the ideal fluid is found to agree with the behaviour of the actual fluids—whether taking a particular fluid, a value of μ can be found such that the values of f calculated by equation (1) agree with the values of f determined by experiments for all values of a and u.

In the mathematical theory of viscous fluid, μ is assumed to be constant for a particular fluid. This supposition is sometimes justified by reference to some assumed dynamical constitution of fluids; but apart from such hypotheses there is no more ground for supposing a constant value for μ than there is for supposing a particular law of gravitation, in other words, there is no ground at all. If a particular value of μ is found to bring the calculated results into agreement with all experimental results, then this value of μ defines a property of actual fluids, and of course it has been with this object that the mathematical theory of μ has been studied.

The chief question as regards μ is a simple one—within a particular fluid is μ constant? In other words, is viscosity a property of a fluid like inertia which is independent of its motion? If it is, our equations may be useful; if it is not then the introduction of μ into the equations renders them so complex that it is almost hopeless to expect anything from them.

Another question of scarcely less practical importance relates to the character of μ near the bounding surfaces of the fluid. If μ is constant in the fluid, does it change its value near the boundary of the fluid? Is there anything like slipping between the fluid and a solid boundary with which it is in contact?

As regards the answers to these questions the present position is somewhat as follows:—

11. *The Two Viscosities.*

The general experience that the resistance varies as the square of the velocity is an absolute proof that μ is not constant unless a restricted meaning be given to the definition of viscosity, excluding such part of the resistance as may be due, in the way explained by Prof. Stokes*, to internal eddies or cross streams, however insensible these may be, so long as they are not simply molecular motions.

On the other hand in the definite experiments made by Colomb, and particularly by Poiseuille, it was found that the resistance was proportional to the velocity, and therefore that μ was absolutely constant—*i.e.*, independent of the velocity†.

To meet this discordance it has been supposed that μ varied with the rate of distortion—*i.e.*, is a function of u/a, but is sensibly constant when u/a is small‡.

To assume this, however, is to neglect Poiseuille's experiments, in which he found for water the resistance absolutely proportional to the velocity in a tube ·6 mm. diameter up to a velocity of 6 metres per second, which corresponds to a value of $u/a = 20,000$.

On the other hand it is found by Darcy§ and others in large tubes that the resistance varies as the square of the velocity for values of $\frac{u}{a}$, as low as 1. Thus in a tube of ·6 mm. we have μ constant for all rates of distortion below 20,000, while in a tube of 500 mm. diameter μ is a function of the distortion for all values greater than 1.

It is, therefore, clear that if μ is a function of the distortion it must also be a function of the dimensions of the channels, and in that case μ cannot be considered as a property of the fluid only.

The change in the law of resistance from the simple ratio has, however, been shown by the author to be due to a change in the character of the motion of the fluid from that of direct parallel motion to that of sinuous or eddying motion‖.

* Stokes's *Reprint*, vol. I., p. 99.

† *Paris Mém. Savans Étrang.*, tom. 9 (1846), p. 434.

‡ Lamb's *Motion of Fluids*, 1879, Art. 180.

§ *Recherches Expl.* Paris, 1852.

‖ "An Experimental Investigation of the circumstances which determine whether the Motion of Water shall be Direct or Sinuous." *Phil. Trans.*, vol. 174 (1883), p. 935.

In the latter case, although the mean motion at any point taken over a sufficient time is parallel to the pipe, it is made up of a succession of motions crossing the pipe in different directions.

The question as to whether, in the case of sinuous motion, μ is to be considered as a function of the velocity or not, depends on whether we regard f as expressing the instantaneous shearing stress at a point, or the mean over a sufficient time. Whether we regard the symbols in the equations of motion as expressing the instantaneous motion or the mean taken over a sufficient time.

If the latter, then μ must be held to include, in addition to the mean stress, the momentum per second parallel to u carried by the cross streams in the negative direction across the surface over which f is measured.

If, however, we regard the motion at each instant, then we must restrict our definition of viscosity by making f the instantaneous value of the intensity of resistance at a point.

This is a quantity which we have and can have no means of measuring except under circumstances which secure that f is constant for all points over a given surface, and for all instants over a given time.

It thus appears that there are two essentially distinct viscosities in fluids. The one a mechanical viscosity arising from the molar motion of the fluid, the other a physical property of the fluid. It is worth while to point out that, although the conditions under which the first of these—the mechanical viscosity—can exist, depend primarily on the physical viscosity, the actual magnitudes of these viscosities are independent, or are only connected in a secondary manner. This is shown by a very striking but little noticed fact. When the motion of the fluid is such that the resistance is as the square of the velocity, the magnitude of this resistance is sensibly quite independent of the character of the fluid in all respects except that of density. Thus, when in a particular pipe the velocity of oil or treacle is sufficient for the resistance to vary as the square of the velocity, the resistance is practically the same as it would be with water at the same velocity, while the physical viscosity of water is more than one hundred times less.

The answer, then, to the question as to the constancy of μ may be clearly given—μ measures a physical property of the fluid which is independent of its motion. But in this sense μ is the coefficient of instantaneous resistance to distortion at a point moving with the fluid.

This restriction is equivalent to restricting the applications of the equations of motion for a viscous fluid to the cases in which there are no eddies or sinuosities.

This, as shown by the author, is the case in parallel channels so long as the product of the velocity, the width of the channel, and the density of the fluid divided by μ is less than a certain constant value. In a round tube this constant is 1400, or

$$\frac{Dv\rho}{\mu} < 1400.$$

At a temperature of 50°, we have, with a foot as unit of length, for water—

$$\frac{\mu}{\rho} = 0{\cdot}00001428,$$

$$Dv < {\cdot}02,$$

so that if D, the diameter of the channel, be ·001 inch, v would have to be at least 240 feet per second for the resistance to vary other than as the velocity.

As regards the slipping at the boundaries, Poiseuille's experiments, as well as those of the author, failed to show a trace of this, although f reached the value of 0·702 lb. per square inch, so that within this limit it may be taken as proved that there is no slipping between any solid surface and water. With other fluids, such as mercury in glass tubes, it is possible that the case may be different; but, as regards oils, the probability seems to be that the limit within which there is no slipping will be much higher than with water.

12. *Experimental Determination of the Value of μ for Olive Oil.*

Since the value of μ for water is known for all moderate temperatures, in order to obtain the value for oil it is only necessary to ascertain the relative times taken by the same volumes of oil and water to flow through the same channel, care being taken to make the channel such that there are no eddies and that the energy of motion is small compared with the loss of head.

These times are proportional to

$$\frac{\mu}{p},$$

where p is the fall of pressure; therefore the times multiplied by the respective falls of pressure are proportional to the viscosities.

The arrangement of apparatus used is shown in Fig. 2.

The test tube (A) containing the fluid to be tested was fixed in a beaker of water, which was heated from below and maintained at any required temperature.

A syphon (*B*), made of glass tube $\frac{3}{16}$ inch internal diameter, with the extremity of its short limb drawn down to capillary size for a length of about 6 inches, this six inches being bent up and down so as only to occupy

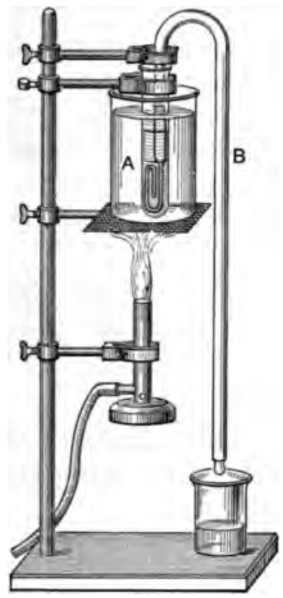


Fig. 2.

some 2 inches at the bottom of the test tube. The long limb of the syphon extended to about 2 feet below the mean level of the fluid in the test tube. Two marks on the test tube at different levels served to show when a definite volume had been withdrawn.

The syphon used was the same for each set of experiments on oil and water, so that the pressure urging the fluid through the tube was proportioned to the density of the fluids—that is, it was 1·915 as great for oil as water, disregarding the effect of the variation of temperature on volume, which in no case amounted to 1 per cent.

Experiments were first made with water at different temperatures, the times taken for the water to fall from the first mark to the second being carefully noted. The syphon was then dried and replaced and oil substituted for water.

Two sets of similar apparatus were used on different occasions, different samples of oil being used. In the first set the experiments on oil were made at temperatures from 95° to 200° Fahr.; in the second set, from 61° to 120° Fahr. In so far as the temperatures overlapped, the viscosities for the two oils agreed to within 4 per cent., but as the law of variation of the viscosity seemed to change rapidly at about 140° Fahr., only the second set have been recorded. These are shown in Table I.

TABLE I.—VISCOSITY OF OIL COMPARED WITH WATER: 11 April, 1884.

Number.	Third.	Temperature.		Time seconds.	$\frac{\mu}{107}$ experimental.	log μ experimental.	log μ calculated.	$\frac{\mu}{107}$ calculated.
		Fahrenheit.	Centigrade.					
1	Water. .	60°	15°·5	25	1·640			
2	" . .	"	"	"	"			
3	" . .	"	"	"	"			
4	Olive oil .	61	16	2040	123·00	5·08990	5·090133	123·06
5	" .	81		1350	81·00	$\bar{6}$·90848	$\bar{6}$·89807	79·08
6	" .	94		1000	60·00	$\bar{6}$·77815	$\bar{6}$·78290	59·34
7	" .	120		555	33·40	$\bar{6}$·52375	$\bar{6}$·52375	33·40

From Poiseuille's experiments it is found that, measuring viscosity in pounds on the square inch, for water at a temperature of 61° Fahr.,

$$\mu = 10^{-7} \times 1{\cdot}61.$$

Adopting this value of μ for the experiments on water at 61° Fahr., the other experimental values of μ for water at different temperatures, obtained as being in the ratios of the times, were found to be in very close agreement with those calculated from Poiseuille's law for the respective temperatures. This tested the efficiency of the apparatus. It has not been thought necessary to record any experiment on water except at the temperature of 61° Fahr.

The experimental value of μ for oil are in the ratios of the times multiplied by ·915, the specific gravity of oil; these are given as the experimental values of μ in the table. Another column contains the values of μ for oil, calculated from an empirical formula fitted to the experimental values.

This formula was found by comparing the logarithms of the experimental values of μ. It appeared that the differences in these logarithms were nearly proportional to the differences in the corresponding temperatures, or that T being temperature in degrees Fahr.,

$$\log \mu_1 - \log \mu_2 = {\cdot}0096\,(T_2 - T_1),$$

in degrees Centigrade

$$\log \mu_1 - \log \mu_2 = {\cdot}00535\,(T_2 - T_1);$$

whence since

$${\cdot}0096 = {\cdot}0021 \log_{10} e,$$

$${\cdot}00535 = {\cdot}0123 \log_{10} e,$$

$$\left.\begin{array}{ll}\text{for degrees Fahrenheit} & \dfrac{\mu_1}{\mu_2} = e^{-\cdot 0221(T_1 - T_2)} \\[2ex] \text{for degrees Centigrade} & \dfrac{\mu_1}{\mu_2} = e^{-\cdot 0123(T_1 - T_2)}\end{array}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2).$$

This ratio holds well within the experimental accuracy from temperatures ranging from 61° to 120° Fahr. This is shown in the table, and again in Fig. 3, in which the ordinates are proportional to $\log \mu$, the abscissæ being proportional to the corresponding temperatures.

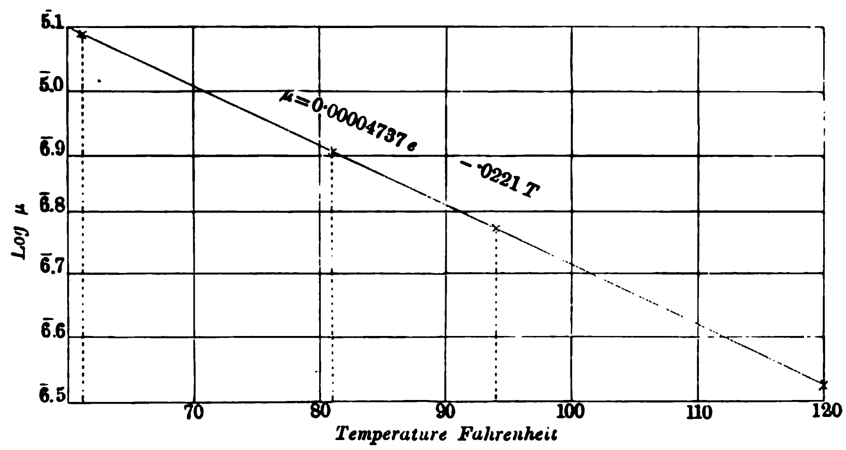


Fig. 3.

13. *The Comparative Values of μ for Different Fluids and Different Systems of Units.*

The values of μ given by different writers for air and water, are expressed in various units of force and length, so that it is a matter of some trouble to compare them. To facilitate this for the future comparative values are here given. Those for water have been deduced from Poiseuille's formula, for air from Maxwell's formulæ, and for olive oil from the experiments recorded in the previous article.

The units of length, mass, and time, being respectively the centimetre, gramme, and second, in which case the unit of force is the weight of one 980·5th (g) part of a gramme, expressing temperature in degrees Centigrade by T and putting

$$P^{-1} = 1 + 0{\cdot}0336793T + 0{\cdot}0002209936T^2 \ldots\ldots\ldots\ldots (3),$$

for water . . . $\mu = 0·0177931P$
air . . . . $\mu = 0·0001879\,(1 + 0·00366T)$ .... (4).
olive oil . . $\mu = 3·2653e^{-·0123T}$ *

With the same unit of length, but g grammes as unit of mass and 1 gramme as unit of force, the values of μ are for

water . . . $\mu = 0·0000181P$
air . . . . $\mu = 0·00000019153\,(1 + ·00366T)$ .... (5).
olive oil . . $\mu = 0·0033303e^{-·0123T}$

The units of length and mass being the foot and pound and the temperature in degrees Fahr. for

water . . . $\mu = 0·0011971P$
air . . . . $\mu = 0·000011788\,(1 + ·0020274T)$ .... (6).
olive oil . . $\mu = 0·21943e^{-·0221T}$

With the same unit of length the unit of mass being g (32·1695) lbs. and the unit of force 1 lb. for

water . . . $\mu = 0·000037166P$
air . . . . $\mu = 0·00000036645\,(1 + ·002074T)$ .... (7).
olive oil . . $\mu = 0·0068213e^{-·0221T}$

Taking the unit of length 1 inch and the unit of force 1 lb. for

water . . . $\mu = 0·000000258105P$
air . . . . $\mu = 0·0000000025447\,(1 + ·0020274T)$ ...... (8).
olive oil . . $\mu = 0·00004737e^{-·0221T}$

SECTION III.—GENERAL VIEW OF THE ACTION OF LUBRICATION.

14. *The case of two nearly Parallel Surfaces separated by a Viscous Fluid.*

Let AB and CD (Fig. 4) be perpendicular sections of the surfaces, CD being of limited but of great extent compared with the distance h

* For olive oil the values of μ have only been tested between the limits of temperature 16° and 49° C. or 61° and 120° Fahr.

between the surfaces, both surfaces being of unlimited length in a direction perpendicular to the paper.

Fig. 4.

Case 1. *Parallel Surfaces in Relative Tangential Motion.*—In Fig. 5 the surface CD is supposed fixed, while AB moves to the left with a velocity U.

Then by the definition of viscosity (Art. 9) there will be a tangential resistance

$$F = \mu \frac{U}{h},$$

and the tangential motion of the fluid will vary uniformly from U at AB to zero at CD. Thus if FG (Fig. 5) be taken to represent U, then PN will represent the velocity in the fluid at P.

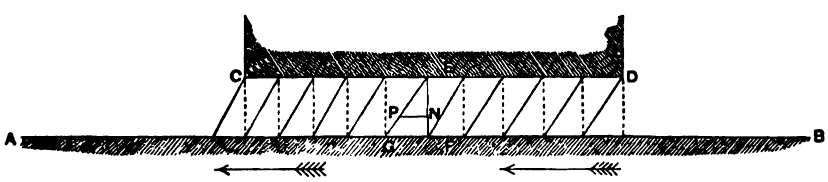


Fig. 5.

The slope of the line EG therefore may be taken to represent the force F, and the direction of the tangential force on either surface is the same as if EG were in tension. The sloping lines therefore represent the condition of motion and stress throughout the film (Fig. 5).

Case 2. *Parallel Surfaces approaching with no Tangential Motion.*—The fluid has to be squeezed out between the surfaces, and since there is no motion at the surface, the horizontal velocity outward will be greatest half-way between the surfaces, nothing at O the middle of CD, and greatest at the ends.

If in a certain state of the motion (shown by dotted line, Fig. 6) the space between AB and CD be divided into 10 equal parts by vertical lines (Fig. 6, dotted figure), and these lines be supposed to move with the fluid, they will shortly after assume the positions of the curved lines (Fig. 6), in which the areas included between each pair of curved lines is the same as

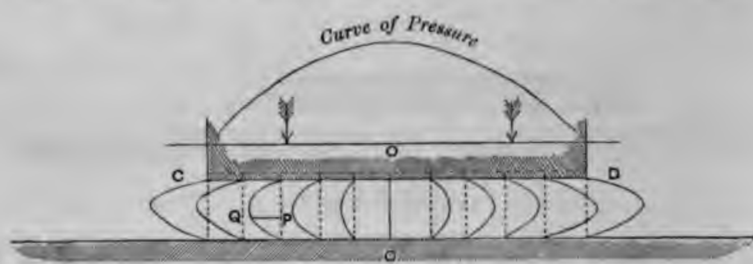


Fig. 6.

in the dotted figure. In this case, as in Case 1, the distance QP will represent the motion at any point P, and the slope of the lines will represent the tangential forces in the fluid as if the lines were stretched elastic strings. It is at once seen from this that the fluid will be pulled towards the middle of CD by the viscosity as though by the stretched elastic lines, and hence that the pressure will be greatest at O and fall off towards the ends C and D, and would be approximately represented by the curve at the top of the figure.

Case 3. *Parallel Surfaces approaching with Tangential Motion.*—The lines representing the motions in Cases 1 and 2 may be superimposed by adding the distances PQ in Fig. 6 to the distances PN in Fig. 5.

The result will be as shown in Fig. 7, in which the lines represent in the same way as before the motions and stresses in the fluid where the surfaces are approaching with tangential motion.

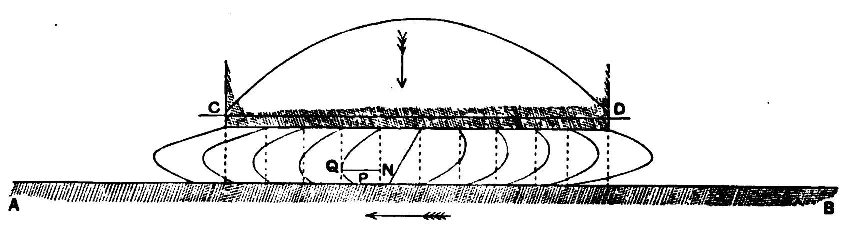


Fig. 7.

In this case the distribution of pressure over CD is nearly the same as in Case 2, and the mean tangential force will be the same as in Case 1. The distribution of the friction over CD will, however, be different. This is shown by the inclination of the curves at the points where they meet the surface. Thus on CD the slope is greater on the left and less on the ght, which shows that the friction will be greater on the left and less on he right than in Case 1. On AB the slope is greater on the right and ss on the left, as is also the friction.

Case 4. *Surfaces inclined with Tangential Movement only.*—AB is in otion as in Case 1, and CD is inclined as in Fig. 8.

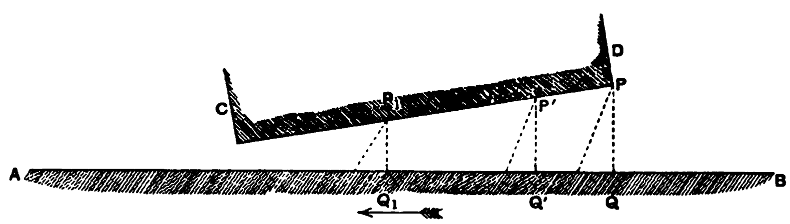


Fig. 8.

The effect in this case will be nearly the same as in the compound ovement (Case 3).

For if corresponding to the uniform movement U of AB, the velocity f the fluid varied uniformly from the surface AB to CD, then the quantity arried across any section PQ would be

$$PQ \times \frac{U}{2},$$

nd consequently would be proportional to PQ; but the quantities carried cross all sections must be the same, as the surfaces do not change their elative distances; therefore there must be a general outflow from any ertical sections PQ, $P'Q'$ given by

$$\frac{U}{2}(PQ - P'Q').$$

his outflow will take place to the right and left of the section of greatest ressure. Let this be P_1Q_1, then the flow past any other section PQ is

$$\frac{U}{2}(PQ - P_1Q_1)$$

o the right or left according as PQ is to the right or left of P_1Q_1. Hence t this section the motion will be one of uniform variation, and to the ight and left the lines showing the motion and friction will be nearly s in Fig. 7. This is shown in Fig. 9.

This is the explanation of continuous lubrication.

The pressure of the intervening film of fluid would cause a force tending o separate the surfaces.

The mean line or resultant of this force would act through some point O.

This point O does not necessarily coincide with P_1, the point of maximum pressure.

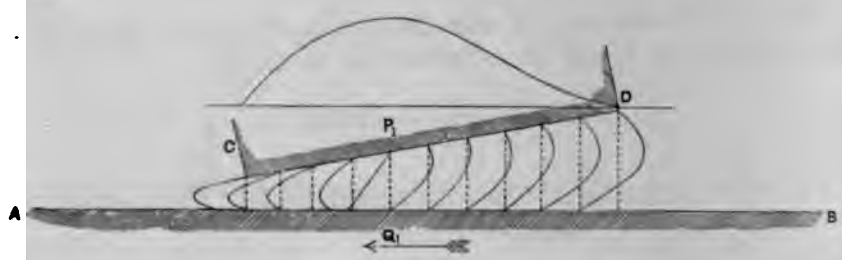


Fig. 9.

For equilibrium of the surface AB, O will be in the line of the resultant external force urging the surfaces together, otherwise the surface ACD would change its inclination.

The resultant pressure must also be equal to the resultant external force perpendicular to AB (neglecting the obliquity of CD). If the surfaces were free to approach the pressure would adjust itself to the load, for the nearer the surfaces the greater would be the friction and consequent pressure for the same velocity, so that the surfaces would approach until the pressure balanced the load.

As the distance between the surfaces diminished O would change its position, and therefore, to prevent an alteration of inclination, the surface CD must be constrained so that it could not turn round.

It is to be noticed that continuous lubrication between plane surfaces can only take place with continuous motion in one direction, which is the direction of continuous inclination of the surfaces.

With reciprocating motion, in order that there may be continuous lubrication, the surfaces must be other than plane.

15. *Revolving Cylindrical Surface.*

When the moving surface AB is cylindrical and revolving about its axis, the general motion of the film will differ somewhat from what it is with flat surfaces.

Case 5. *Revolving Motion, CD flat and symmetrically placed.*—The surface velocity of AB may be expressed by U as before. The curves of motion found by the same method as in the previous cases are shown in Fig. 10.

The curves to the right of GH, the shortest distance between the surfaces, will have the same character as those in Fig. 9 to the right of C, at which is also the shortest distance between the surfaces.

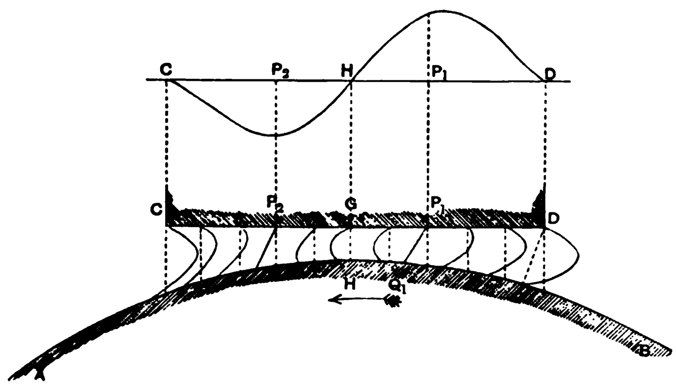


Fig. 10.

On the left of GH the curves will be exactly similar to those on the right, only drawn the other way about, so that they are concave towards a section at P_2 in a similar position on the left to that occupied by P_1 on the right.

This is because a uniformly varying motion would carry a quantity of fluid proportional to the thickness of the stratum from right to left, and thus while it would carry more fluid through the sections towards the right than it would carry across GH, necessitating an outward flow from the position P_1 in both directions, the same motion would carry more fluid away from sections towards C than it would supply past GH, thus necessitating an inward flow towards the position P_2.

Since G is in the middle of CD these two actions, though opposite, will be otherwise symmetrical, and

$$P_2G = GP_1.$$

From the convexity of the curves to the section at P_2 it appears that this section would be one of minimum pressure, just as P_1 is of maximum. Of course this is supposing the lubricant under sufficient pressure at C and D to allow of the pressure falling. The curve of pressure would be similar to that at the top of Fig. 10, in which C and D are points of equal pressure, P_1HP_2 the singular points in the curve.

Under such conditions the fluid pressure acts to separate the surfaces on the right, but as the pressure is negative on the left the surfaces will be drawn together. So that the total effect will be to produce a turning moment on the surface AB.

Case 6. *The same as Case 5, except that G is not in the middle of CD.*—In this case the curves of motion will be symmetrical on each side of H at equal distances, as shown in Fig. 11.

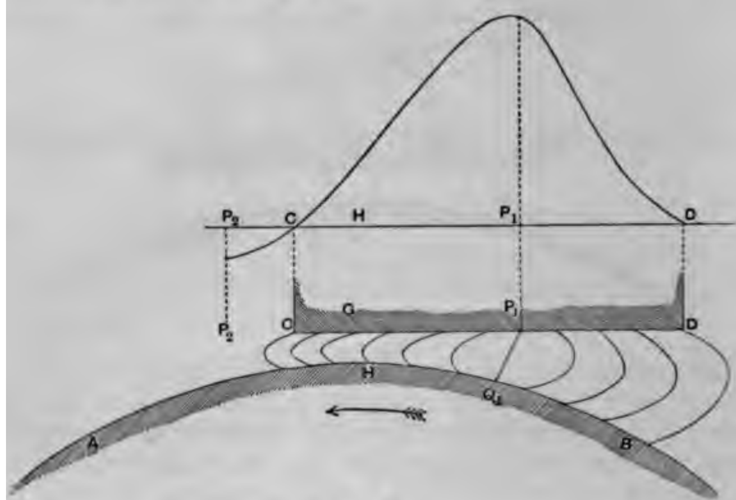


Fig. 11.

If C lies between H and P_2 the pressure will be altogether positive, as shown by the curve above Fig. 11—that is, will tend to separate the surfaces.

16. *The Effect of a Limiting Supply of Lubricating Material.*

In the cases already considered C and D have been the actual limits of the upper surface. If the supply of lubricant is limited C and D may be the extreme points to which the separating film reaches on the upper surface, which may be unlimited, as in Fig. 12.

Case 7. *Supply of Lubricant Limited.*—If the surface AB be supposed to have been covered with a film of oil, the oil adhering to the surface and moving with it, then the surface CD to have been brought up to a less distance than that occupied by the film of oil, the oil will accumulate as it is brought up by the motion of AB, forming a pad between the surfaces, particularly on the side D.

The thickness of the film as it leaves the side C being reduced until the whole surface AB is covered with a film of such thinness that as much leaves at C as is brought up to D, then the condition will be steady.

Putting b for the thickness of the film of oil outside the pad, the quantity of oil brought up to D by the motion of this film will be per second

$$bU,$$

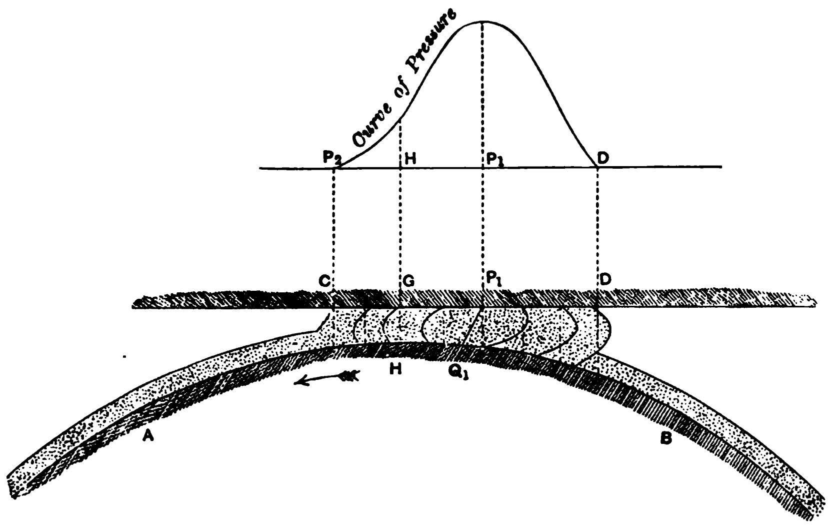


Fig. 12.

and the quantity which passes the section P_1Q_1, across which the velocity varies uniformly, will be

$$\frac{P_1Q_1U}{2}.$$

Therefore since there is no further accumulation

$$P_1Q_1 = 2b,$$

also, since $GP_2 = GP_1$ (Fig. 10, Case 5)

$$P_2Q_2 = 2b.$$

And since the quantity which passes P_2Q_2 will not be sufficient to occupy the larger sections on the left, the fluid will not touch the upper surface to the left of P_2. The limit will therefore be at P_2, the fluid passing away with AB in a film of thickness b.

This is the ordinary case of partial lubrication: AB, the surface of the journal, is covered with a film of oil; CD, the surface of the brass or bearing, is separated from AB by a pad of oil near H, the point of nearest approach.

This pad is under pressure, which is a maximum at P_1, and slopes away to nothing at D and P_2, the extremities of the pad, as is shown by the curve above, Fig. 12.

17. *The Relation between Resistance, Load, and Speed for Limited Lubrication.*

In Case 7 a definite quantity of oil must be in the film round the journal, or in the pad between the surfaces. As the surfaces approach, the pad will increase and the film diminish, and *vice versâ*. The resistance increases with the length of the pad, and with the diminution of the distance between the surfaces. The mean intensity of pressure increases with the length of the pad, and inversely with the thickness of the film, but not in either case in the simple ratio. The total pressure, which is equal to the load, increases with the intensity of pressure and the length of the pad.

The definite expressions of these relations depend on certain integrations, which have not yet been effected. From the general relations pointed out, it follows that an increase of load will diminish HG and P_1Q_1, and consequently the thickness of the film round the journal, and will increase the length of the pad. It will therefore increase the friction.

Thus with a limited supply of oil the friction will increase with the load in some ratio not precisely determined.

Further, both the friction and the pressure increase in the direct ratio of the speed, provided the distance between the surfaces and the length of the pad remains constant; then, if the load remains constant, the thickness of the film must increase, and the length of the pad diminish with the speed; and both these effects will diminish friction in exactly the same ratio as the reduction of load diminishes friction.

Thus if with a speed U a load W and friction F a certain thickness of oil is maintained, the same will be maintained with a speed MU, a load MW, and the friction will be MF.

How far this increase of friction is to be attributed to the increased velocity, and how far to the increased load, is not yet shown in the theory for this case; but, as has been pointed out, if the load be altered from MW to W, the velocity remaining the same, the friction will be altered from MF in the direction of F. Therefore, with the load constant, it does appear from the theory that the friction will not increase as the first power of the velocity.

There is nothing therefore in this theory contrary to the experience that, with very limited lubrication, the friction is proportional to the load and independent of the velocity, while the theoretical conclusion that the friction, with any particular load and speed, will depend on the supply of oil in the pad, is in strict accordance with Mr Tower's conclusion, and with the general disagreement of the coefficients of friction in different experiments.

17 A. *The Conditions of Equilibrium with Cylindrical Surfaces.*

So far CB has been considered as a flat surface, in which case the equilibrium of CB requires that it should be so far constrained by external forces that it cannot either change its direction or move horizontally.

When AB is a portion of a cylindrical surface, having its axis parallel to that of AB, the only condition of constraint necessary for equilibrium is that CB shall not turn about its axis. This will appear on consideration of the following cases:—

Case 8. *Surfaces Cylindrical and the Supply of Oil Limited.*—Fig. 13 shows the surfaces AB and CD.

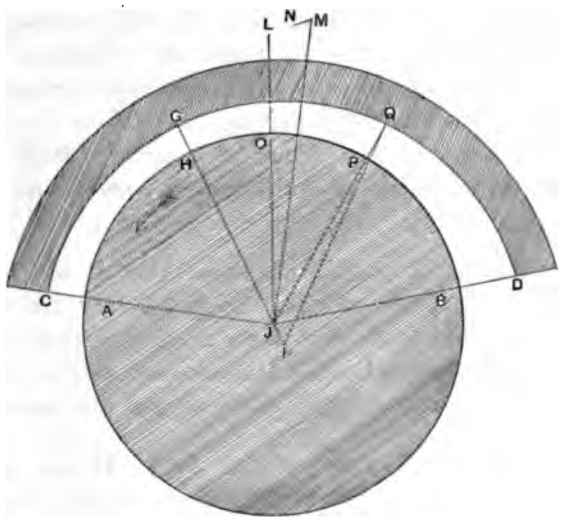


Fig. 13.

J is the axis of the journal AB.

I is the axis of the brass CD.

JL is the line in which the load acts.

O is the point in which JL meets AB.

$R = JP$.

$R + a = IQ$.

$h = PQ$.

$h_0 = HG$.

The condition for the equilibrium of I the centre of the brass is that the resultant of the oil pressure on DC together with friction shall be in the direction OL, and the magnitude of this resultant shall be equal to the load.

As regards the magnitude of this resultant, it increases as HG diminishes to a certain limit, *i.e.*, as the surfaces approach, so that in this respect equilibrium is obviously secured, and it is only the direction of the resultant pressure and friction that need be considered.

Since the fluid film is in equilibrium under the forces exerted by the two opposite surfaces, these forces must be equal and opposite, so that it is only necessary to consider the forces exerted by AB on the fluid.

From what has been already seen in Cases 6 and 7 it appears that the resultant line of pressure JM always lies on the right or *on* side of GH. The resultant friction clearly acts to the left, so that if JM be taken to represent the resultant pressure and MN the resultant friction, N is to the left of M and JN the resultant of pressure and friction is to the left of JM.

Taking LJ to represent the load, then LN will represent the resultant moving force on CD that is on I. Since H will move in the opposite direction to I, and since the direction of the resultant pressure moves in the same direction as H, the effect of a moving force LN on I will be to move N towards L until they coincide. Thus, as long as JM is within the arc covered by the brass, a position of equilibrium is possible and the equilibrium will be stable.

So far the condition of equilibrium shows that H will be on the left or off side of the line of load, and this holds whether the supply of oil is abundant or limited; but while with a very limited supply of oil, *i.e.*, a very short oil pad, H must always be in the immediate neighbourhood of O, this is by no means the case as the length of the oil pad increases.

Case 9. *Cylindrical Surfaces in Oil Bath.*—If the supply of oil is sufficient, the oil film or pad between the surfaces will extend continuously from the extremities of the brass, unless such extension would cause negative pressure which might lead to discontinuity. In this case the conditions of equilibrium determine the position of H.

The conditions of equilibrium are as before—

1. That the horizontal component of the oil pressure on the brass shall balance the horizontal component of the friction;

2. That the vertical components of the pressure and friction shall balance the load.

Taking the surface of the brass, as is usual, to embrace nearly half the circumference of the journal and, to commence with, supposing the brass to be unloaded, the movement of H may be traced as the load increases.

When there is no load, the conditions of equilibrium are satisfied if the position of H is such, that the vertical components of pressure and friction are each zero, and the horizontal components are equal and opposite.

This will be when H is at O (Fig. 13); for then, as has been shown, Case 5, the pressure on the left of H will be negative, and will be exactly equal to the pressure at corresponding points on the right, so that the vertical components left and right balance each other. On the other hand the horizontal component of the pressure to the left and right will both act on the brass to the right, and as these will increase as the surfaces approach, the distance JI must be exactly such that these components balance the resultant friction, which by symmetry will be horizontal and acting to the left.

It thus appears that when the brass is unloaded its point of nearest approach will be its middle point. This position, together with the curves of pressure, are shown in Fig. 14.

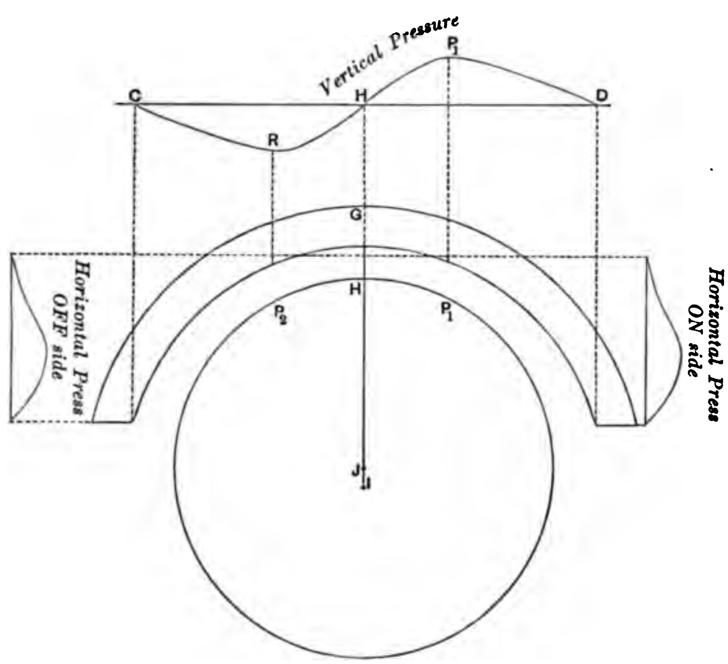


Fig. 14.

As the load increases, the positive vertical component on the right of GH must overbalance the negative component on the left. This requires that H should be to the left of O.

It is also necessary that the horizontal components of pressure and friction should balance.

These two conditions determine the position of H and the value of JI.

As the load increases it appears from the exact equations (to be discussed in a subsequent article) that OH reaches a maximum value, which places H nearly, but not quite, at the left extremity of the brass, but leaves JI still small as compared with GH.

For a further increase of the load H moves back again towards O.

In this condition the load has become so great that the friction, which remains nearly constant, is so small by comparison that it may be neglected, and the condition of equilibrium is that the horizontal component of the pressure is zero, and the vertical component equal to the load.

H continues to recede as the load increases. But when HC becomes greater than HP_2, the pressure between P_2 and C would become negative if the condition did not break down by discontinuity in the oil, which is sure to occur when the pressure falls below that of zero, and then the condition becomes the same as that with a limited supply of oil.

This is important, as it shows that with extreme loads the oil bath comes to be practically the same as that of a limited supply of oil, and hence that the extreme load which the brass would carry would be the same in both cases—as Mr Tower has shown it to be.

In all Mr Tower's experiments with the oil bath it appears that the conditions were such that as the load increased H was in retreat from C towards O, and that, except in the extreme cases, P_2 had not come up to C.

Figs. 2, 3, 4, show the exact curves of pressure as calculated by the exact method to be given, for circumstances corresponding very closely with one of Mr Tower's experiments, in which he actually measured the pressure of oil at three points in the film. These measured pressures are shown by the crosses.

The result of the calculations for this experiment is to show, what could not indeed be measured, that in Mr Tower's experiment the difference in the radii of the brass and journal at 70°, and a load of 100 lbs. per square inch was:

$$a = \cdot 00077$$

$$GH = \cdot 000375$$

$$\text{(The angle) } OJH = 48°.$$

18. *The Wear and Heating of Bearings.*

Before the journal starts the effect of the load will have brought the brass into contact with the journal at O. At starting the surfaces will be in contact, and the initial friction will be between solid surfaces, causing some abrasion.

After motion commences the surfaces gradually separate as the velocity increases, more particularly in the case of the oil bath, in which case at starting the friction will be much the same as with a limited supply of oil.

As the speed increases according to the load, GH approaches, according to the supply of oil, to a, and varies but slightly with any further increase of speed; so that the resistance becomes more nearly proportional to the speed and less affected by the load.

When the condition of steady lubrication has been attained, if the surfaces are completely separated by oil, there should be no wear. But if there is wear, as it appears from one cause or another there generally is, it would take place most rapidly where the surfaces are nearest: that is, at GH on the off side of O.

Thus while the motion is in one direction the tendency to wear the surfaces to a fit would be confined to the off side of O.

This appears to offer a very simple and well-founded explanation of the important and common circumstance that new surfaces do not behave so well as old ones; and of the circumstance, observed by Mr Tower, that in the case of the oil bath, running the journal in one direction does not prepare the brass for carrying a load when the journal is run in the opposite direction. This explanation, however, depends on the effect of misfit in the journal and brass which has yet to be considered.

Case 10. *Approximately cylindrical surfaces of limited length in the direction of the axis of rotation.* Nothing has so far been said of any possible motion of the fluid perpendicular to the direction of motion and parallel to the axis of the journal. It having been assumed that the surfaces were truly cylindrical and of unlimited length in direction of their axes, and in such case there would be no such flow.

But in practice brasses are necessarily of limited length, so that the oil can escape from the ends of the brass. Such escape will obviously prevent the pressure of the film of oil from reaching its full height for some distance from the ends of the brass and cause it to fall to nothing at the extreme ends.

This was shown by Mr Tower, who measured the pressure at several points along the brass in the line through O, and found it to follow a curve similar to that, shown in Fig. 15, which corresponds to what might be expected from escape at the free ends.

If the surfaces are not strictly parallel in the directions *TU* and *VW*, the pressure would be greatest in the narrowest parts, causing axial flow from those into the broader spaces. Hence, if the surfaces were considerably

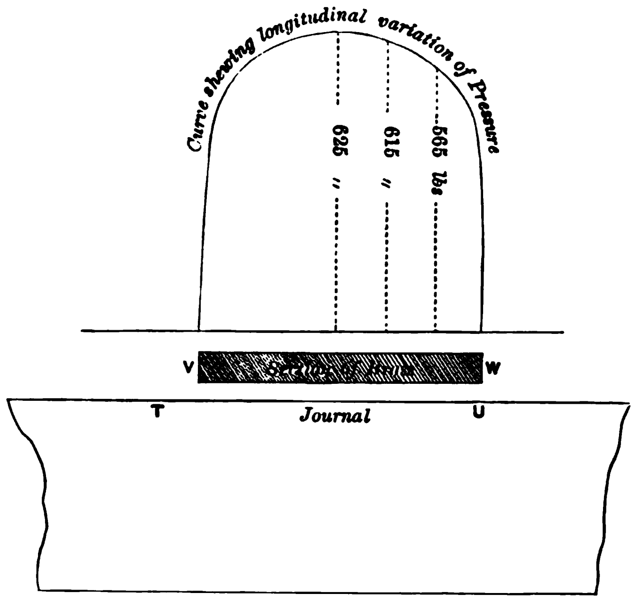


Fig. 15.

irregular, the lubricant would, by escaping into broader spaces, allow the brass to approach and eventually to touch the journal at the narrowest spaces, and this would be particularly the case near the ends.

As a matter of fact, the general fit of two new surfaces can only be approximate; and how near the approximation is, is a matter of the time and skill spent on preparing, or, as it is called, bedding them. Such bedding as brasses are subject to would not bring them to a condition in which the hills and hollows differed by less than a $\frac{1}{10000}$th part of an inch, so that two such surfaces touching each other on the hills would have spaces as great as a $\frac{1}{5000}$th of an inch between them. This seems a small matter, but not when compared with the mean width of the interval between the brass and the journal which, as will be subsequently shown, was less than $\frac{1}{1000}$th of an inch.

It may be assumed, therefore, that such inequalities generally exist in the surfaces of new brasses and journals. And as the surfaces according to their material and manner of support yield to pressure the brass will close on the journal at its ends, where, owing to the escape of oil, there is no pressure to keep them separate.

The section of a new brass and journal taken at GH will therefore be, f sufficiently magnified, as shown in Fig. 16, the thickness of the film,

Fig. 16.

nstead of being, say, of $\frac{3}{10000}$ths of an inch, varies from 0 to $\frac{5}{10000}$ ths, and s less at the ends than at the middle.

In this condition the wear will be at the points of contact, which will be n the neighbourhood of GH on the off side of O (Fig. 13), so that, if the ournal runs in one direction only, the surfaces in the neighbourhood of GH on the *off* side) will be gradually worn to a fit, during which wear the iction will be great and attended with heating, more or less, according to he rate of wear and the obstruction to the escape of heat.

So long, however, as the journal runs in one direction only GH will be n one side (the *off* side) of O, and the wear will be altogether or mainly on his side, according to the distance of H from O.

In the meantime the brass on the *on* side is not similarly worn, so that the motion of the journal is reversed, and the point H transferred to the te *on* side, the wear will have to be gone through again.

That this is the true explanation is confirmed if, as seems from Mr Tower's eport, the heating effect on first reversing the journal was much more vident in the case of the oil bath.

For when the supply of oil is short, HG will be very small, and H will e close to O. So that the wearing area will probably extend to both sides f O, and thus the brass be partially, if not altogether, prepared for running n the opposite direction.

When the supply of oil is complete, however, as has been shown, H is 0° or 60° from O, unless the load is in excess, so that the wear in the eighbourhood of H on the one side of O would not extend to a point 100° r 120° over to the other side.

Even in the case of a perfectly smooth brass, the running of the journal nder a sufficient load in one direction should, supposing some wear, according to the theory render the brass less well able to carry the load when unning in the opposite direction. For, as has already appeared, the pressure etween the journal and brass depends on the radius of curvature of the brass on the *on* side being greater than that of the journal. If then the effect of wear is to diminish the radius of the brass on the *off* side, so that when the motion is reversed the radius of the new *on* side is equal to or less than that of the journal, while the radius of the new *off* side is greater, the oil pressure would not rise. And this is the effect of wear; for as will be definitely shown, the effect of the oil pressure is to increase the radius of curvature of the brass, and as the centre of wear is well on the *off* side, the effect of sufficient wear will be to bring the radius on this side, while the pressure is on, more nearly to that of the journal, so that on the pressure being removed, the brass on this side may resume a radius even less than that of the journal.

Section IV.—The Equations of Hydrodynamics as applied to Lubrication.

19. According to the usual method of expressing the stress in a viscous fluid (which is the same as in an elastic solid)*:

$$\left.\begin{aligned} p_{xx} &= -p - \tfrac{2}{3}\mu\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)+2\mu\frac{du}{dx} \\ p_{yy} &= -p - \tfrac{2}{3}\mu\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)+2\mu\frac{dv}{dy} \\ p_{zz} &= -p - \tfrac{2}{3}\mu\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)+2\mu\frac{dw}{dz} \end{aligned}\right\}\quad\text{..............(9)}.$$

$$\left.\begin{aligned} p_{xy} &= p_{yx} = \mu\left(\frac{dv}{dx}+\frac{du}{dy}\right) \\ p_{yz} &= p_{zy} = \mu\left(\frac{dw}{dy}+\frac{dv}{dz}\right) \\ p_{zx} &= p_{xz} = \mu\left(\frac{du}{dz}+\frac{dw}{dx}\right) \end{aligned}\right\}\quad\text{.............................(10)}.$$

In which the left-hand members are the stresses on planes perpendicular to the first suffix in directions parallel to the second, the first three being the normal stresses, the last six the tangential stresses.

* Stokes, "On the Theories of the Internal Friction of Fluids in Motion, and of the Equilibrium and Motion of Elastic Solids."—*Trans. Cambridge Phil. Soc.*, vol. VIII., p. 287. Also reprint, vol. I., p. 84. Also Lamb's *Motion of Fluids*, p. 219.

The values of these substituted in the equations of motion

$$\left.\begin{aligned}
\rho\frac{\delta u}{\delta t} &= \rho X + \frac{dp_{xx}}{dx} + \frac{dp_{yx}}{dy} + \frac{dp_{zx}}{dz} \\
\rho\frac{\delta v}{\delta t} &= \rho Y + \frac{dp_{xy}}{dx} + \frac{dp_{yy}}{dy} + \frac{dp_{zy}}{dz} \\
\rho\frac{\delta w}{dt} &= \rho Z + \frac{dp_{xz}}{dx} + \frac{dp_{yz}}{dy} + \frac{dp_{zz}}{dz} \\
\frac{\delta\rho}{\delta t} &= \rho\left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}\right)
\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots(11)$$

give the complete equations of motion for the interior of a viscous fluid.

These equations involve terms severally depending on the inertia and the weight of the fluid, also the variation of stress in the fluid.

In the case of lubrication the spaces between the solid surfaces are so small compared with

$$\frac{\mu}{U}$$

that the motion of the fluid is shown to be free from eddies as already explained (Art. 11). Also that the forces arising from weight and inertia are altogether small compared with the stresses arising from viscosity.

The equations which result from the substitution from (9) and (10) in the first 3 of (11) may therefore be simplified by the omission of the inertia and gravitation terms, which are the terms involving ρ as a factor.

In the case of oil the remaining terms may still further be simplified by omitting the terms depending on the compressibility of the fluid.

Also if, as is the case, μ is nearly constant, the terms involving $d\mu$ may be omitted, or considered of secondary importance.

From equations (11) we then have

$$\left.\begin{aligned}
\frac{dp}{dx} &= \mu\left(\frac{d^2u}{dx^2} + \frac{d^2u}{dy^2} + \frac{d^2u}{dz^2}\right) \\
\frac{dp}{dy} &= \mu\left(\frac{d^2v}{dx^2} + \frac{d^2v}{dy^2} + \frac{d^2v}{dz^2}\right) \\
\frac{dp}{dz} &= \mu\left(\frac{d^2w}{dx^2} + \frac{d^2w}{dy^2} + \frac{d^2w}{dz^2}\right) \\
0 &= \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz}
\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(12).$$

Again, since in the case of lubrication we always have to do with a film of fluid between nearly parallel surfaces, of which the radii of curvature are large compared with the thickness of the film, we may, without error, disregard any curvature there may be in the surfaces, and put

x for distances measured on one of the surfaces in the direction of relative motion,

z for distances measured on the same surface in the direction perpendicular to relative motion,

y for distances measured everywhere at right angles to the surface.

Then, if the surfaces remain in their original direction, since they are nearly parallel,

v will be small compared with u and w, and the variations of u and w in the directions x and z are small compared with their variations in the direction y.

The equations (12) for the interior of the film then become

$$\left.\begin{aligned} \frac{dp}{dx} &= \mu \frac{d^2u}{dy^2} \\ \frac{dp}{dy} &= 0 \\ \frac{dp}{dz} &= \mu \frac{d^2w}{dy^2} \\ 0 &= \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} \end{aligned}\right\} \quad \text{..........................(13).}$$

Equations (10) become

$$\left.\begin{aligned} p_{xy} &= p_{yx} = \mu \frac{du}{dy} \\ p_{yz} &= p_{zy} = \mu \frac{dw}{dy} \\ p_{zx} &= p_{xz} = 0 \end{aligned}\right\} \quad \text{..........................(14).}$$

20. The fluid is subject to boundary conditions as regards pressure and velocity. These are—

(1) At the lubricated surfaces the fluid has the velocity of those surfaces;

(2) At the extremities of the surfaces or film the pressure depends on external conditions.

Thus taking the solid surfaces as $y = 0$, $y = h$, and as being limited in the directions x and z by the curve

$$f(xz) = 0.$$

For boundary conditions

$$\left.\begin{array}{llll} y=0 & u=U_0 & w=0 & v=0 \\ y=h & u=U_1 & w=0 & v=U_1\dfrac{dh}{dx}+V_1 \\ & f(xz)=0 & & p=p_0 \end{array}\right\} \quad \text{.........(15).}$$

21. Equations (13) may now be integrated, the constants being determined by the conditions (15).

The second of these equations gives p independent of y, so that the first and third are directly integrable, whence

$$\left.\begin{aligned} u &= \frac{1}{2\mu}\frac{dp}{dx}(y-h)\,y + U_0\frac{h-y}{h} + U_1\frac{y}{h} \\ w &= \frac{1}{2\mu}\frac{dp}{dz}(y-h)\,y \end{aligned}\right\} \quad \text{...............(16).}$$

Differentiating these equations with respect to x and z respectively, and substituting in the last of equations (13)

$$\frac{dv}{dy} = -\frac{1}{2\mu}\left[\frac{d}{dx}\left\{\frac{dp}{dx}(y-h)\,y\right\} + \frac{d}{dz}\left\{\frac{dp}{dz}(y-h)\,y\right\}\right] - \frac{d}{dx}\left\{U_0\frac{h-y}{h} + U_1\frac{y}{h}\right\}.$$

Integrating from $y=0$ to $y=h$, and substituting from conditions (15)

$$\frac{d}{dx}\left(h^3\frac{dp}{dx}\right) + \frac{d}{dz}\left(h^3\frac{dp}{dz}\right) = 6\mu\left\{(U_0+U_1)\frac{dh}{dx} + 2V_1\right\} \quad \text{.........(17).}$$

From equations (16) and (14)

$$\left.\begin{aligned} p_{yz} &= \frac{1}{2}\frac{dp}{dx}(2y-h) + \mu\,(U_1-U_0)\frac{1}{h} \\ p_{yz} &= \frac{1}{2}\frac{dp}{dz}(2y-h) \end{aligned}\right\} \quad \text{..................(18).}$$

Putting $f_x f_z$ for the shearing stresses at the solid on the surfaces in the directions x and z respectively, then taking the positive sign when $y=h$, and the negative when $y=0$

$$\left.\begin{aligned} f_x &= \mu\,(U_1-U_0)\frac{1}{h} \mp \frac{1}{2}\frac{dp}{dx}h \\ f_z &= \mp\frac{1}{2}\frac{dp}{dz}h \end{aligned}\right\} \quad \text{.....................(19).}$$

Equations (17) and (19) are the general equations of equilibrium for the lubricant between continuous surfaces at a distance h, where h is any continuous function of x and z, and μ is constant.

22. For the further integration of these equations it is necessary to know the exact manner in which x and z enter into h, as well as the function which determines the limit of lubricated surfaces.

These integrations have been effected either completely or approximately for certain cases, which include the chief case of practical lubrication.

Complete integration has been obtained for the case of two parallel circular or elliptical surfaces approaching without tangential motion. This case is interesting from the experiment, treated approximately by Stefan*, of one surface-plate floating on another in virtue of the separating film of air. It is introduced here, however, as being the most complete as well as the simplest case in which to consider the important effect of normal motion in the action of lubricants. It corresponds with Case 2, Section III.

Complete integration is also obtained for two plane surfaces

$$h = h_1\left(1 + m\frac{x}{a}\right)$$

between the limits at which $p = \Pi$ (the pressure of the atmosphere)

$$x = 0, \qquad x = a,$$

the surfaces being unlimited in the direction of z. This corresponds with Case 4, Section III.

For the most important case, that of cylindrical surfaces, approximate integration has been effected for the case of complete lubrication with the surfaces unlimited in the direction of z. Case 9, Section III.

Section V.—Cases in which the Equations are Completely Integrated.

23. *Two Parallel Plane Surfaces approaching each other, the Surfaces having Elliptical Boundaries.*

Here h is constant over the surfaces, and when

$$\frac{x^2}{a^2} + \frac{z^2}{c^2} = 1, \quad p = \Pi \quad \text{..........................(20)},$$

U_0, U_1 are zero.

* *Wien. Sitz. Ber.*, vol. 69 (1874), p. 713.

Equation (17) becomes

$$\left(\frac{d}{dx^2}+\frac{d}{dz^2}\right)p=\frac{12\mu}{h^3}\frac{dh}{dt} \quad \text{.........................(21).}$$

The solution of which is

$$p=\phi(t)\left\{\frac{x^2}{a^2}+\frac{z^2}{c^2}+C_1\right\}+E_1e^{-\frac{z}{a}}\sin\frac{y}{c}+\&\text{c.} \quad \text{............(22).}$$

Therefore
$$2\phi(t)\left(\frac{1}{a^2}+\frac{1}{c^2}\right)=\frac{12\mu}{h^3}\frac{dh}{dt} \quad \text{.........................(23),}$$

and
$$E_1=0, \ \&\text{c.}$$

$$C_1=-1+\frac{\Pi}{\phi(t)}$$

$$p-\Pi=\frac{12\mu}{h^3}\cdot\frac{a^2c^2}{a^2+c^2}\left\{\frac{x^2}{a^2}+\frac{z^2}{c^2}-1\right\}\frac{dh}{dt} \quad \text{............(24).}$$

From equations (19)

$$\left.\begin{aligned} f_x&=\mp\frac{24\mu}{h^3}\frac{a^2c^2}{a^2+c^2}\cdot x\frac{dh}{dt}\\ f_z&=\mp\frac{24\mu}{h^3}\frac{a^2}{a^2+c^2}\cdot z\frac{dh}{dt}\end{aligned}\right\} \quad \text{.........................(25),}$$

supposing surfaces horizontal and the upper surface supported solely by the pressure of the fluid. The conditions of equilibrium in this case are obvious by symmetry.

The centre of gravity of the load must be vertically over the centre of the ellipse. Since by symmetry

$$\left.\begin{aligned} \int_0^c\int_0^{a\sqrt{\left(1-\frac{z^2}{c^2}\right)}}pxdxdz&=0\\ \int_0^c\int_0^{a\sqrt{\left(1-\frac{z^2}{c^2}\right)}}pzdxdz&=0\\ \int_0^c\int_0^{a\sqrt{\left(1-\frac{z^2}{c}\right)}}f_xdxdz&=0\\ \int_0^c\int_0^{a\sqrt{\left(1-\frac{z^2}{c}\right)}}f_zdxdz&=0\end{aligned}\right\} \quad \text{.....................(26).}$$

And
$$W = \int_0^c \int_0^{a\sqrt{1-\frac{z^2}{c^2}}} p - \Pi dx dy \quad \text{.....................(27)}$$

$$= -\frac{3\mu\pi}{h^3} \frac{a^3c^3}{a^2+c^2} \cdot \frac{dh}{dt} \quad \text{.........................(28).}$$

Therefore integrating

$$t = \frac{3\mu\pi a^3c^3}{(a^2+c^2)W}\left(\frac{1}{h_2^2} - \frac{1}{h_1^2}\right) \quad \text{.........................(29),}$$

t being the time occupied in falling from h_1 to h_2.

24. *Plane Surfaces of unlimited Length and parallel in the Direction of z.*

The lower surface unlimited in the direction x and moving with a velocity $-U$. The upper surface fixed and extending from $x=0$ to $x=a$. This case corresponds with Case 4, Section III.

The boundary conditions are

$$\left.\begin{array}{lll} \left.\begin{array}{l} x=0 \\ x=a \end{array}\right\} & p=\Pi & \\ y=0 & U_0=-U & \\ y=h & U_1=0 & V_1=0 \\ h=h_0\left(1+m\dfrac{x}{a}\right) & & \end{array}\right\} \quad \text{..................(30)}$$

p is a function of x only.

And from equation (17), Section IV., by integration

$$\frac{dp}{dx} = -6\mu U\frac{h-h_1}{h^3} \quad \text{............................(31),}$$

h_1 being the value of h when $x=x_1$ where the pressure is a maximum.

Integrating with respect to x, and putting $p=\Pi$ at the boundaries

$$x_1 = \frac{a}{2+m} \quad \text{.................................(32)}$$

$$p = \Pi + 6\frac{\mu Ua}{h^2m}\left\{\frac{1}{1+m\dfrac{x}{a}} - \frac{1+m}{2+m}\frac{1}{\left(1+m\dfrac{x}{a}\right)} - \frac{1}{2+m}\right\} \quad \text{......(33)}$$

$$\int_0^a (p-\Pi)\,dx = \frac{6\mu Ua^2}{h^2m^2}\left\{\log_e(1+m) - \frac{m}{1+\dfrac{m}{2}}\right\} \quad \text{...........(34),}$$

or putting W for the load per unit of breadth, W is a maximum when $m = 1{\cdot}2$ approximately and

$$\frac{W}{a} = {\cdot}16\mu U \frac{a^2}{h^2} \quad \text{.............................(35)};$$

again, by equation (19)

$$f_x = \mu \frac{U_1}{h} \quad \text{................................(36)};$$

therefore

$$\int_0^a f_x dx = \frac{\mu a U}{h_1 m} \log_e (1 + m) \quad \text{........................(37)};$$

and if

$$m = 1{\cdot}2$$

$$F = {\cdot}6572 \frac{\mu a U}{h_1} \quad \text{..............................(38)}.$$

In order to render the application of equations (35) and (38) clear, a particular case may be assumed.

Let $\mu = 10^{-5}$,

which is the value for olive oil at a temperature of 70° Fahr., the unit of length being the inch, and that of force the lb.

Let

$$U = 60 \text{ (inches per sec.)}$$

$$h_1 = {\cdot}0003.$$

Then from (35), the load in lbs. per square inch of lubricated surface is given by

$$\frac{W}{a} = 1070a^2,$$

and from (38), the frictional resistance in lbs. per square inch is

$$\frac{F}{a} = 1{\cdot}31.$$

This seems to be about the extreme case of perfect lubrication between plane metal surfaces having what appears to be about the minimum value of h_1.

Section VI.—The Integration of the Equations for the Case of Cylindrical Surfaces.

25. *General Adaptation of the Equations.*

Fig. 17 represents a section of two circular cylindrical surfaces at right angles to the axes; as in Art. 17.

J is the axis of the journal AB;

I is the axis of the brass CD;

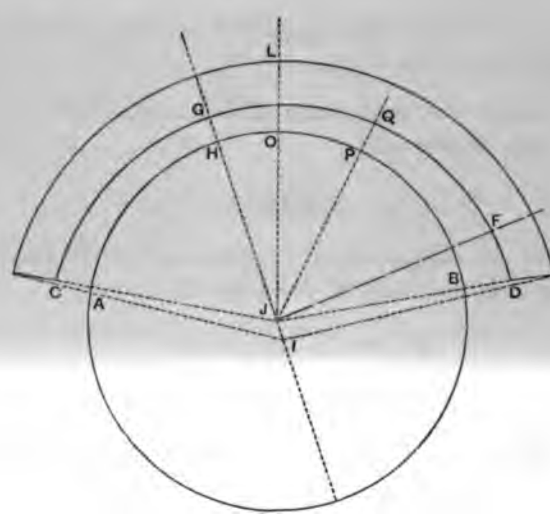


Fig. 17.

JO is the line of action of the load cutting the brass symmetrically, and

$$\left.\begin{aligned}
R &= JP \\
R + a &= IQ \\
h &= PQ \\
h_0 &= HG, \text{ the smallest section} \\
JI &= ca \\
PJO &= \theta \\
GJO &= \frac{\pi}{2} - \phi_0 \\
P_1JO &= \phi_1. \quad P_1 \text{ being the point of maximum pressure.}
\end{aligned}\right\} \quad \ldots \; (39).$$

Then taking x for distances measured in the direction OA from O on the surface AB, and putting r for the distance of any point from J,

$$\left.\begin{aligned} x &= R\theta \\ y &= r - R \end{aligned}\right\} \quad \text{.............................(40)}$$

$$x_1 = R\phi_1 \quad \text{...................................(41).}$$

Neglecting quantities of the order $\overline{\frac{ca}{R}}^2$

$$h = a\{1 + c\sin(\theta - \phi_0)\} \quad \text{........................(42).}$$

For if I be moved up to J, Q moves through a distance ca in the direction JH.

The boundary conditions are such that

(1) all quantities are independent of z;

(2) U_0 is constant, U_1 and $V_1 = 0$;

(3) putting $\theta_0 = OJA$, $\theta_1 = OJB$, whence by symmetry $\theta_0 = -\theta_1$,

$$\left.\begin{aligned} \theta &= \theta_0 \\ \theta &= \theta_1 \end{aligned}\right\} \dots p = p_0 \quad \text{..........................(43).}$$

Putting $-L$ for the effect of the external load and $-M$ for the external moment per unit of length in the direction z, and assuming that there are no external horizontal forces, the conditions of equilibrium for the brass are

$$\int_{\theta_0}^{\theta_1} \{p\sin\theta - f\cos\theta\}\, d\theta = 0 \quad \text{........................(44)}$$

$$\int_{\theta_0}^{\theta_1} \{p\cos\theta + f\sin\theta\}\, d\theta = \frac{L}{R} \quad \text{......................(45)}$$

$$\int_{\theta_0}^{\theta} f d\theta = \frac{M}{R^2} \quad \text{...(46).}$$

Substituting from equations (40), (41), (42) in equations (17) and (19), Section IV., putting

$$K_1 = \frac{6R\mu U_0}{a^2}, \quad K_2 = \frac{\mu U_0}{a} \quad \text{........................(47)}$$

and remembering the boundary conditions, these equations become on integration

$$\frac{dp}{d\theta} = \frac{6R\mu U_0 c\{\sin(\theta - \phi_0) - \sin(\phi_1 - \phi_0)\}}{a^2\{1 + c\sin(\theta - \phi_0)\}^3} \quad \text{...............(48)}$$

$$f = -\frac{3\mu U_0 c\,\{\sin(\theta-\phi_0) - \sin(\phi_1-\phi_0)\}}{a\,\{1 + c\sin(\theta-\phi)\}^2}$$

$$-\frac{\mu U_0}{\{1+c\sin(\theta-\phi)\}} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(49).$$

26. *The Method of Approximate Integration.*

The second numbers of equations (48) and (49) may be expanded so that

$$\left.\begin{aligned}\frac{1}{K_1 c}\frac{dp}{d\theta} &= A_0 + A_1\sin(\theta-\phi_0) + A_2\cos 2(\theta-\phi) + \&c.\\ &\quad + A_{2n}\cos 2x(\theta-\phi_0) + A_{2n+1}\sin\{(2x+1)(\theta-\phi)\}\end{aligned}\right\}\ldots(50)$$

$$\left.\begin{aligned}-\frac{1}{K_2}f &= B_0 + B_1\sin(\theta-\phi_0) + B_2\cos 2(\theta-\phi) + \&c.\\ &\quad + B_{2n}\cos 2x(\theta-\phi) + B_{2n+1}\sin\{(2x+1)(\theta-\phi)\}\end{aligned}\right\}\ldots(51).$$

Putting $$\chi = \sin(\phi_1-\phi_0)\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(52)$$

$$A_0 = -\chi - \sum_{r=2}^{r=2\infty}\left\{\frac{(r+1)r^2(r-1)\ldots\frac{r+2}{2}}{2^{r+1}\,\underline{\left|\frac{r}{2}\right.}}c^{r-1} + \frac{(r+2)(r+1)r(r-1)\ldots\frac{r+2}{2}}{2^{r+1}\,\underline{\left|\frac{r}{2}\right.}}c^r\chi\right\}$$

$$\begin{aligned}A_{2n} = (-1)^{n+1}&\left\{\frac{(2n+1)\,2n}{2^{2n}}c^{2n+1} + \frac{(2n+2)(2n+1)}{2^{2n}}c^{2n}\chi\right.\\ &\left. + \sum_{r=2n+2}^{r=2\infty}\left[\frac{(r+1)r^2(r-1)\ldots\frac{r+2n+2}{2}}{2^r\,\underline{\left|\frac{r}{2}-n\right.}}c^{r-1} + \frac{(r+2)(r+1)r\ldots\frac{r+2n+2}{2}}{2^r\,\underline{\left|\frac{r}{2}-n\right.}}c^r\chi\right]\right\}\end{aligned}$$

$$\begin{aligned}A_{2n+1} = (-1)^n&\left\{\frac{(2n+2)(2n+1)}{2^{2n+1}}c^{2n} + \frac{(2n+3)(2n+2)}{2^{2n+1}}c^{2n+1}\chi\right.\\ &\left. + \sum_{r=2n+3}^{r=2\infty+1}\left[\frac{(r+1)r^2(r-1)\ldots\frac{r+2n+3}{2}}{2^r\,\underline{\left|\frac{r-2n-1}{2}\right.}}c^{r-1} + \frac{(r+2)(r+1)r\ldots r+2n+2}{2^r\,\underline{\left|\frac{r-2n-1}{2}\right.}}c^r\chi\right]\right\}\end{aligned}$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(53).$$

$$= 1 - 3c\chi + \sum_{r=2}^{r=2\infty}\left[\frac{[4-3(r+1)]\,r.(r-1)\ldots\frac{r+2}{2}}{2^r\,\underline{\left|\frac{r}{2}\right.}}c^r - \frac{3(r+1)\,r(r-1)\ldots\frac{r+1}{2}}{2^r\,\underline{\left|\frac{r}{2}\right.}}c^{r+1}\chi\right]$$

$$= (-1)^n\left\{\frac{[4-3(2n+1)]\,c^{2n} - 3(2n+1)\,c^{2n+1}\chi}{2^{2n-2}}\right.$$

$$\left.+\sum_{r=2n+2}^{r=2\infty}\left[[4-3(r+1)]\,c^r - 3(r+1)\,c^{r+1}\chi\right]\frac{r.(r-1)\ldots\frac{r+2n+2}{2}}{2^{r-1}\,\underline{\left|\frac{r-2n}{2}\right.}}\right\}$$

$$_{+1} = (-1)^{n+1}\left\{\frac{[4-3(2n+2)]\,c^{2n+1} - 3(2n+2)\,c^{2n+2}\chi}{2^{2n}}\right.$$

$$\left.+\sum_{r=2n+3}^{r=2\infty+3}\left[[4-3(r+1)]\,c^r - 3(r+1)\,c^{r+1}\chi\right]\frac{r.(r+1)\ldots r+2n+3}{2^{r-1}\,\underline{\left|\frac{r-2n-1}{2}\right.}}\right\}$$

..................................(54).

The coefficients A_0, A_1, &c., B_0, B_1, &c., are thus expanded in a series of ascending powers of c with numerical coefficients which do not converge. It seems, however, that if c is not greater than ·6 the series are themselves convergent, and it is only necessary to go to the tenth or twelfth term, to which extent they have been calculated, and are as follows:—

$$\left.\begin{aligned}
A_0 &= -1{\cdot}5c - 3{\cdot}75c^3 - 6{\cdot}565c^5 - 9{\cdot}85c^7 - 13{\cdot}51c^9 - 17{\cdot}6c^{11}\\
&\qquad - \{1 + 3c^2 + 5{\cdot}625c^4 + 8{\cdot}75c^6 + 12{\cdot}225c^8 + 16{\cdot}2c^{10}\}\,\chi\\
A_1 &= 1 + 4{\cdot}5c^2 + 9{\cdot}375c^4 + 15{\cdot}23c^6 + 21{\cdot}92c^8 + 29{\cdot}8c^{10} + 38{\cdot}6c^{12}\\
&\qquad + \{3c + 7{\cdot}5c^3 + 13{\cdot}13c^5 + 19{\cdot}7c^7 + 27{\cdot}01c^9 + 35{\cdot}2c^{11}\}\,\chi\\
A_2 &= 1{\cdot}5c + 5c^3 + 9{\cdot}85c^5 + 15{\cdot}75c^7 + 22{\cdot}56c^9 + 20{\cdot}24c^{11}\\
&\qquad + \{3c^2 + 7{\cdot}5c^4 + 13{\cdot}13c^6 + 19{\cdot}7c^8 + 27{\cdot}02c^{10} + 35{\cdot}2c^{12}\}\,\chi\\
A_3 &= -1{\cdot}5c^2 - 4{\cdot}7c^4 - 9{\cdot}2c^6 - 14{\cdot}7c^8 - 21{\cdot}45c^{10}\\
&\qquad - \{2{\cdot}5c^3 + 6{\cdot}56c^5 + 11{\cdot}78c^7 + 18{\cdot}03c^9 + 25{\cdot}4c^{11}\}\,\chi\\
A_4 &= -1{\cdot}25c^3 - 3{\cdot}94c^5 - 7{\cdot}875c^7 - 12{\cdot}88c^9 - 18{\cdot}8c^{11}\\
&\qquad - \{1{\cdot}875c^4 + 5{\cdot}25c^6 + 9{\cdot}85c^8 + 15{\cdot}48c^{10} + 22{\cdot}45c^{12}\}\,\chi\\
A_5 &= {\cdot}939c^4 + 3{\cdot}07c^6 + 6{\cdot}33c^8 + 10{\cdot}68c^{10}\\
&\qquad + \{2{\cdot}63c^5 + 3{\cdot}94c^7 + 7{\cdot}76c^9 + 12{\cdot}6c^{11}\}\,\chi
\end{aligned}\right\}\quad\ldots\ldots(55).$$

$$\left.\begin{aligned}
B_0 &= 1 - \{2{\cdot}5c^2 + 4{\cdot}125c^4 + 5{\cdot}3125c^6 + 6{\cdot}54c^8\} \\
&\quad - \{3c + 4{\cdot}5c^3 + 5{\cdot}625c^5 + 6{\cdot}562c^7 + 7{\cdot}63c^9\}\,\chi \\
B_1 &= 2c + 6c^3 + 9{\cdot}75c^5 + 11{\cdot}3125c^7 \\
&\quad + \{6c^2 + 9c^4 + 11{\cdot}25c^6 + 12{\cdot}5c^8\}\,\chi \\
B_2 &= 2{\cdot}5c^2 + 5{\cdot}5c^4 + 7{\cdot}97c^6 + 10{\cdot}06c^8 \\
&\quad + \{4{\cdot}5c^3 + 7{\cdot}5c^5 + 9{\cdot}85c^7 + 11{\cdot}8c^9\}\,\chi \\
B_3 &= -2c^3 - 4{\cdot}375c^5 - 6{\cdot}5625c^7 - 8{\cdot}61c^9 \\
&\quad - \{3c^4 + 5{\cdot}625c^6 + 7{\cdot}873c^8 + 9{\cdot}84c^{10}\}\,\chi \\
B_4 &= -1{\cdot}375c^4 - 3{\cdot}2c^6 - 5{\cdot}03c^8 \\
&\quad - \{1{\cdot}875c^5 + 3{\cdot}94c^7 + 5{\cdot}09c^9\}\,\chi
\end{aligned}\right\} \quad \ldots\ldots\ldots(56).$$

27. *The Integration of the Equations.*

Integrating equation (50) between the limits θ_0 and θ

$$\begin{aligned}
\frac{p - p_0}{K_1 c} &= A_0(\theta - \theta_0) \\
&\quad - A_1\{\cos(\theta - \phi_0) - \cos(\theta_0 - \phi_0)\} \\
&\quad + \frac{A_2}{2}\{\sin 2(\theta - \phi_0) - \sin 2(\theta_0 - \phi_0)\} \\
&\qquad \&\text{c.} \qquad\qquad \&\text{c.} \\
&\quad - \frac{A_{2n+1}}{2n+1}\{\cos[(2n+1)(\theta - \phi_0)] - \cos[(2n+1)(\theta_0 - \phi_0)]\} \\
&\quad + \frac{A_{2n}}{2n}\{\sin 2n(\theta - \phi_0) - \sin 2n(\theta_0 - \phi_0)\} \quad \ldots\ldots\ldots\ \ldots\ldots\ldots(57),
\end{aligned}$$

whence putting $\theta = \theta_1$ by condition (43)

$$\begin{aligned}
0 &= A_0\theta_1 - A_1 \sin\theta_1 \sin\phi_0 + \frac{A_2}{2}\sin 2\theta_1 \cos 2\phi_0 - \&\text{c.} \\
&\quad - \frac{A_{2n+1}}{2n+1}\sin[(2n+1)\theta_1]\sin(2n+1)\phi_0 \\
&\quad + \frac{A_{2n}}{2n}\sin 2n\theta_1 \cos 2n\phi_0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ \ldots\ldots\ldots(58).
\end{aligned}$$

Putting
$$\begin{aligned}
E &= A_1 \cos\theta_1 \cos\phi_0 + \frac{A_2}{2}\cos 2\theta_1 \sin 2\phi_0 + \&\text{c.} \\
&\quad + \frac{A_{2n+1}}{2n+1}\cos[(2n+1)\theta_1]\cos[(2n+1)\phi_0 \\
&\quad + \frac{A_{2n}}{2n}\cos 2n\theta_1 \sin 2n\phi_0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(59),
\end{aligned}$$

whence from equations (57) and (58)

$$\frac{p-p_0}{K_1c} = E + A_0\theta - A_1\cos(\theta-\phi_0) + \frac{A_2}{2}\sin 2(\theta-\phi_0)$$
$$-\frac{A_{2n+1}}{2n+1}\cos[(2n+1)(\theta-\phi_0)]$$
$$+\frac{A_{2n}}{2n}\sin 2n(\theta-\phi_0) \quad\text{.................................(60)}.$$

Multiplying equation (60) by $\sin\theta$ and integrating between θ_0 and θ_1, remembering that $\theta_0 = -\theta_1$

$$\int_{\theta_0}^{\theta_1}\frac{p-p_0}{Kc}\sin\theta d\theta = 2A_0(\sin\theta_1 - \theta_1\cos\theta_1)$$
$$+A_1\left(\frac{\sin 2\theta_1\sin\phi_0}{2} - \theta_1\sin\phi_0\right)$$
$$+\frac{A_2}{2}\left(\sin\theta\cos 2\phi - \frac{\sin 3\theta\cos 2\phi}{3}\right)$$
$$\&c. \qquad \&c.$$
$$+\frac{A_{2n+1}}{2n+1}\left\{\frac{\sin(2n+2)\theta_1\sin(2n+1)\phi_0}{2n+2} - \frac{\sin 2n\theta_1\sin(2n+1)\phi_0}{2n}\right\}$$
$$\frac{A_{2n}}{2n}\left\{\frac{\sin(2n+1)\theta_1\cos 2n\phi}{2n-1} - \frac{\sin(2n+1)\theta\cos 2n\phi}{2n+1}\right\} \quad\text{......(61)}.$$

Multiplying equation (60) by $\cos\theta$, and integrating from θ_0 to θ_1

$$\int_{\theta_0}^{\theta_1}\frac{p-p_0}{Kc}\cos\theta d\theta = -A_1\left(\theta_1\cos\phi_0 - \frac{\sin 2\theta_1}{2}\cos\phi_0\right)$$
$$+\frac{A_2}{2}\left(\frac{\sin 3\theta_1}{6}\sin 2\phi_0 - \tfrac{3}{2}\sin\theta\sin 2\phi\right) - \&c.$$
$$+\frac{A_{2n+1}}{2n+1}\left\{\frac{2n+1}{2n+2}\sin(2n+2)\theta\cos(2n+1)\phi - \frac{2n+1}{2n}\sin 2n\theta_1\cos(2n+1)\phi\right\}$$
$$+\frac{A_{2n}}{2n}\left\{\frac{2n}{2n+1}\sin(2n+1)\theta\sin 2n\phi - \frac{2n}{2n-1}\sin(2n-1)\theta\sin 2n\phi\right\}\text{...(62)}.$$

Multiplying equation (51) by $\cos\theta$ and integrating

$$-\int_{\theta}^{\theta}\frac{f\cos\theta}{K_2} = 2B_0\sin\theta_1 - B_1\left(\frac{\sin 2\theta_1\sin\phi_0}{2} + \theta_1\sin\phi_0\right)$$
$$+B_2\left(\frac{\sin 3\theta_1\cos 2\phi_0}{3} + \sin\theta_1\cos 2\phi_0\right)$$
$$-\&c.$$
$$+B_{2n}\left\{\frac{\sin(2n+1)\theta_1\cos 2n\phi_0}{2n+1} + \frac{\sin(2n-1)\theta_1\cos 2n\phi_0}{2n-1}\right\}$$
$$-B_{2n+1}\left\{\frac{\sin(2n+2)\theta_1\sin(2n+1)\phi_0}{2n+2} + \frac{\sin 2n\theta_1\sin(2n+1)\phi_0}{2n}\right\}$$
$$\text{.....................(63)}.$$

Multiplying equation (51) by $\sin\theta$ and integrating

$$-\int_{\theta_0}^{\theta_1}\frac{f\sin\theta\,d\theta}{K_2}=B_1\left(\frac{\sin 2\theta_1\cos\phi_0}{2}-\theta_1\cos\phi_0\right)$$

$$-B_2\left(\frac{\sin 3\theta_1\sin 2\phi_0}{3}-\sin\theta_1\sin 2\phi_0\right)$$

$$-B_{2n}\left\{\frac{\sin(2n+1)\theta_1\sin 2n\phi_0}{2n+1}-\frac{\sin(2n-1)\theta_1\sin 2n\phi_0}{2n-1}\right\}$$

$$+B_{2n+1}\left\{\frac{\sin(2n+2)\theta_1\cos(2n+1)\phi_0}{2n+2}-\frac{\sin 2n\theta_1\cos(2n-1)\phi_0}{2n}\right\}$$

......(64).

Integrating equation (51)

$$-\int_{\theta_0}^{\theta_1}\frac{f\,d\theta}{K_2}=2B_0\theta_1-2B_1\sin\theta_1\sin\phi_0+\frac{2B_2}{2}\sin 2\theta_1\cos 2\phi_0$$

$$+\frac{2B_{2n}}{2n}\sin 2n\theta_1\cos 2n\phi_0$$

$$-\frac{2B_{2n+1}}{2n+1}\sin(2n+1)\theta_1\sin(2n+1)\phi_0 \qquad \text{.....................(65).}$$

Substituting from equations (61)—(65) in the equations of equilibrium (44), (45), (46), there results—

From (44)

$$0=2(K_1cA_0+K_2B_0)\sin\theta_1-2K_1cA_0\theta_1\cos\theta_1$$

$$+(K_1cA_1-K_2B_1)\frac{\sin 2\theta_1}{2}\sin\phi_0-(K_1cA_1+K_2B_1)\theta_1\sin\phi_0$$

$$+\ \&c.$$

$$+\sum_{n=1}^{n=\infty}\left\{\left(\frac{K_1cA_{2n}}{2n}+K_2B_{2n}\right)\frac{\sin(2n-1)\theta_1\cos 2n\phi_0}{2n-1}\right.$$

$$-\left(\frac{K_1cA_{2n}}{2n}-K_2B_{2n}\right)\frac{\sin(2n+1)\theta_1\cos 2n\phi_0}{2n+1}$$

$$-\left(\frac{K_2cA_{1n+1}}{2n+1}+K_2B_{2n+1}\right)\frac{\sin 2n\theta_1\sin(2n+1)\phi_0}{2n}$$

$$\left.+\left(\frac{K_1cA_{3n+1}}{2n+1}-K_2B_{2n+1}\right)\frac{\sin(2n+1)\theta_1\sin(2n+1)\phi_0}{2n+2}\right\} \qquad \text{......(66).}$$

From (45)

$$\frac{L}{R} = (K_1cA_1 - K_2B_1)\left(\frac{\sin 2\theta_1}{2} - \theta_1\right)\cos\phi_0$$

$$+ \sum_{n=1}^{n=\infty}\left\{(K_1cA_{2n} + K_2B_{2n})\left[\frac{\sin(2n+1)\theta_1}{2n+1} - \frac{\sin(2n-1)\theta_1}{2n-1}\right]\sin 2n\phi_0\right.$$

$$\left. + (K_1cA_{2n+1} - K_2B_{2n+1})\left[\frac{\sin(2n+2)\theta_1}{2n+2} - \frac{\sin 2n\theta_1}{2n}\right]\cos(2n+1)\phi_0\right\} \quad (67)$$

From (46)

$$-\frac{M}{R^2} = K_2\left\{2B_0\theta_1 - 2B_1\sin\theta_1\sin\phi_0 + \frac{2B_2}{2}\sin 2\theta_1\cos 2\phi_0\right.$$

$$+ \frac{2B_{2n}}{2n}\sin 2n\theta_1\cos 2n\phi_0$$

$$\left. - \frac{2B_{2n+1}}{2n+1}\sin(2n+1)\theta_1\sin(2n+1)\phi_0\right\} \quad \text{.....................(68).}$$

The equations (66), (67), (68), together with (58), which expresses the boundary conditions as regards pressure, are the integral equations of equilibrium for the fluid between the brass and journal, and hence for the brass.

The quantities involved in these equations are

$$R,\ U,\ M,\ L,\ \mu,\ \theta_1 \text{ and } a,\ c,\ \phi_0,\ \phi_1.$$

If, therefore, the former are given, the latter are determined by the solution of these equations.

SECTION VII.—SOLUTION OF THE EQUATIONS FOR CYLINDRICAL SURFACES.

28. *c and* $\sqrt{\frac{a}{R}}$ *small compared with Unity.*

In this case equations (55) become

$$\left.\begin{array}{ll} A_0 = -\chi & B_0 = 1 \\ A_1 = \ \ 1 & B_1 = 0 \\ A_2 = \ \ 0, \text{ \&c.} & B_2 = 0, \text{ \&c.} \end{array}\right\} \quad \text{.....................(69).}$$

Equation (58) gives

$$0 = \chi\theta_1 + \sin\theta_1\sin\phi_0 \quad \text{...........................(70).}$$

Equation (66) gives

$$0 = (2K_1c\chi - 2K_2)\sin\theta_1 - 2K_1c\chi\theta_1\cos\theta_1 - K_1c\frac{\sin 2\theta_1}{2}\sin\phi_0 + K_1c\theta_1\sin\phi_0 \quad\text{..............(71).}$$

Equation (67) gives

$$\frac{L}{R} = K_1c\left(\frac{\sin 2\theta_1}{2} - \theta_1\right)\cos\phi_0 \quad\text{....................(72)}$$

and equation (68) gives

$$\frac{M}{R^2} = -2K_2\theta_1 \quad\text{..............................(73).}$$

Also equation (57)

$$p - p_0 = K_1c\,\{\cos\theta_1\cos\phi_0 - \chi\theta - \cos(\theta - \phi)\} \quad\text{...........(74).}$$

Eliminating χ between equations (70) and (71)

$$\sin\phi_0 = \frac{K_2}{K_1}\frac{\sin\theta_1}{c\left(\theta - \frac{\text{[illegible]}}{\theta_1} + \frac{1}{2}\sin 2\theta\right)} \quad\text{...............(75).}$$

The equations (74) and (75) suffice to determine a, c, ϕ_1 and ϕ_0 under the conditions $\sqrt{\frac{a}{R}}$ and c small so long as ϕ_0 is not small, in which case the terms retained in the equations become so small that some that have been neglected rise into relative importance.

To commence with let

$$L = 0.$$

(Cases 5 and 9, Section III.)

Then by (72)

$$\cos\phi_0 = 0$$

and by (70)

$$\chi = -\frac{\sin\theta_1}{\theta_1},$$

putting for χ its value $\sin(\phi_1 - \phi_0)$

$$\cos\phi_1 = \frac{\sin\theta_1}{\theta_1} \quad\text{..............................(76).}$$

Equation (76) gives two equal values of opposite signs for ϕ_1. These correspond to the positions of P_1 and P_2, the points of maximum and minimum pressure.

For the extreme cases

$$\left.\begin{array}{ll} \theta_1 = 0 & \phi_1 = \pm\sqrt{\tfrac{2}{3}}\,\theta_1 \\ \theta_1 = \frac{\pi}{2} & \phi_1 = \pm\cos^{-1}\frac{2}{\pi} \\ \theta_1 = \pi & \phi = 0_1 \end{array}\right\} \quad\text{........................(77).}$$

From equations (73) and (47)

$$a = \frac{2\mu U\theta}{M} \quad \text{.................................(78)}$$

and from (75)
$$c = \frac{a}{3R} \frac{\sin\theta_1}{\left(\theta_1 - \dfrac{2\sin^2\theta_1}{\theta_1} + \frac{1}{2}\sin 2\theta_1\right)} \quad \text{.................(79).}$$

When L increases

From (72) and (75)

$$\tan\phi_0 = \frac{R}{L} K_2 \left(\frac{\sin 2\theta_1}{2} - \theta_1\right) \frac{2\sin\theta_1}{\left(\theta_1 - \dfrac{2\sin^2\theta_1}{\theta_1} + \frac{1}{2}\sin 2\theta_1\right)} \quad \text{......(80).}$$

Hence as L increases $\tan\phi_0$ diminishes until the approximation fails. This, however, does not happen so long as c is small.

As the load increases from zero, equation (80) shows that G moves away from O towards A.

It also appears from equation (70) that χ and ϕ_1 diminish as ϕ_0 diminishes, and that ϕ_1 is positive as long as the equations hold.

To proceed further it is necessary to retain all terms of the first order of small quantities.

Retaining the first power of c only, equations (55) become

$$\left.\begin{array}{ll} A_0 = -1\cdot5c - \chi & B_0 = 1 - 3c\chi \\ A_1 = 1 + 3c\chi & B_1 = 2c \\ A_2 = 1\cdot5c & B_2 = 0 \end{array}\right\} \quad \text{..................(81).}$$

From (58)

$$\chi(\theta_1 + 3c\sin\theta_1\sin\phi_0) = -\sin\theta_1\sin\phi_0 - 1\cdot5c\theta_1 + 1\cdot5c\frac{\sin 2\theta_1}{2}\cos 2\phi_0 \text{...(82).}$$

From (66)

$$\begin{aligned} 0 = &\{-2K_1c(1\cdot5c + \chi) + 2K_2(1 - 3c\chi)\}\sin\theta_1 \\ &+ 2K_1c(1\cdot5c + \chi)\,\theta_1\cos\theta_1 \\ &+ \{K_1c(1 + 3c\chi) - 2K_2c\}\frac{\sin 2\theta_1}{2}\sin\phi_0 \\ &- \{K_1c(1 + 3c\chi) + 2K_2c\}\,\theta_1\sin\phi_0 \\ &+ \tfrac{3}{4}K_1c^2\left(\sin\theta_1 - \frac{\sin 3\theta_1}{3}\right)\cos 2\phi_0 \quad \text{.....................(83).} \end{aligned}$$

From (67)

$$\frac{L}{R} = -\{K_1 c (1 + 3c\chi) - 2K_2 c\}\left(\theta_1 - \frac{\sin 2\theta_1}{2}\right)\cos\phi_0$$
$$+ K_1 c\frac{1{\cdot}5c}{4}\left(\tfrac{1}{3}\sin 3\theta_1 \sin 2\phi_0 - \sin\theta_1 \sin 2\phi_0\right) \quad \text{......(84).}$$

From (68)

$$\frac{M}{R^2} = -K_2\{2(1 - 3c\chi)\theta_1 - 4c\sin\theta_1 \sin\phi_0\} \quad \text{...............(85).}$$

In the equations (82) to (85) terms have been retained as far as the second power of c, but these terms have very unequal values. As χ and $\sin\phi_0$ diminish c increases, and the products of $c\chi$ or $c\sin\phi_0$ may be regarded as never becoming important and be omitted when multiplied by $K_1 c$ or K_2.

Making such omissions and eliminating χ between (82) and (83)

$$\sin\phi_0 = \frac{\dfrac{2K_2}{K_1 c}\theta_1 \sin\theta_1 + c\left\{\tfrac{3}{4}\theta_1\left(\sin\theta_1 - \dfrac{\sin 3\theta_1}{3}\right) - 3(\sin\theta_1 - \theta_1\cos\theta_1)\dfrac{\sin 2\theta_1}{2}\right\}}{\theta_1\left(\theta_1 - \dfrac{\sin 2\theta_1}{2}\right) - 2(\sin\theta_1 - \theta_1\cos\theta_1)\sin\theta_1}$$
$$\text{...............(86).}$$

Equation (86) is a quadratic for c in terms of $\sin\phi_0$, from which it is clear that as c increases from zero ϕ_0 goes through a minimum value when

$$c^2 = \frac{2K_2}{K_1}\frac{\theta_1 \sin\theta_1}{\tfrac{3}{4}\theta_1\left(\sin\theta_1 - \dfrac{\sin 3\theta_1}{3}\right) - 3(\sin\theta_1 - \theta_1\cos\theta_1)\dfrac{\sin 2\theta_1}{2}} \quad \text{......(87).}$$

As the load increases from zero the value of c increases from that of equation (79) to the positive root of (87). As the load continues to increase c further increases, but ϕ_0 again increases, so that, as shown by equation (86), for values of ϕ_0 greater than the minimum there are two loads, two values of c, and two values of χ.

If θ_1 is nearly $\frac{\pi}{2}$, c will be of the order $\sqrt{\frac{a}{2R}}$ when ϕ_0 is small, and $\sin\phi_0$ will be of the order $4c$; so that, so long as $\sqrt{\frac{a}{2R}}$ is sufficiently small, no error has been introduced by the neglect of products and squares of these quantities.

For example

$$\theta_1 = 1{\cdot}37045 \ (78^\circ\ 31'\ 30'' \text{ as in Tower's experiments}) \text{......(88).}$$

By equation (86)

$$\sin\phi_0 = 3{\cdot}934c + 1{\cdot}9847\frac{a}{cR} \qquad (89).$$

And by (87) at the minimum value of ϕ_0

$$\left.\begin{aligned} c &= \sqrt{\frac{a}{2R}} \\ \sin\phi_0 &= 5{\cdot}61\sqrt{\frac{a}{R}} \end{aligned}\right\} \qquad (90).$$

Putting $\chi = \sin\phi_1 - \sin\phi_0$ equation (82) becomes

$$\sin\phi_1 = -{\cdot}16776c + {\cdot}5656\frac{a}{Rc} \qquad (91),$$

or, when ϕ_0 is a minimum,

$$\sin\phi_1 = {\cdot}682\sqrt{\frac{a}{R}} \qquad (92).$$

Therefore

$$\chi = 4{\cdot}928\sqrt{\frac{a}{R}} \qquad (93).$$

Equations (84) and (85) give

$$\frac{L}{R} = -1{\cdot}1753K_1c \qquad (94),$$

$$\frac{M}{R^2} = -2{\cdot}74K_2 \qquad (95),$$

whence equations (47)

$$c = {\cdot}388a\frac{L}{M} \qquad (96),$$

$$\frac{\mu}{a} = -{\cdot}3635\frac{M}{R^2U_0} \qquad (97).$$

So long then as $a\frac{L}{M}$ is not greater than 0·2, these approximate solutions are sufficiently applicable to any case.

For greater values of $\frac{L}{M}$ the solution becomes more difficult, as long however as c is not greater than ·5 the solution can be obtained for any particular value of c.

29. *Further Approximation to the Solution of the Equations for particular Values of c.*

The process here adopted is to assume a value for c. From equations (53) and (54) to find

$$A_0 = A_0' + A_0''\chi \quad B_0 = B_0' + B_0''\chi \text{..................(98)},$$
$$\&c. \qquad \&c.$$

where A_1', A_1'', B_1', B_1'' are numerical.

These coefficients are then introduced into equations (58) and (66) which on eliminating χ give one equation for ϕ_0.

The complex manner in which ϕ_0 enters into the equation renders solution difficult except by trial, in which way values of ϕ_0 corresponding to different values of c have been found.

The value of ϕ_0 substituted in equations (58) or (66) gives χ and ϕ_1.

The corresponding values of c_1, ϕ_0 and χ being thus obtained, a complete table might be calculated. This, however, has not been done, as there does not exist sufficient experimental data to render such a table necessary.

What has been done is to obtain ϕ_1 and ϕ_0 for $c = \cdot 5$, θ_1 having the value 1·3704 as in equation (88) and in Tower's experiments.

The value $c = \cdot 5$ was chosen by a process of trial in order to correspond with the experiments in which Mr Tower measured the pressure at different parts of the journal as described in his second report, and as being the greatest value of c for which complete lubrication is certain.

Putting $c = \cdot 5$, equations (43) and (44) give

$$\left.\begin{array}{llll} A_0 = -1{\cdot}5351 - 2{\cdot}3018\chi & B_0 = -\ {\cdot}012 - 2{\cdot}304\chi \\ A_1 = \ \ 3{\cdot}0723 + 3{\cdot}0721\chi & B_1 = \ \ 2{\cdot}143 + 2{\cdot}286\chi \\ A_2 = \ \ 1{\cdot}8647 + 1{\cdot}5360\chi & B_2 = \ \ 1{\cdot}139 + {\cdot}896\chi \\ A_3 = -\ {\cdot}8911 - {\cdot}6571\chi & B_3 = -\ {\cdot}455 - {\cdot}316\chi \\ A_4 = -\ {\cdot}3753 - {\cdot}2582\chi & B_4 = -\ {\cdot}146 - {\cdot}097\chi \\ A_5 = \ \ {\cdot}1396 + {\cdot}1343\chi & \end{array}\right\} \text{......(99).}$$

$$\left.\begin{array}{l} \text{Taking} \qquad \theta_1 = 1{\cdot}3704 \text{ or } 78^\circ\ 31'\ 30'' \\ \text{it was found by trial that when} \\ \qquad \phi_0 = 48^\circ \end{array}\right\} \text{..................(100),}$$

and K_2 was neglected (under the circumstances K_2 was about $\cdot 0003K_1$), equation (58) gave

$$\left.\begin{array}{l} \chi = -\cdot 82295 \text{ or } -\sin 55^\circ\, 22'\, 40'' \\ \text{and equation (66) gave} \\ \chi = -\cdot 82274 \;\;,, \;\; -\sin 55^\circ\, 21'\, 40'' \end{array}\right\} \dots\dots\dots\dots(101).$$

The difference $\cdot 00021$ being in the same direction and about the magnitude of the effect of neglecting K_2.

This solution was therefore sufficiently accurate, and adopting the value of $\phi_1 - \phi_0$
$$\phi_1 = 7^\circ\, 21'\, 40'' \dots\dots\dots\dots(102).$$

Equations (99) then became

$$\left.\begin{array}{ll} A_0 = \;\;\cdot 3587 & B_0 = \;\;1\cdot 912 \\ A_1 = \;\;\cdot 5449 & B_1 = \;\;2\cdot 263 \\ A_2 = \;\;\cdot 6010 & B_2 = \;\;\cdot 303 \\ A_3 = -\cdot 3505 & B_3 = -\cdot 195 \\ A_4 = -\cdot 0407 & B_4 = \;\;\cdot 066 \\ A_5 = -\cdot 0291 & \end{array}\right\} \dots\dots\dots\dots(103).$$

Substituting the values from equations (100), (102) and (103) in

equation (67) $$\frac{L}{R} = -1\cdot 2752K_1c \dots\dots\dots\dots(104),$$

equation (68) $$\frac{M}{R^2} = -4\cdot 7546K_2 \dots\dots\dots\dots(105).$$

By equation (59) $$E = -\cdot 25257 \dots\dots\dots\dots(106).$$

By equation (60)

$$\begin{aligned} \frac{p - p_0}{K_1c} = -\cdot 25257 + \cdot 35870\theta - \cdot 545 \cos(\theta - 48^\circ) \\ + \cdot 3005 \sin 2(\theta - 48^\circ) + \cdot 1168 \cos 3(\theta - 48^\circ) \\ - \cdot 0407 \sin 4(\theta - 48^\circ) + \cdot 0058 \cos 5(\theta - 48^\circ) \dots(107). \end{aligned}$$

From equation (107) values of

$$\frac{p - p_0}{Kc}$$

have been found for values of θ differing by 10°, and at certain particular values of θ—

$$\left.\begin{array}{ll} \theta = \pm\, 29^\circ\, 20'\, 20'' & \text{points at which the pressure was measured} \\ \theta = -\;\; 7^\circ\, 21'\, 40'' & \text{point of maximum pressure} \\ \theta = -\, 76^\circ\, 38'\, 40'' & \text{point of minimum pressure} \\ \theta = \pm\, 78^\circ\, 31'\, 30'' & \text{the extremities of the brass} \end{array}\right\} \dots(108).$$

These are given in Table II.

TABLE II.—The Pressure at Various Points round the Bearing.

θ off side.	Arc radius = 1.	$\dfrac{p-p_0}{-K_1c}$	θ on side.	Arc radius = 1.	$\dfrac{p-p_0}{-K_1c}$
0° 0′ 0″	0·0	1·0151	0° 0′ 0″		1·0151
− 7 21 40	−0·12837	1·0269			
−10 0 0	−0·17453	1·0232	10 0 0	0·17453	0·9331
−20 0 0	−0·34907	0·9412	20 0 0	0·34907	0·8022
−29 20 20	−0·5120	0·7923	29 20 20	0·5120	0·6609
−40 0 0	−0·6981	0·5612	40 0 0	0·6981	0·5003
−50 0 0	−0·8737	0·3349	50 0 0	0·8737	0·3555
−60 0 0	−1·0472	0·1449	60 0 0	1·0472	0·2249
−70 0 0	−1·2217	0·0293	70 0 0	1·2217	0·1002
−76 38 20	−1·3367	−0·001			
−78 31 20	−1·3704	0·0002	78 31 20	1·3704	0·0002

The Figs. 18, 19, 20 represent the results of Table II.

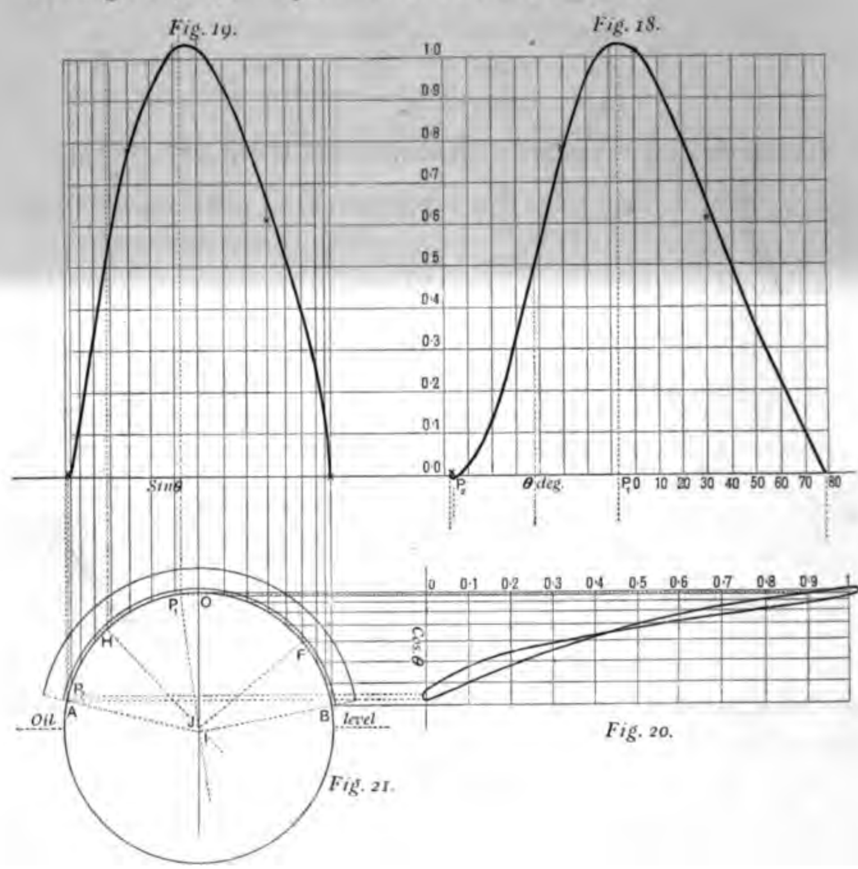


Fig. 19. Fig. 18. Fig. 20. Fig. 21.

In the curve Fig. 18 the ordinates are the pressures, and the abscissæ the arcs corresponding to θ.

In the curve Fig. 19 the ordinates are the same plotted to abscissæ $= R \sin \theta$.

In the curve Fig. 20 the horizontal ordinates are the same as the vertical ordinates in Figs. 18 and 19, and the vertical abscissæ are $= R \cos \theta$.

These theoretical results will be further discussed in Section IX., where they will be compared with Mr Tower's experiments.

29 A. $c = \cdot 5$ *is the Limit to this Method of Integrating.*

In the case considered, in which $\theta = 78° \, 31' \, 20''$, Table II., shows that the pressure towards the extreme off side is just becoming negative. With greater values of c this negative pressure would increase according to the theory.

The possibility of this negative pressure would depend on whether or not the extreme off edge of the brass was completely drowned in the oil bath, a condition not generally fulfilled, and even then it is doubtful to what extent the negative pressure would hold, probably not with certainty below that of the atmosphere.

With an arc of contact anything like that of the case considered it would be necessary, in order to proceed to larger values of c than 5, that the limits between which the equations have been integrated would have to be changed from

$$-\theta_1, \; +\theta_1,$$

to

$$2\left(\phi_0 - \frac{\pi}{2}\right) - \phi_1, \; +\theta_1.$$

This integration has not been attempted, partly because it only applies, in the case of complete lubrication, when the value of $c > \cdot 5$ renders approximation very laborious, but chiefly because it appears almost obvious that the value of c, which renders the pressure negative at the off extremity of the brass is the largest value of c under which lubrication can be considered certain.

The journal may run with considerably higher values of c, the continuity of the film being maintained by the pressure of the atmosphere, which would be most likely to be the case with high speeds. But although the load which makes $c = \cdot 5$ is not necessarily the limit of carrying power of the journal, it would seem to be the limit of the safe working load, a conclusion which, as will appear on considering Mr Tower's experiments, seems to be in accordance with experience.

This concludes the hydro-mechanical theory of lubrication so far as it has been carried in this investigation. There remain, however, physical considerations as to the effect of variations of the speed and load on a and μ which have to be taken into account before applying the theory.

Section VIII.—The Effects of Heat and Elasticity.

30. *μ and a are only to be inferred from the experiments.*

The equations of the last section give directly the friction, the intensity of pressure, and the distance between the cylindrical surfaces, when the velocity, the radii of curvature of the journal and the brass, the length of the brass, and the manner of loading are known (*i.e.* when U, R, a, θ_1, L, and μ are known); and, further, if M the moment of friction is known, the equations afford the means of determining a when μ is known, or μ when a is known.

The quantities U, R, θ_1, and L are of a nature to be easily determined in any experiment or actual case, and M is easily measured in special experiments, but with a and μ it is different.

By no known means can the difference of radii (a) of the journal and its brass be determined to one ten-thousandth part of an inch, and this would be necessary in order to obtain a precise value of a. As a matter of fact even a rough measurement of a is impossible. To determine a, therefore, it is necessary to know the moment of friction or the distribution of pressure; then if the value of μ be known by experiments such as those described for olive oil (Section II.), a can be deduced from the equations for any particular value of μ. But although the values of μ may have been determined for all temperatures for the particular oil used, and that value chosen which corresponds with the temperature of the oil bath in the experiment, the question still arises whether the oil bath (or wherever the temperature is measured) is at the same temperature as the oil film. Considering the thinness of the film and the rapid conduction of heat by metal surfaces, it seemed at first sight reasonable to assume that there would be no great difference, but when on applying the equations to determine the value of a for one of the journals and brasses used in Mr Tower's experiments, it was found that the different experiments did not give the same values for a, and that the calculated values of a increased much faster with the velocity when the load was constant than with the load when the velocity was constant, it seemed probable that the temperature of the oil film must have varied in a manner unperceived, increasing with the velocity and diminishing the viscosity, which would account for an apparent increase of a.

That a should increase with the load was to be expected, considering that the materials of both journal and brass are elastic, and that the loads range up to as much as 600 lbs. per square inch, but there does not appear any reason why a should increase with the velocity unless there is an increase of temperature in the metal. If this occurs, the apparent increase of a would be partly real and partly due to the unappreciated diminution of μ owing to the rise of temperature.

Until some law of this variation of temperature and of the variation of a with the load is found, the results obtained from the equations, with values of μ corresponding to some measured temperature, such as that of the oil bath or a point in the brass, can only be considered as approximately applicable to actual results. Even so, however, the degree of approximation is not very wide as long as the conditions are such that the journal "runs cool."

But, treated so, the equations fail to show in a satisfactory way what is one of the most important matters connected with lubrication—the circumstances which limit the load which a journal will carry. For, although it may be assumed that the limit is reached when ca, the shortest distance between the surfaces, becomes zero or less than a certain value, yet, according to the equations, assuming a and μ to be constant, the value of c increases directly as U if the load be constant; so that the limiting load should increase with U. But this is not the case, for it seems from experiments that at a certain value of U the limiting load is a maximum if it does not diminish for a further increase of U.

Although, therefore, the close agreement of the calculated distribution of the pressure over the bearing with that observed and the approximate agreement of the calculated values of the friction for different speeds and loads, such as result when μ and a are considered constant, seem to afford sufficient verification of the theory, and hence a sufficient insight into the general action of lubrication, without entering into the difficult and somewhat conjectural subject of the effects of heat and elasticity, yet the possibility of obtaining definite evidence as to the circumstances which determine the limits to lubrication, which, not having been experimentally discovered, are a great desideratum in practice, seemed to render it worth while making an attempt to find the laws connecting the velocity and load with a and μ.

As neither the temperature of the oil film nor the interval between the surfaces can be measured, the only plan is to infer the law of the variations of these quantities for such complete series of experiments as Mr Tower's. In attempting this, a probable formula with arbitrary constants is first assumed or deduced from theoretical considerations, and then these constants are determined from the experiment and the general agreement tested. In order to determine the actual circumstances on which the constants depend, it is important to obtain the formula from theoretical considerations. This has therefore been done, although these considerations would not be sufficient to establish the formulæ without a close agreement with the experiments.

31. *The Effect of the Load and Velocity to alter the Value of the Difference of Radii of the Brass and Journal, i.e., of a.*

The effect of the load is owing to the elasticity of the materials, hence it is probable that the effect will be proportional to the load L. To express this put

$$a = a_0 + mL \quad\quad (109).$$

The effect of the temperature on a is owing to the different coefficients of expansion of brass and iron. Thus:—

$$a_T - a_0 = (B' - S')(T - T_0) R,$$

where B' and S' are the respective coefficients of expansion of the bearing and journal. These for brass and iron are :—

$$B' = {\cdot}0000111$$
$$S' = {\cdot}0000061,$$

therefore putting $T - T_0 = Tm$ (the mean rise of temperature due to friction)

$$a_T = a_0 \left(1 + {\cdot}000005 \frac{R}{a_0} Tm\right)$$
$$= a_0 (1 + ETm) \quad\quad (110),$$

putting E for ${\cdot}000005 \frac{R}{a_0}$.

If, as seems general, a_0 is about ·0005 inch, then with a 4-inch journal

$$E = {\cdot}02 \text{ about} \quad\quad (111),$$

which is sufficiently large to be important.

32. *The Effect of Speed on the Temperature.*

Putting—

T_0 the temperature of the surroundings and bath,

T_1 the mean temperature of the oil as it is carried out of the film,

T_m the mean rise in temperature of the film due to friction,

Q the volume of the oil carried through per second,

D the density,

S the specific heat,

H the heat generated,

H_1 the heat lost by conduction,

C' a coefficient of conduction,

taking the inch, lb., and degree Fahr., as units, and $12J$ as mechanical equivalent of heat.

$$Q = \frac{Uh_1}{2},$$

$$H = \frac{2R\theta fU}{12J},$$

$$H_1 = 2R\theta C' T_m,$$

$$T_1 - T_0 = \frac{H - H_0}{DSQ}.$$

Putting $$H - H_1 = qT_m Q \qquad \text{(112)}$$

where q is a constant depending on the relative values of $T - T_0$ and T_m, also on how far the metal of the journal assists the oil in carrying out heat.

On substituting the values of H_1, H_2, Q, it appears

$$T_m = \frac{fU}{\dfrac{3JDSqh_1U}{R\theta} + B} \qquad \text{(112 A)}$$

$$B = 12JC'.$$

There does not appear to be any reason to assume any of the quantities in the denominator to be functions of the temperature except h_1. By equation (42)

$$h_1 = a\{1 + c\sin(\phi_1 - \phi_0)\}.$$

Equation (112 A) may thus have the form

$$T_m = \frac{f}{A(1 + ET_m) + \dfrac{B}{U}} \qquad \text{(113)},$$

where $$A = 3JDSqa_0(1 + ml)\{1 + c\sin(\phi_1 - \phi_0)\} \qquad \text{(114)}.$$

This shows that A is a function of the load, increasing as the first power and diminishing as the second power, but the experiments show the effects of these terms are small, and A is constant except for extreme loads.

33. *The Formulæ for Temperature and Friction, and the Interpretation of the Constants.*

From equation (8), Section II.,

$$\left.\begin{aligned} \mu &= \mu_0^{-C(T-T_0)} \\ C &= \cdot 0221 \text{ (for olive oil)} \end{aligned}\right\} \quad \text{.....................(115).}$$

From equations (109) and (110)

$$a = (a_0 + ml)\{1 + E(T - T_0)\} \quad \text{.....................(116),}$$

or approximately since $E(T_1 - T_0)$ is small

$$a = (a_0 + ml)\, e^{E(T-T_0)} \quad \text{.........................(117).}$$

Whence substituting in the equation which results from (51) c being small

$$f = \frac{\mu}{a} U \quad \text{.................................(118).}$$

Putting T_x for any particular temperature, μ_x, a_x corresponding values

$$f = \frac{\mu_x}{a_x + ml} U e^{-(C+E)(T-T_x)} \quad \text{......................... (119).}$$

From (113)

$$f = A\{T_m + ET^2_m\} + \frac{B}{U} T_m \quad \text{........................(120).}$$

These equations (119) and (120) are independent, and therefore furnish a check upon each other when the constants are known.

Thus substituting the experimental values of U and f in (120) a series of values of T_m are obtained, which when substituted in (119) should give the same value of f.

In these equations the meaning of the constants is as follows:—

C is the rate at which viscosity increases with temperature.

E is the rate at which a increases with temperature, owing to the different expansion of brass and iron.

AU expresses generally the mechanical equivalent of the heat which is carried out of the oil film by the motion of the oil and journal for each degree rise of temperature in the film.

B expresses the mechanical equivalent of the heat conducted away through the brass and journal.

The respective importance of these two coefficients is easily apparent. When the velocities are low, but little heat will be carried out, and hence the temperature of the journal depends solely on the value of B. But when the velocities are high, B becomes insignificant compared with AU, and it is A alone which keeps the journal cool.

The value of A may to some extent be inferred from the quantities which enter into it as in equation (114). Thus in the case of Mr Tower's experiment, since

$$R = 2 \qquad \theta = 1{\cdot}37 \qquad a = {\cdot}00075 \qquad J = {\cdot}772$$

$$D = 0{\cdot}033 \qquad S = 0{\cdot}31 \text{ (for olive oil)}$$

$$A = {\cdot}0063q \text{..................................} (121).$$

It is very difficult to form an estimate of q, but it would seem probable that it has a value not far from 2; and, as will be subsequently shown, in the case of Mr Tower's experiments, q is about 3·5 or

$$A = 0{\cdot}0223 \text{ ....} (122).$$

As B expresses the rate at which the heat generated in the oil film is carried away, by conduction through the oil and the surrounding metal, any estimate of its value is very difficult. If we could measure the temperature at the surfaces of the metal, B might be made to depend only on the thickness and conductivity of the oil film. But before heat can escape from the journal or bearing, it must pass along intricate metal channels formed by the journal or shaft and its supports; and, on consideration, it appears that in ordinary cases the resistance of such channels would be much greater than the resistance of the oil film itself. For example, in the case of a railway axle, the heat generated must escape either along the journal to the nearest wheel, or through the brass and the cast-iron axle box to the outside surface, so that either way it must traverse at least three or four inches of iron. This is about the best arranged class of journals for cooling. In most other cases heat has much further to go before it can escape. However, in every case B will depend on the surrounding conditions, and can only be determined by experiment. From the experiments, to be considered in the next section, it appears that

$$B = 1 \text{ (about) ....} (123).$$

But it is to be noticed that Mr Tower has introduced a somewhat abnormal condition by heating the oil bath above the surrounding temperature. For in this way, letting alone the heat generated by friction, there must have been a continual flow of heat from the bath along the journal to the machinery; and, considering the comparatively limited surface of the journal in contact with the hot oil and the large area of section of the journal, it appears unlikely that the temperature of the journal was raised by the bath to anything like the full temperature of the latter, a conclusion which is borne out by Mr Tower's experiments with different temperatures in the bath (Table XII., page 291), which shows that the temperature of the bath produced a much smaller effect on the friction than would have followed from the known viscosity of oil had the temperature of the oil film corresponded with the temperature of the bath.

Thus the temperature of the film independent of friction is not the temperature of the bath or surrounding objects, and as it is unknown until determined from the experiments, it will be designated as

$$T_x,$$

and the suffix x used to designate the particular value for $T = T_x$ of all those quantities which depend on the temperature as

$$\mu_x,\ a_x.$$

If T be the mean temperature of the film,

$$T - T_x = T_m \quad \text{.............................(123 A)}$$

where T_m is the rise of temperature due to the film.

33 A. *The Maximum Load the Journal will carry at any Speed.*

It has already been pointed out that the carrying power of the journal is at its greatest when c is between ·5 and ·6. If, therefore, taking the load constant, c passes through a minimum value as the velocity increases with a constant load, then the load which brings c to a constant value will be a maximum for some particular velocity, and if the particular value of c be that at which the carrying power is greatest, the carrying power will be greatest at that particular speed.

The question whether, according to the theory, journals have a maximum carrying power at any particular speed turns on whether

$$\frac{dL}{dU}\ (c \text{ being constant})$$

is zero for any value of U.

This admits of an answer if the values for μ, a, T_x, and equations (119) and (120) hold, for when c is constant $\dfrac{L}{K_1 U}$, $\dfrac{f}{K_2 U}$, and A are constant, whence differentiating and substituting it appears that when c is constant

$$\frac{dL}{dU} = \frac{L}{U} \frac{A + \frac{B}{U} + \left\{AE - (3E + C)\frac{B}{U}\right\} T - AE^2T^2}{A + \frac{B}{U} + \left\{(3E + C)A + (E + C)\frac{B}{U}\right\} T + AE(E + C)T^2} \quad \text{...(123 B)}$$

rhere U is to be taken positive, and T increases as U increases. This shows ıat $\frac{dL}{dU}$, for a constant value of c, changes sign for some value of U if T ɔntinues to increase with U.

Hence, according to the theory, the values of L, which make c constant s U increases, approach a maximum value as U increases, and since this alue, when c is about ·5, represents the carrying power of the journal, this pproaches a maximum as U increases.

SECTION IX.—APPLICATION OF THE EQUATIONS TO MR TOWER'S EXPERIMENTS.

34. *References to Mr Tower's Reports.*

From the experiments described in Mr Tower's Reports I. and II., in the linutes of the Institution of Mechanical Engineers, 1884, the journal had diameter of 4 inches, and the chord of the arc covered by the brass was ·92 inches, the length of the brass being 6 inches.

The loads on the brass in lbs., divided by 24, are called the *nominal load er square inch.*

The moments of friction in inches and lbs., divided by 24 × R, are called he nominal friction per square inch.

These nominal loads, and nominal frictions, with the number of revoluions from 100 to 450, at which they were taken, are arranged in tables for each ind of oil used, and also for the same oil at different temperatures of the bath.

All the tables relative to the oil bath in the first report refer to the same rass and journal. And with this brass, to be here called No. 1, no definite ıeasurements of the actual pressure were made.

The second report contains the account of the pressure measurements, ut it is to be noticed that these were made with a new brass, here called lo. 2, and that the only friction measurements recorded with this brass are hree made at velocities five times less than the smallest velocity used in the ase of brass No. 1.

It thus happens that while by the application of the foregoing theory to he friction experiments on brass No. 1 a value is obtained for a, the ifference in radii of brass No. 1 and the journal; and from the pressure xperiments on brass No. 2 a value is obtained for a in the case of bras lo. 2; since these are different brasses there is no means c the o stimate against the other.

The following tables extracted from Mr Tower's reports are those to which reference has chiefly to be made.

The first of these extracts is a portion of Table I. in Mr Tower's first report; this related to olive oil, but corresponds very closely with the results for lard oil also, although not quite so close for mineral oil.

From TABLE I.—(Mr Tower's 1st Report, Brass No. 1.) Bath of Olive Oil. Temperature, 90 deg. Fahr., 4-in. journal, 6-in. long. Chord of arc of contact = 3·92 in.

Nominal load lbs. per square inch. Total load divided by 4 × 6.	Nominal friction per square inch of bearing.							
	100 rev. 105 ft. per min.	150 rev. 157 ft. per min.	200 rev. 209 ft. per min.	250 rev. 262 ft. per min.	300 rev. 314 ft. per min.	350 rev. 360 ft. per min.	400 rev. 419 ft. per min.	450 rev. 471 ft. per min.
520	...	·416	·520	·624	·675	·728	·779	·883
468	...	·514	·607	·654	·701	·794	·841	·935
415	...	·498	·580	·622	·705	·787	·870	·995
363	...	·472	·580	·616	·689	·725	·798	·907
310	...	·464	·526	·588	·650	·680	·742	·835
258	·361	·438	·515	·592	·644	·669	·747	·798
205	·368	·43	·512	·572	·613	·675	·736	·818
153	·351	·458	·535	·611	·672	·718	·764	·871
100	·36	·45	·555	·63	·69	·77	·82	·89

The second extract is Table IX. in the first report; this shows the effect of temperature on the friction of the journal with lard oil.

From Mr Tower's 1st Report, Brass No. 1.

TABLE IX.—Bath of Lard Oil. Variation of Friction with Temperature. Nominal Load 100 lbs. per square inch.

Temperature. Fahr.	Nominal friction per square inch of bearing.							
	100 rev. per min.	150 rev. per min.	200 rev. per min.	250 rev. per min.	300 rev. per min.	350 rev. per min.	400 rev. per min.	450 rev. per min.
120°	·24	·29	·35	·40	·44	·47	·51	·54
110	·26	·32	·39	·44	·50	·55	·59	·64
100	·29	·37	·45	·51	·58	·65	·71	·77
90	·34	·43	·52	·60	·69	·77	·85	·93
80	·4	·52	·63	·73	·83	·93	1·02	1·12
70	·48	·65	·8	·92	1·03	1·15	1·24	1·33
60	·59	·84	1·03	1·19	1·30	1·4	1·48	1·56

The third extract is from the second report, being Table XII., representing the oil pressure at different parts of the bearing as measured with brass No. 2.

From Mr Tower's Second Report, Brass No. 2. Heavy Mineral Oil. Nominal Load 333 lbs. per square inch. Number of revolutions 150 per minute. Temperature, 90°.

TABLE XII.—Oil pressure at different points of a bearing.

Longitudinal planes............on	centre	off
Pressure per square inch......lb.	lb.	lb.
Transverse plane, middle......370	625	500
Transverse plane, No. 1 355	615	485
Transverse plane, No. 2 310	565	430

The points at which the pressure was measured were at the intersections of six planes, three parallel vertical longitudinal planes parallel to the axis of the journal, one through the axis, and the other two, one on the *on* and the other on the *off* side, both of them at a distance of ·975 inch from that through the axis, three transverse planes, one in the middle of the journal, the other two respectively at a distance of one and two inches on the same side of that through the middle.

In referring to these experimental results in the subsequent articles,

The nominal load per square inch is expressed by L';

The number of revolutions per minute by N';

The nominal friction by f';

The effect of the fall of pressure at the ends of the journal on the mean pressure is expressed by $\frac{1}{s}$, thus—

n is a coefficient depending on the way in which the journal fits the shaft.

$$\left.\begin{aligned} L' &= \frac{L}{4s} \\ f' &= \frac{nM}{4R} \\ N' &= -\frac{U \times 60}{2\pi R} \\ s &= 1{\cdot}21 \end{aligned}\right\} \qquad (124).$$

35. *The Effect of Necking the Journal.*

The expression (124) for f' assumes that the journal was not necked into the shaft. From Mr Tower's reports it does not appear whether or not the brass was fitted into a neck on the shaft; but since there is no mention of such necking, the theory is applied on the supposition that there was not.

If there were, the friction at the ends of the brass would increase the moment of friction. Put b for the depth of the neck and a' for the thickness of the oil film at the ends, then the moment of resistance of these ends would be—

$$\frac{\mu}{a'} \times \frac{N\pi}{30}\left(R+\frac{b}{2}\right)^3 b.$$

Hence if M be the moment of friction of the cylindrical portion of the journal only

$$24f' - 2\frac{\mu}{a}\frac{\pi N}{30}6\left(R+\frac{b}{2}\right)^3 = 6\frac{M}{R} \quad \text{..................(125).}$$

And from equation (97)—

$$f' = \frac{\pi N}{360}\frac{\mu}{a}\left\{\frac{ab\left(R+\frac{b}{2}\right)^3}{a'R} + 8{\cdot}25R^2\right\} \quad \text{..............(126).}$$

For example, a 5-inch shaft necked down to a 4-inch journal would give $b = {\cdot}5$ inch. Whence, assuming

$$\left.\begin{aligned} a' &= {\cdot}0005 \\ \text{and} \qquad \frac{a}{a'} &= 2 \end{aligned}\right\} \quad \text{..............................(127),}$$

the relative friction of the ends to that of the journal would be 11·36 to 31·00, or 28 per cent. of the friction; and the values of a, calculated on the assumption of no necking, would have to be increased in the ratio $n = 1{\cdot}33$.

Even if there is no necking the value of a will probably not be the same all along the journal, in which case the values of a^2 and a in K_1 and K_2 will be means, and then the square of the mean will be less than the mean of the squares, so that n will probably have a value greater than unity, although there may be no necking of the shaft.

36. *A first Approximation to the Difference in the Radii of the Journal and Brass No.* 1.

The recorded temperature in Mr Tower's Table I. is 90° Fahr. Accepting this, and taking the value of μ, equation (8), Section II.,

$$\mu_{90} = 10^{-6} \times 6{\cdot}81 \quad \text{..............................(128).}$$

By equation (97)—

$$\frac{n\mu}{a} = \frac{-nM}{2{\cdot}75R^2U} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(129).$$

Since $\qquad R = 2$ and $U = -\frac{4\pi N}{60}$,

$$\frac{n\mu}{a} = -\frac{{\cdot}72f''}{U}$$

$$= 3{\cdot}46\frac{f'}{N} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(130).$$

Whence substituting from equation (128) for μ_{90}

$$\frac{a}{n} = 10^{-6} \times 1{\cdot}97\frac{N}{f'} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(131),$$

and from the tabular Nos. for $L' = 100$.

$$\left.\begin{aligned} N &= 100, \quad f' = {\cdot}36 \\ \frac{a}{n} &= 10^{-4} \times 5{\cdot}5 \text{ (inch)} \\ N &= 480, \quad f' = {\cdot}89 \\ \frac{a}{n} &= 10^{-4} \times 10 \text{ (inch)} \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots(132).$$

These are the extreme cases; for intermediate velocities intermediate values of $\frac{a}{n}$ are found.

In order to be sure that these are the values of $\frac{a}{n}$, which result from the application of the equations, it is necessary (since the approximate equations only have been used) to see that the squares of c may be neglected.

Substituting from equation (124) in (96)

$$c = {\cdot}388na\frac{L's}{f'R}$$

which for $L' = 100$, $N = 100$, gives

$$c = {\cdot}033n^2 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(133).$$

and for $L' = 100$, $N = 450$,

$$c = {\cdot}065n^2$$

So that the approximations hold, and, as already stated in Art. 30, this considerable increase in the value of a with the load constant suggests that the temperature of the film was not really 90°. And as this point has been considered in the last section, the equations of that section may be at once used to determine the law of this temperature, after which the values of $\frac{a}{n}$ may be determined with precision.

37. *The Rise in Temperature of the Film owing to Friction.*

In order to determine the values of E, A, and B in equations (119) and (120), by substituting in these equations corresponding values of N and f' for $L' = 100$, the tabular values of f' were somewhat rectified by plotting and drawing the curve N, f'. These corrected values are in the second row, Table III.

From these values, and the corresponding values of N, it was then found by trial that the equations (119) and (120) respectively

$$f = \frac{\mu_x}{a_x + mL} Ue^{-(C+E)(T-T_x)},$$

$$f = A\{T_m + ET_m^2\} + \frac{B}{U}(T_m),$$

$$c = {\cdot}0221,$$

are approximately satisfied for values of $T - T_x = T_m$, if

$$\frac{\mu_x}{a_x + m \times 400} = \frac{{\cdot}01345}{n} \quad \text{.........................(134)}$$

$$\left.\begin{aligned} A &= {\cdot}022308 \\ E &= {\cdot}0222 \\ B &= {\cdot}95914 \end{aligned}\right\} \quad \text{......................(135).}$$

TABLE III.—Rise of Temperature in the Film of Oil caused by Friction, calculated by Equation (120) from Experiments with a Nominal Load 100 lbs. (see Table I., Tower, p. 290).

Nominal load per square inch $L' = 100$ lbs.		Nominal friction per square inch, as calculated by equation (119) from the rise of temperature.								
	N	Revolutions per minute . .	100	150	200	250	300	350	400	450
	f'	Nominal friction per square inch for olive oil: Table I., Tower .	·36	·45	·54	·63	·69	·77	·82	·89
		Nominal friction per square inch for olive oil: Corrected to a curve	·33	·55	·55	·63	·705	·768	·83	·89
	$T - T_0$ Fahr.	Rise of temperature by equation (120)	3·45	5·83	8·13	10·02	11·77	13·26	14·48	15·37
	f'_1	Nominal friction per square inch calculated by equation (119) assuming c small. .	·336	·453	·546	·628	·697	·76	·823	·89

Which seemed to agree very well with the reasoning in Section VIII. With these values of the constants the values of $T - T_x$ were then calculated from equation (120), and are given in the third row in Table III. These temperatures were then substituted in equation (119), and the corresponding values of f' calculated, these are given in the fourth row, Table III.

The agreement between these calculated values of f' and the experimental values is very close; and it may be noticed that a very small variation in any of A, B and E makes a comparatively large difference in some one or other of the calculated values of f', or, in other words, these are the only positive values of these quantities which satisfy the equations.

The only difference between the experimental and calculated values of f which is not explainable as experimental error, is for the lowest speed at which the experimental value of f' is 6·7 per cent. too large. This is important as it is in accordance with what might be the result of neglecting c^2, since at that speed c is becoming too large to be neglected, and taking c^2 into account the calculated value of f agrees very closely with the experimental.

38. *The Actual Temperature of the Film.*

Having found the approximate values of T_m, the rise of temperature, owing to friction, it remains to find T_x, the temperature of the film, the rise due to friction, so that

$$T_x + T_m = \text{temperature of film.}$$

This is found from Mr Tower's Table IX. (see p. 290).

Putting T_B for temperature of the bath,

T_0 for temperature of surrounding objects,

and assuming

$$T_x + T_m = Z(T_B - T_0) + T_m + T_0 \quad\text{.................(136).}$$

From equations (119) and (130)

$$f' = \frac{N}{3{\cdot}46} \cdot \frac{n\mu_0}{a_0 + mL} e^{-{\cdot}0443\{Z(T_B - T_0) + T_m\}}$$

whence

$$\log f' = -{\cdot}0443\{Z(T_B - T_0) + T_m\}\log e + \log\left(\frac{N}{3{\cdot}46}\frac{\mu_0}{a_0 + mL}\right) \quad\text{...(137).}$$

In Table IX. (Tower) the values of f' are given for the same values of N' and L' corresponding to different values of T_B.

Substituting corresponding values of f' and T_B in equation (137), and subtracting the resulting equations, we have an equation in which the only unknown quantities are Z and the differences of T_m.

The values of f' being known, the values of T_m are obtained from equations (120), (134), and (135), and substituting these, the equations resulting from (137) give the values of Z. Thus from Table IX. (Tower)

$$L' = 100$$
$$U = 100$$

$$T_B = 60 \qquad f = \cdot 59 \qquad T_m = 5\cdot 9$$
$$T_B = 70 \qquad f = \cdot 48 \qquad T_m = 4\cdot 8.$$

From (136) $$Z + \frac{5\cdot 9 - 4\cdot 8}{10} = \frac{\log \cdot 59 - \log \cdot 48}{\cdot 0443 \times 10 \times \log e}.$$

Therefore $$Z = 35 \quad \text{.................................(138).}$$

From this value of Z the values of f' corresponding to those in Tower's Table IX. have been calculated and agree well with the experimental values.

The smallest temperature of the oil bath recorded in Tower's Table IX. is 60° Fahr., therefore it is assumed that this was the normal temperature, whence

$$T_x = \cdot 35\,(T_B - 60) + 60 \quad \text{........................(139).}$$

Hence it is concluded that the actual temperature of the oil film in all the experiments with the bath, at a temperature of 90° Fahr., is given by

$$T = 70\cdot 5 + T_m \quad \text{..............................(140).}$$

By the formula for μ since $T_x = 70\cdot 5$

$$\mu_x = \cdot 00004737 e^{-\cdot 0021 \times 70\cdot 5}$$
$$= \cdot 000009974$$
$$= \cdot 00001 \text{ (approximately)} \quad \text{..................(141),}$$

and since, by equation (134), when $L' = 100$

$$a_x = \frac{n\mu_x}{\cdot 01345}$$
$$= \cdot 0007413 n$$
$$= \cdot 00074 n \text{ (approximately)} \quad \text{.........(142).}$$

This is the value of a_x with a load of 100 lbs. per square inch.

39. *The Variation of a with the Load.*

All Mr Tower's experiments, when the loads are moderate and the elocities high, show a diminution of resistance with an increased load.

Since c increases with the load and the friction increases as c increases, and μ being constant, the diminution of friction with increased loads shows ither that the load increases the temperature of the film and so diminishes he viscosity, or increases the radius of curvature of the brass as compared ith that of the journal, *i.e.*, increases a.

These effects have been investigated by substituting the experimental alues of f' and L', obtained with the same velocity in equations (119) and 120).

In this way, from equation (119) the value of m is determined, where, rom equations (117) and (135),

$$a_x = (a_0 + mL')\, ne^{\cdot 0222\,(T_x - T_0)} \dots\dots\dots\dots (143).$$

nd the equation (120) gives the effects of the load on the value of the onstant A. After trial, however, it appears that the effects of the load pon the constant A are small so long as the loads are moderate, and that he diminution of the resistance with the increased load is explained by the alue obtained for m from equation (119). From this equation, taking $_1'f'^1$, $L_2'f'^2$, simultaneous values of L' and f', and assuming T_x independent f the load,

$$\frac{a_0 + mL_1'}{a_0 + mL_2'} = \frac{f'^2}{f'^1} \dots\dots\dots\dots (144)$$

hich gives the value of m.

The slight irregularities in the experiments affect the values of m thus ound to a considerable extent, and a mean has been taken, which is

$$m = \cdot 002 a_0 \dots\dots\dots\dots (145).$$

utting $T_x = 70\cdot 5$, $T_0 = 60$, $a_x = \cdot 00074$, when $L' = 100$, from equation (143)

$$a_0 + mL' = \cdot 0005861 n.$$

Therefore $$a_0 = \cdot 0004885 n \dots\dots\dots\dots (146).$$

The value for a thus obtained is therefore

$$a = \cdot 0004885 n\,(1 + \cdot 002\,L')\, e^{\cdot 0222\,(T_x + T_m - T_0)} \dots\dots\dots\dots (147),$$

nd for the experiments with the bath at 90° Fahr.

$$a = \cdot 0004885 n\,(1 + \cdot 002 L')\, e^{\cdot 0222\,(T_m + 10\cdot 5)}$$

r $$a = \cdot 0006161 n\,(1 + \cdot 002 L')\, e^{\cdot 0222 T_m} \dots\dots\dots\dots (148).$$

From equation (148) and the values of T_m, Table III., the values of

$$\frac{a}{n}$$

for Mr Tower's experiments have been calculated.

Putting $$\mu = {\cdot}00001 e^{-{\cdot}0221 T_m} \quad \text{.........................(149)},$$

and, substituting in equations (47) from (148) and (149),

$$\left.\begin{aligned} -K_1 &= \frac{66{\cdot}08 N e^{-{\cdot}0663 T_m}}{(1+{\cdot}002L')^2 n^2} \\ -K_2 &= \frac{{\cdot}0034 N e^{-{\cdot}0443 T_m}}{(1+{\cdot}002L') n} \\ \frac{K_2}{K_1} &= {\cdot}00005139\,(1+{\cdot}002L')\, e^{{\cdot}0221 T_m} \end{aligned}\right\} \quad \text{...............(150)},$$

for the circumstances of Mr Tower's experiments, to which the equations of Sections VI. and VII. then become applicable.

40. *Application of the Equations to the Circumstances of Mr Tower's Experiments on Brass No. 1, given in Table I.,* p. 290, *to determine* c, ϕ_0, ϕ_1, f' *and* $p - p_0$.

The circumstances are, *the unit of length being the inch,*

$$\left.\begin{aligned} &R = 2 \text{ (inches)} \\ &\theta = 78^\circ\, 31'\, 20'' \\ &\frac{a_0}{n} = {\cdot}0004885, \text{ already deduced equation (146)} \\ &\left.\begin{aligned} L &= 484L' \\ U &= {\cdot}2094 \end{aligned}\right\} \text{equations (124)} \\ &T_0 = 60, \text{ assumed} \\ &T_x = 70{\cdot}5, \text{ equation (140)} \\ &T_m = \text{tabular values, Table III., the increase with load being neglected} \end{aligned}\right\}$$

$$\text{.........................(151)}.$$

For a first Approximation as long as c *is small.*

Equations (89), (91), (94), and (95) are used to determine c, ϕ_0, ϕ, f_1' for the experiments in Table I. (Tower), these being made with brass No. 1.

Putting as in equation (124)

$$f' = \frac{nM}{2R^2},$$

equation (94) gives

$$f_1' = -1{\cdot}37 K_2 n,$$

and by equation (150)

$$f_1' = {\cdot}004658 \frac{N e^{-{\cdot}0443 T_m}}{1 + {\cdot}002 L'} \quad\text{.......................(152).}$$

From equation (152) the values of f' to a first approximation have been calculated, using the values of T_m given in Table III. These are given as f_1' in Table IV.

TABLE IV.—Olive Oil, Brass No. 1.

Length of the journal ... ...6 inches
Chord of the arc of contact of the brass ... .3·92 inches.
Radius of the journal ... ...2 inches.
Temperature of the oil bath ... ...90° Fahr.
,, surrounding objects... ... ...60° Fahr. (assumed).
Difference in radii of brass and journal at 60° ... ...0·0006 inch (deduced).
Effect of necking or variations in radius to increase friction ...1·25.

L' the nominal load in lbs. per square inch, being the total load divided by 24.
N the number of revolutions per minute.
f' the nominal friction in lbs. per square inch from Table I. in Mr Tower's first Report (see Art. 34, p. 290).
(f_1') the nominal friction calculated by complete approximation for $c=5$ (see Art. 40, equation (159)).
f_2' the nominal friction calculated to a second approximation, equation (154).
f_1' the nominal friction calculated to a first approximation, equation (152).
c the ratio of the distance between the centres of the brass and journal to the difference in the radii, equation (153).
a the difference in the radii of the brass and journal (see equation (157)).
ϕ_1 the angular distance from the middle of the arc of contact of the point of maximum pressure, equation (91).
$\phi_0 - \frac{\pi}{2}$ the angular distance from the middle of the arc of contact of the point of nearest approach (see equation (89)).
T_m the rise in temperature of the film of oil owing to the friction, equation (120).

	N.	100	150	200	250	300	350	400	450
	Fahr.	3·45	5·83	8·13	10·02	11·77	13·26	14·46	15·37
·5	f'	...	·498	·580	·622	·705	·787	·870	·995
	(f_1')	...	...	(·57)	(·65)	...	...	...	f_2' ·108
	f_1'	...	·300	·360	·414	·460	·504	·544	·589
	c	...	·67	·578	·520	·487	·457	·436	·413
	a	·00154	·00162	·0017	·00178	·00182	·00187	·00191	·00194
	ϕ_1	...	...	− 7° 0′ 0″	− 7° 0′ 0″	...	...	...	...
	$\phi_1 - \frac{\pi}{2}$	...	...	− 42° 0′ 0″	− 42° 0′ 0″	...	...	...	...

TABLE IV.—Olive Oil, Brass No. 1—continued.

	N.	100	150	200	250	300	350	400
	T_m/Pdm	3·45	5·83	8·13	10·02	11·77	13·26	14·46
$E' = 358$	f	...	·472	·580	·616	·689	·725	·798
	f_2'	...	(·498)	...	...	...	...	...
	f_1'	·233	·314	·378	·434	·482	·526	·573
	c	...	·520	·462	·408	·380	·357	·340
	a	...	·00151	·00161	·00168	·00171	·00177	·00181
	ϕ_1	...	$-$ 7° 0′ 0″	...	...	...	...	...
	$\phi_0 - \frac{\pi}{2}$	...	$-$ 42° 0′ 0″	...	...	...	...	...
$E' = 310$	f	...	·464	·526	·588	·650	·684	·742
	f_2'	(·370)	...	...	...	...	f_2' ·765	·805
	f_1'	·249	·336	·404	·464	·517	·582	·610
	c	·510	·392	·337	·305	·284	·266	·254
	a	·00138	·00144	·00151	·00158	·00161	·00166	·00172
	ϕ_1	$-$ 7° 0′ 0″	...	...	...	...	...	...
	$\phi_0 - \frac{\pi}{2}$	$-$ 42° 0′ 0″	...	...	...	...	...	...
$E' = 253$	f	·361	·436	·514	·592	·644	·669	·747
	f_2'	...	...	...	·62	·670	·712	·760
	f_1'	·266	·358	·431	·495	·550	·600	·650
	c	·377	·287	·242	·224	·200	·195	·180
	a	·00128	·00135	·00141	·00148	·00151	·00156	·00159
	ϕ_1	...	...	...	...	...	...	...
	$\phi_0 - \frac{\pi}{2}$	...	...	...	...	...	...	...
$E' = 205$	f	·368	·430	·512	·572	613	·675	·736
	f_2'	·380	·457	·530	·595	·65	·701	·755
	f_1'	·285	·385	·464	·534	·592	·646	·700
	c	·255	·195	·165	·152	·141	·132	·126
	a	·00119	·00126	·00131	·00138	·00240	·00145	·00148
	ϕ_1	...	...	...	...	...	...	$-$ 1° 13′ 0″
	$\phi_0 - \frac{\pi}{2}$	...	...	...	...	...	...	$-$ 60° 0′ 0″
$E' = 152$	f	·351	·458	·535	·611	·672	·718	·764
	f_2'	·352	·458	·530	·601	·665	·717	·778
	f_1'	·307	·414	·498	·574	·638	·695	·753
	c	·165	·126	·107	·098	·089	·086	·082
	a	·00111	·00116	·00122	·00128	·00130	·00134	·00137
	ϕ_1	...	$-$ 1° 13′ 0″	$-$ 1° 0′ 0″	$-$ 0° 57′ 0″	$-$ 0° 50′ 0″	$-$ 0° 44′ 0″	$-$ 0° 38′ 0″
	$\phi_1 - \frac{\pi}{2}$	...	$-$ 61° 0′ 0″	$-$ 65° 0′ 0″	$-$ 27° 20′ 0″	$-$ 69° 0′ 0″	$-$ 69° 40′ 0″	$-$ 70° 20′ 0″
$E' = 100$	f	·360	·450	·550	·630	·690	·770	·820
	f_2'	·352	·465	·555	·637	·708	·770	·831
	f_1'	·336	·463	·546	·628	·697	·760	·823
	c	·090	·0691	·0600	·0541	·0492	·0471	·0460
	a	·00101	·00106	·00112	·00116	·00120	·00123	·00125
	ϕ_1	$-$ 0° 43′ 0″	$-$ 0° 26′ 0″	$-$ 0° 17′ 0″	$-$ 0° 10′ 0″	$-$ 0° 4′ 0″	$-$ 0° 2′ 30″	$-$ 0° 0′ 0′
	$\phi_0 - \frac{\pi}{2}$	$-$ 68° 30′ 0″	$-$ 73° 20′ 0″	$-$ 75° 20′ 0″	$-$ 76° 30′ 0″	$-$ 77° 20′ 0″	$-$ 77° 40′ 0″	$-$ 0° 78′ 0′

As compared with the experimental values f' given in the Table IV., it is seen that the agreement holds as long as c is less than ·06, after which, as c increases, the values of f_1' become too small, or while the values of f_1' continue to diminish as the load and a increase, the experimental values of f' after diminishing till c is about ·1 or ·15 begin to increase again. In order to see how far this law of variation was explained by the theory, it was necessary to find f_2' the values of f' to a second approximation, and before this to obtain the values of c.

Putting, as in equation (124),

$$L = 4{\cdot}84L',$$

equation (94) gives

$$c = -\,2{\cdot}059\,\frac{L'}{K_1};$$

and by equation (150)

$$c = {\cdot}03116\,(1 + {\cdot}1002L')^2\,\frac{n^2L'}{N}\,e^{{\cdot}0665T_m} \quad\ldots\ldots\ldots\ldots\ldots(153).$$

Equation (153) gives the values of

$$\frac{c}{n^2}.$$

To obtain the value of n from the experiments, these values of $\frac{c}{n^2}$ are substituted in the equation for f', retaining the squares of c, which obtained from equations (85) and (89), is

$$f_2' = f_1'\,(1 + 5c^2)\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(154),$$

whence, substituting the values of $\frac{c}{n^2}$ obtained from (153) we have

$$f_2' = f_1' + 5\left(\frac{c}{n^2}\right)^2 n^4 f_1'.$$

Therefore, choosing any experimental values of f', and subtracting the corresponding value of f_1' in Table IV., n is given by—

$$n^4 = \frac{f' - f_1'}{5\left(\frac{c}{n^2}\right)^2} \quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(155).$$

In this experiment irregularities become important, and it has been necessary to calculate several values of n in this way and take the mean, which is

$$n = 1{\cdot}25\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(156).$$

It has been shown (Art. 35, p. 292) that necking might account for a value of n as great as 1·33, while if there were no necking n would still have a value in consequence of variations of a along the journal.

Substituting this value of n in equations (148) and (153),

$$\left.\begin{aligned} a &= \cdot 00077\,(1+\cdot 002\,L')\,e^{\cdot 0222 T_m} \\ c &= \cdot 0487\,(1+\cdot 002\,L')^2\,\frac{L'}{N}\,e^{\cdot 0665 T_m} \end{aligned}\right\} \qquad (157),$$

from which equation the values of a and c have been calculated for Table IV. all values of L' less than 415 lbs. These are all Mr Tower's experiments olive oil, except those of which Mr Tower has expressed himself doubtful as to the results.

The values of c as given by equation (94) are only a first approximation, and are too large, but the error is not large, even when $c = \cdot 5$ only amounting to 8 per cent., as is shown by comparing equation (104) with (95).

With these values of c in the equation (154) the values of f_2' have been calculated for all values of c up to ·250. At $c = \cdot 12$ these values of f_2' are about 5 per cent. larger than the experimental values, but they have been carried to $c = \cdot 25$ in order to show that the calculated friction follows in its variations the idiosyncracies of the experimental frictions, falling with the load to a certain minimum, and then rising again.

These values of f_2' carry the comparison of the frictions deduced from the theory up to loads of 205 lbs. for all velocities, and up to 363 lbs. for the highest velocity. To carry the approximation further, use has been made of the more complete integrations of the equations for the case of

$$c = \cdot 5.$$

These are given by equations (104) and (105).

As already stated, comparing (104) with (94) it appears that when $c = \cdot 5$ the approximate values of c in the Table IV. are about 8 per cent. too large; that is to say, a value $c = \cdot 540$ in the table would show that the actual value was $c = \cdot 5$.

Comparing equation (95), from which the values f_1' have been calculated, with equation (105), it appears that when $c = \cdot 5$ the values of f' by (105) are given by

$$f' = \frac{2\cdot 3773}{1\cdot 37} f_1'.$$

This is not, however, quite satisfactory, as that portion of the friction which is due to necking does not increase with the load. This portion in f_1' is

$$\frac{n-1}{n} f_1'.$$

So that for $c = \cdot 5$

$$f' = \frac{2\cdot 3773 + 1\cdot 37\,(n-1)}{1\cdot 37n} f \quad\text{.................(158)},$$

and since $n = 1\cdot 25$, this gives for $c = \cdot 5$

$$f' = 1\cdot 585 f_1'.$$

If, therefore, any of the approximate values of c were exactly ·540, the complete value of f' would be 1·585 times the value of f_1'. This does not happen, the nearest approximate values of c being ·578, ·520, ·520, ·510. Multiplying the corresponding values of f_1' by 1·585, the results are as follows :—

Tabular. c	Tabular. f_1'	$1\cdot 505 f_1'$	Experimental. f	Difference.
·578	·36	·57	·58	·01
·520	·414	·656	·65	− ·005
·520	·314	·498	·472	− ·026
·510	·249	·394		

It thus appears that the approximation is very close, the calculated values for the first, in which c is greater than ·540, being too small, and for the rest, in which c was smaller than ·540, too large, which is exactly what was to be expected.

These corrected values of f_1' have been introduced in Table IV. in brackets. As they occur with different loads and different velocities, they afford a very severe test of the correctness of the conclusions arrived at as to the variations of A and T with the load and temperature, also as to the condition expressed by n. Had the values of c and f' been completely calculated as for the case of $c = \cdot 5$, there would have been close agreement for all the calculated and experimental values of f'.

This close agreement strongly implies, what was hardly to be expected, namely, that the surfaces, in altering their form under increasing loads, preserve their circular shape so exactly that the thickness of the oil film is everywhere approximately

$$a\,(1 + c\sin(\theta - \phi)).$$

A still more severe test of this is, however, furnished by the pressure experiments with brass No. 2 in Mr Tower's second report.

41. *The Velocity of Maximum Carrying Power.*

The limits to the carrying powers are not very clearly brought out in these recorded experiments of Mr Tower, as indeed it was impossible they should be, as each time the limit is reached the brass and journal require refitting. But it appears from Table I. and all the similar tables with the oil bath in Mr Tower's reports, that the limit was not reached in any case in which the load and velocity were such as to make c less than ·5. In many cases they were such as to make c considerably greater than this, but in such cases there seems to have been occasional seizing. There seems, however, to have been one exception to this case, in which the journal was run at 20 revolutions per minute with a nominal load of 443 lbs. per square inch with brass No. 2 without seizi[illegible]h case c, as determined either by the friction or load, becomes nearly ·9.

It does not appear that any case is mentioned of seizing having occurred at high speeds, so that the experiments show no evidence of a maximum carrying power at a particular velocity.

This is so far in accordance with the conclusions of Art. (33a), for, substituting the values of $ABCE$, as determined Art. (37), it appears by equation (123 B) that the maximum would not be reached until T_m, the rise of temperature due to friction, reached 72° Fahr., which, seeing that at a velocity of 450 revolutions T_m is less than 17°, implies that the maximum carrying power would not be reached until the speed was 1500 or 2000 revolutions; notwithstanding that $\frac{dL}{dU}$ (c constant) is very small at 450 revolutions.

This is with the rise of temperature due to legitimate friction with perfect lubrication. But if, owing to inequalities of the surfaces, there is excessive friction without corresponding carrying power, *i.e.*, if n, the effect of necking, is as large as 3 or 4, which it is with new brasses, then the maximum carrying power might be reached at comparatively small velocities; thus suppose $T = 13$ when $N = 100$, $U = 21$, equation (123 B) gives

$$\frac{dL}{dU} = 0,$$

or the maximum carrying power would be reached; all which seems to be in strict accordance with experience, particularly with new brasses.

42. *Application of the Equations to Mr Tower's Experiments with Brass No. 2 to determine the Oil Pressure round the Journal.*

The approximate equation (74) is available to determine the pressure at any part of the journal, *i.e.*, for any value of θ so long as c is small, but these approximations fail for much smaller values of c than for others; for this reason, together with the fact that the only case in which the pressure has been measured c is large, the pressures have only been calculated for $c = \cdot 5$, in which the approximations have been carried to the extreme extent.

These are obtained directly from equation (107), and the pressures divided by $K_1 c$ are given in Table II., Section VI.

The results of Mr Tower's experiments with brass No. 2 are given in Table XII., Art. 34.

Had the friction been recorded in the experiments in which Mr Tower measured the pressures with brass No. 2 as with brass No. 1, the values of c might have been obtained as in the case of brass No. 1. But as this was not done the value of c for these experiments with brass No. 2 could only be inferred from the agreement of the relative oil pressures measured in different parts of the journal, those calculated for the same parts with a particular value of c. This was a matter of trial, and as it was found that the agreement was very close when

$$c = \cdot 5,$$

further attempts were not made.

With the section at the middle of the brass the calculated and experimental results are shown in Table V.

TABLE V.—Comparison of Relative Pressures, calculated by Equation (107) when $c = \cdot 5$, with the Pressures measured by Mr Tower, see Table XII., Art. 34, Brass No. 2.

The values of θ measured from middle of arc at which pressures were measured	Pressure measured at the middle of the journal. Table XII., Tower	$\frac{p - p_0}{-K_1 c}$ calculated. Table II.	Relative values, experimental	Relative values, calculated	$-K_1 c$
$-20^\circ\ 20'\ 20''$	500	·7923	·800	·781	639
0 0 0	625	1·0150	1·000	1·000	615
29 20 20	370	·6609	·592	·651	560

This agreement, although not exact, is, considering the nature of the test, very close. The divergence seems to show that in the experiments c was somewhat more than ·5, but it is doubtful if the agreement would have been exact, as, owing to the journal having been run in one direction only, it seems probable that the radius of the brass was probably slightly greatest on the *on* side.

Deducing the value of K_1c by dividing the experimental pressure by the calculated values of $\frac{p-p_0}{K_1c}$ the values given in the last column are found. An alteration in the value of c would but slightly have altered the middle value of $\frac{p-p_0}{Kc}$ in the same direction as the alteration of c; hence taking this value, and making $c = \cdot 520$, as being nearer the real value,

$$K_1c = -640 \quad\ldots\ldots(159).$$

In these experiments

$$N = 150,$$

$$L' = 333 \quad\ldots\ldots(160).$$

From equation (104)

$$L = -2\cdot 5504 \times Kc,$$

$$\frac{L}{4} = 408 \quad\ldots\ldots(161),$$

therefore

$$s = \frac{L}{4L'}$$

$$= 1\cdot 21 \quad\ldots\ldots(162).$$

To find a

$$K_1 = -1230,$$

by equation (150)

$$= \frac{66\cdot 08e^{-\cdot 0665T_m}}{n^2(1+002L)^2} N,$$

whence

$$a_x^2 = \cdot 000001088e^{-\cdot 0665T_m} \quad\ldots\ldots(163)$$

and taking T_m the same as with brass No. 1, and olive oil at $N = 180$, *i.e.*, 5·83° Fahr., with brass No. 2, at 70·5° Fahr.

instead of with brass No. 1

$$\left.\begin{aligned} a_x &= \cdot 00086 \\ a_n &= \cdot 00077 \end{aligned}\right\} \quad\ldots\ldots(164).$$

The difference in the radii of curvature of the two brasses, the one deduced from the measured friction, the other deduced from the measured differences of pressure at different positions round the journal, come out equal within $\frac{1}{10000}$th part of an inch, and the values of a differing only by 11 per cent. Had the frictions been given with brass No. 2, this agreement would have afforded an independent comparison of the values of a. As it is, the only probability of equality in these two brasses arises from the probability of their having been bedded in the same way.

In deducing the value a for brass No. 2, it has been assumed that the oil, which was mineral, had the same law of viscosity as the olive oil. Both these oils were used with brass No. 1, and the results are nearly the same, the mean resistances, as given by Mr Tower, are as 0·623 to 0·654, or the viscosity of the mineral oil being 0·95 that for olive oil; had this been taken into account, the value of a_x for brass No. 2 would have been still nearer that for brass No. 1, being ·00084 as against ·00077.

As the radii of the two brasses seem to be so near, and as the resistance was measured for brass No. 1 under circumstances closely resembling those of the experiment with No. 2, a further test of the exactness of the theory is furnished by comparing the calculated friction with brass No. 2 with that measured with brass No. 1, with the same oil, the same speed, and nearly the same load.

As in equation (158)

$$-f' = 2·3773K_2 + 1·37\,(n-1)\,K_2 \text{..................}(165)$$

$$-K_2 = ·21\frac{\mu}{a}N.$$

Whence, taking account of the values of μ for mineral and olive oils, and the values of a for brass No. 1 and No. 2 for mineral oil and brass No. 2, K_2 has 0·871 of the value in equation (150)

$$-K_2 = \frac{·871 \times ·0034e^{-·0443T_m}}{(1+·002L')\,n}N \text{.....................}(166),$$

which, when $T = 5·83°$, $N = 150$, $L' = 337$, $n = 1·25$, being substituted in the equations

$$K_2 = -0·1665$$

$$f' = 4·46 \text{...................................}(167).$$

In Mr Tower's Table IV., it appears that with brass No. 1, mineral oil,

$$N = 150 \qquad L' = 310 \qquad f' = 4·4 \qquad L' = 415 \qquad f' = ·51,$$

whence interpolating for

$$L' = 337$$

$$f' = 4·58 \text{.................................}(168).$$

This agreement is very close, for taking account of the difference of radius, the calculated friction for brass No. 2 should have been about ·95 of the measured friction with brass No. 2.

In order to show the agreement between the calculated pressures and those of Mr Tower, the values of $\frac{p-p_0}{K_1c}$ for $c = ·5$ have been plotted, and are shown in Figs. 18 and 19 (page 280), the crosses indicating the experiments with brass No. 2, as in Table VII. (Tower).

43. *Conclusions.*

The experiments to which the theory has been definitely applied may be taken to include all Mr Tower's experiments with the 4-inch journal and oil bath, in which the number of revolutions per minute was between 100 and 450, and the nominal loads in lbs. per sq. inch between 100 and 415. The other experiments with the oil bath were with loads from 415, till the journal seized at 520, 573, or 625; and a set of experiments with brass No. 2 at 20 revolutions per minute. All these experiments were under extreme conditions, for which, by the theory, c was so great as to render lubrication incomplete, and preclude the application of the theory without further integrations.

The theory has, therefore, been tested by experiments throughout the entire range of circumstances to which the particular integrations undertaken are applicable. And the results, which in many cases check one another, are consistent throughout.

The agreement of the experimental results with the particular equations obtained on the assumption that the brass, as well as the journal, are truly circular, must be attributed to the same causes as the great regularity presented by the experimental results themselves.

Fundamental amongst these causes is, as Mr Tower has pointed out, the perfect supply of lubricant obtained with the oil bath. But scarcely less important must have been the truth with which the brasses were first fitted to the journal, the smallness of the subsequent wear, and the variety of the conditions as to magnitude of load, speed, and direction of motion.

That a brass in continuous use should preserve a circular section with a constant radius requires either that there should be no wear at all, or that the wear at any point P should be proportional to $\sin(90° - POH)$.

Experience shows that there is wear in ordinary practice, and even in Mr Tower's experiments there seems to have been some wear. In these experiments, however, there is every reason to suppose that the wear would have been approximately proportional to $c\sin(\phi_0 - \theta) = c\sin(90° - POH)$, because this represents the approach of the brass to the journal within the mean distance a, for all points, except those at which it is negative; at these there would be no wear. So long then as the journal ran in one direction only, the wear would tend to preserve the radius and true circular form of that portion of the arc from C to F (Fig. 17, p. 266), altering the radius at F, and enlarging it from F to D. On reversal, however, C and F change sides, and hence alternate motion in both directions would preserve the radius constant all over the brass. The experience, emphasized by Mr Tower, that the journal after running for some time in one direction would not run at first in the other, strongly bears out this conclusion. Hence it follows that had the journal been continuously run in one direction, the condition of lubrication, as shown by the distribution of oil pressure round the journal, would have been modified, the pressure falling between O and B on the *on* side of the journal, a conclusion which is borne out by the fact that in the experiments with brass No. 2, which was run for some time continuously in one direction, the pressure measured on the *on* side is somewhat below that calculated on the assumption of circular form, although the agreement is close for the other four points (see Fig. 18, page 280).

When the surfaces are completely separated by oil it is difficult to see what can cause wear. But there is generally metallic contact at starting, and hence abrasion, which will introduce metallic particles into the oil (blacken it); these particles will be more or less carried round and round, causing wear and increasing the number of particles and the viscosity of the oil. Thus the rate of wear would depend on the impurities in the oil, the values of c, $1/a$ and the velocity of the journal, and hence would render the greatest velocity at which the maximum load could be carried with a large value of c small. A conclusion which seems to be confirmed by Mr Tower's experiments at twenty revolutions per minute.

In cases such as engine bearings, the wear causes the radius of curvature of the brass continually to increase, and hence a and c must continually increase with wear. But in order to apply the theory to such cases the changes in the direction of the load (or U_1 and V_1) have to be taken into account.

That the circumstances of Mr Tower's experiments are not those of ordinary practice, and hence that the particular equations deduced in order to apply the theory to these experiments do not apply to ordinary cases, does not show that the general theory, as given in equations (15), (18), and (19) could not be applied to ordinary cases were the conditions sufficiently known.

These experiments of Mr Tower have afforded the means of verifying the theory for a particular case, and hence have established its truth as applicable to all cases for which the integrations can be effected.

The circumstances expressed by

$$\mu, \frac{L}{U}, \frac{a}{R}, c, \phi_0, \phi_1, n, m, C, A, E, B$$

which are shown by the theory to be the principal circumstances on which lubrication depends, although not the same in other cases, will still be the principal circumstances, and indicate the conditions to be fulfilled in order to secure good lubrication.

The verification of the equations for viscous fluids, under such extreme circumstances, affords a severe test of the truth and completeness of the assumptions on which these equations were founded. While the result of the whole research is to point to a conclusion (important in Natural Philosophy) that not only in cases of intentional lubrication, but wherever hard surfaces under pressure slide over each other without abrasion, they are separated by a film of some foreign matter, whether perceivable or not. And that the question as to whether this action can be continuous or not, turns on whether the action tends to preserve the matter between the surfaces at the points of pressure, as in the apparently unique case of the revolving journal, or tends to sweep it to one side, as is the result of all backwards and forwards rubbing with continuous pressure.

The fact that a little grease will enable almost any surfaces to slide for a time, has tended doubtless to obscure the action of the revolving journal to maintain the oil between the surfaces at the point of pressure. And yet, although only now understood, it is this action that has alone rendered our machines, and even our carriages possible. The only other self-acting system of lubrication is that of reciprocating joints with alternate pressure on and separation (drawing the oil back or a fresh supply) of the surfaces. This plays an important part in certain machines, as in the steam-engine, and is as fundamental to animal mechanics as the lubricating action of the journal is to mechanical contrivances.

53.

ON THE FLOW OF GASES.

[*From the* "Philosophical Magazine," *March*, 1886.]

(*Read before the* "Manchester Literary and Philosophical Society," November 17, 1885.)

1. AMONGST the results of Mr Wilde's experiments on the flow of gas, ne, to which attention is particularly called, is that when gas is flowing from discharging vessel through an orifice into a receiving vessel, the rate at vhich the pressure falls in the discharging vessel is independent of the ressure in the receiving vessel, until this becomes greater than about five-enths the pressure in the discharging vessel. This fact is shown in Tables V. and V. in Mr Wilde's paper; thus, the fall of pressure from 135 lbs. 9 atmospheres) in the discharging vessel is 5 lbs. in 7·5 seconds for pressures n the receiving vessel, ranging from one half-pound to nearly 5 or 6 atmo-pheres.

With smaller pressures in the discharging vessel, the times occupied by he pressure in falling a proportional distance are nearly the same, until the ressure in the receiving vessel reaches about the same relative height.

What the exact relation between the two pressures is when the change n rate of flow occurs, is not determined in these experiments. For as the hange comes on slowly, it is at first too small to be appreciable in such hort intervals as 7·5 and 8 seconds. But an examination of Mr Wilde's 'able VI. shows that it lies between ·5 and ·53.

This very remarkable fact, to which Mr Wilde has recalled attention, xcited considerable interest fifteen or twenty years ago. Graham does not ppear to have noticed it, although on reference to Graham's experiments it ppears that these also show it in the most conclusive manner (see Table IV., 'hil. *Trans.* 1846, Vol. IV. pp. 573—632; also Reprint, p. 106). These experiments also show that the change comes on when the ratio of the pressures is between ·483 and ·531.

R. D. Napier appears to have been the first to make the discovery*. He found, by his own experiments on steam, that the change came on when the ratio of pressures fell to ·5 (see *Encyc. Brit.* Vol. XII. p. 481). Zeuner, Fliegner, and Hirn have also investigated the subject.

At the time when Graham wrote, a theory of gaseous motion did not exist. But after the discovery of the mechanical equivalent of heat and thermodynamics, a theory became possible, and was given with apparent mathematical completeness in 1856. This theory appeared to agree well with experiments until the particular fact under discussion was discovered. This fact, however, directly controverts the theory. For on applying the equations giving the rate of flow through an orifice to such experiments as Mr Wilde's, it appears that there is a marked disagreement between the calculated and experimental results. The calculated results are even more remarkable than the experimental; for while the experiments only show that diminishing the pressure in the receiving vessel below a certain limit does not increase the flow, the equations show that by such diminution of pressure the flow is actually reduced and eventually stopped altogether.

In one important respect, however, the equations agree with the experiments. This is in the limit at which diminution of pressure in the receiving vessel ceases to increase the flow, which limit by the equations is reached when the pressure in the receiving vessel is ·527 of the pressure in the discharging vessel.

The equations referred to are based on the laws of thermodynamics, or the laws of Boyle, Charles, and that of the mechanical equivalence of heat. They were investigated by Thomson and Joule (see *Proc. Roy. Soc.*, May 1856), and by Prof. Julius Weisbach (see *Civilingenieur*, 1856); they were given by Rankine (articles 637, 637 A, Applied Mechanics), and have since been adopted in all works on the theory of motion of fluids.

Although discussed by the various writers, the theory appears to have stood the discussion without having revealed the cause of its failure; indeed, Hirn, in a late work, has described the theory as mathematically satisfactory.

Having passed such an ordeal, it was certain that if there were a fault, it would not be on the surface. But that by diminishing the pressure on the receiving side of the orifice the flow should be reduced and eventually

* The account of R. D. Napier's experiments is contained in letters in the *Engineer*, 1867, vol. xxiii. January 4 and 25. They were made with steam generated in the boiler of a small screw-steamer and discharged into an iron bucket, the results being calculated from the heat imparted to a constant volume of water in the bucket in which the steam was condensed.

stopped, is a conclusion too contrary to common sense to be allowed to pass when once it is realized; even without the direct experimental evidence in contradiction, and in consequence of Mr Wilde's experiments, the author was led to re-examine the theory.

2. On examining the equations, it appears that they contain one assumption which is not part of the laws of thermodynamics, or of the general theory of fluid motion. And although commonly made and found to agree with experiments in applying the laws of hydrodynamics, it has no foundation as generally true. To avoid this assumption, it is necessary to perform for gases integrations of the fundamental equations of fluid motion which have already been accomplished for liquids. These integrations being effected, it appears that the assumption above referred to has been the cause of the discrepancy between the theoretical and experimental results, which are brought into complete agreement, both as regards the law of discharge and the actual quantity discharged. The integrations also show certain facts of general interest as regards the motion of gases.

When gas flows from a reservoir sufficiently large, and initially (before flow commences) at the same pressure and temperature, then, gas being a nonconductor of heat when the flow is steady, a first integration of the equation of motion shows that the energy of equal elementary weights of the gas is constant. This energy is made up of two parts, the energy of motion and the intrinsic energy. As the gas acquires energy of motion, it loses intrinsic energy to exactly the same extent. Hence we have an equation between the energy of motion, *i.e.* the velocity of the gas, and its intrinsic energy. The laws of thermodynamics afford relations between the pressure, temperature, density, and intrinsic energy of the gas at any point. Substituting in the equation of energy, we obtain equations between the velocity and either pressure, temperature, or density of the gas.

The equation thus obtained between the velocity and pressure is that given by Thomson and Joule; this equation holds at all points in the vessel or the effluent stream. If, then, the pressure at the orifice is known, as well as the pressure well within the vessel where the gas has no energy of motion, we have the velocity of gas at the orifice; and obtaining the density at the orifice from the thermodynamic relation between density and pressure, we have the weight discharged per second by multiplying the product of velocity with density by the effective area of the orifice. This is Thomson and Joule's equation for the flow through an orifice. And so far the logic is perfect, and there are no assumptions but those involved in the general theories of thermodynamics and of fluid motion.

But in order to apply this equation, it is necessary to know the pressure at the orifice; and here comes the assumption that has been tacitly made: *that the pressure at the orifice is the pressure in the receiving vessel at a distance from the orifice.*

3. The origin of this assumption is that it holds, when a denser liquid like water flows into a light fluid like air, and approximately when water flows into water.

Taking no account of friction, the equations of hydrodynamics show that this is the only condition under which the ideal liquid can flow steadily from a drowned orifice. But they have not been hitherto integrated so far as to show whether or not this would be the case with an elastic fluid.

In the case of an elastic fluid, the difficulty of integration is enhanced. But on examination it appears that there is an important circumstance connected with the steady motion of gases which does not exist in the case of liquid. This circumstance, which may be inferred from integrations already effected, determines the pressure at the orifice irrespective of the pressure in the receiving vessel when this is below a certain point.

4. To understand this circumstance, it is necessary to consider a steady narrow stream of fluid in which the pressure falls and the velocity increases continuously in one direction.

Since the stream is steady, equal weights of the fluid must pass each section in the same time; or, if u be the velocity, ρ the density, and A the area of the stream, the joint product $u\rho A$ is constant all along the stream, so that

$$A = \frac{W}{g\rho u},$$

where $\frac{W}{g}$ is the mass of fluid which passes any section per second.

In the case of a liquid ρ is constant, so that the area of the section of the stream is inversely proportional to the velocity, and therefore the stream will continuously contract in section in the direction in which the velocity increases and the pressure falls, as in Fig. 1, also Fig. 2 A.

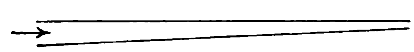


Fig. 1.

In the case of a gas, however, ρ diminishes as the velocity increases and the pressure falls; so that the area of the section will not be inversely proportional to u, but to $u \times \rho$, and will contract or increase according to whether u increases faster or slower than ρ diminishes.

As already described, the value of ρu may be expressed in terms of the pressure. Making this substitution, it appears that ρu increases from zero as p diminishes from a definite value p_1 until $p = {\cdot}527p_1$; after this ρu diminishes to zero as p diminishes to zero. A varies inversely as ρu, and therefore diminishes from infinity as p diminishes from p_1 till $p = {\cdot}527p_1$; then A has a minimum value and increases to infinity as p diminishes to zero, as in Fig. 2.

Fig. 2.

The equations contain the definite law of this variation, which, for a particular fall of pressure, is shown in Fig. 2 A.

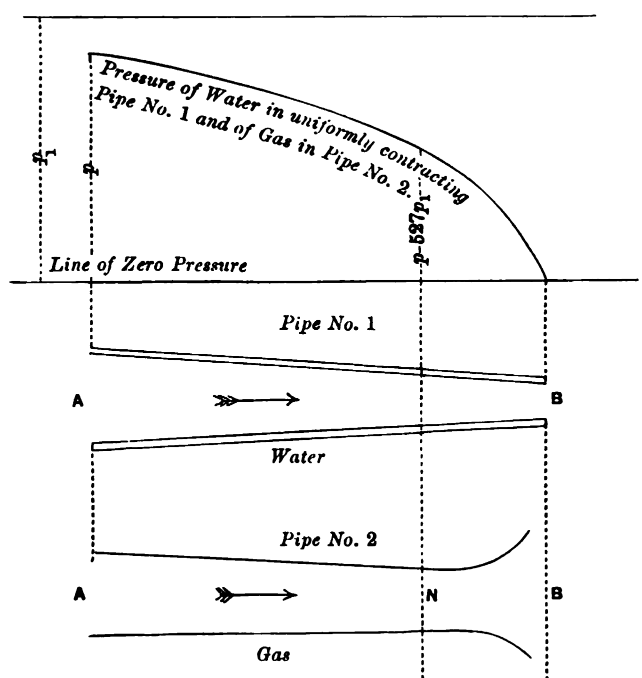


Fig. 2 A.

For the present argument it is sufficient to notice that A has a minimum value when $p = {\cdot}527p_1$; since this fact determines the pressure at the orifice when the pressure in the receiving vessel is less than ${\cdot}527p_1$, that being the pressure in the discharging vessel.

5. If, instead of an orifice in a thin plate, the fluid escaped through a pipe which gradually contracted to a nozzle, then it would follow at once,

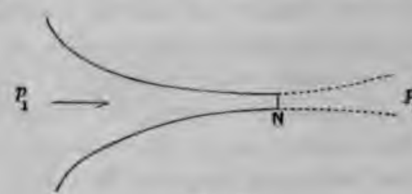


Fig. 3.

from what has been already said, that when p_2 was less than $\cdot 527 p_1$, the narrowest portion of the stream would be at N, for since the stream converges to N the pressure above N can be nowhere less than $\cdot 527 p_1$; and since emerging into the smaller surrounding pressure p_2 the stream would expand laterally, N would be the minimum breadth on the stream, and hence the pressure at N would be $\cdot 527 p_1$. In a broad view we may in the same way look on an orifice in the wall of a vessel as the neck of a stream. But if we begin to look into the argument, it is not so clear, on account of the curvature of the paths in which some of the particles approach the orifice.

Since the motion with which the fluid approaches the orifice is steady, the whole stream, which is bounded all round by the wall, may be considered to consist of a number of elementary streams, each conveying the same quantity of fluid. Each of these elementary streams is bounded by the neighbouring streams, but as the boundaries do not change their position they may be considered as fixed.

The figure (4) shows approximately the arrangement of such stream. But for the mathematical difficulty of integrating the equations of motion, the exact form of these streams might be drawn. We should then be able to determine exactly the necks of each of these streams. Without complete integration, however, the process may be carried far enough to show that the lines bounding the streams are continuous curves which have for asymptotes on the discharging-vessel side lines radiating from the middle of the orifice at equal angles, and, further, that these lines all curve round the nearest edge of the orifice, and that the curvature of the streams diminishes as the distance of the stream from the edge increases.

These conclusions would be definitely deducible from the theory of fluid motion could the integrations be effected, but they are also obvious from the figure and easily verified experimentally by drawing smoky air through a small orifice.

From the foregoing conclusions it follows, that if a curve be drawn from

A to B, cutting all the streams at right angles, the streams will all be converging at the points where this line cuts them, hence the necks of the streams will be on the outflow side of this curve. The exact position of

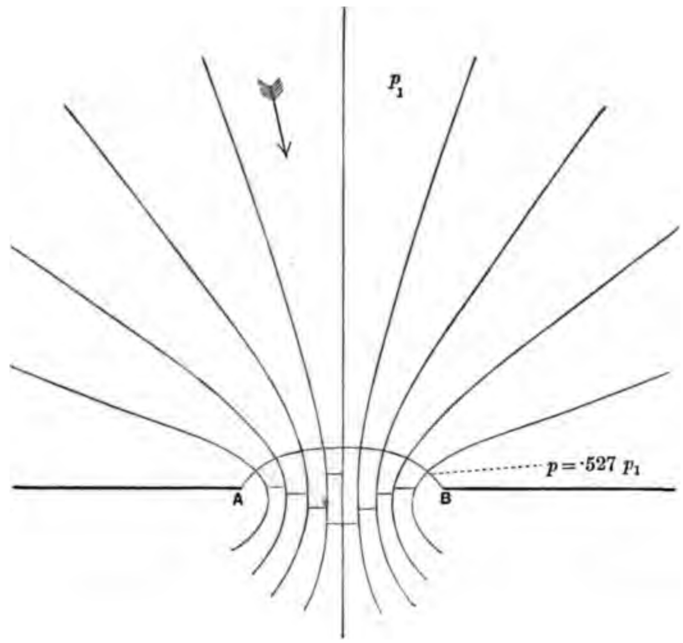


Fig. 4.

these necks is difficult to determine, but they must be nearly as shown in the figure by cross lines. The sum of the areas of these necks must be less than the area of the orifice, since, where they are not in the straight line AB the breadth occupied on this line is greater than that of the neck. The sum of the areas of the necks may be taken as the effective area of the orifice; and, since all the streams have the same velocity at the neck, the ratio which this aggregate area bears to the area of the orifice may be put equal to K, a coefficient of contraction.

If the pressure in the vessel on the outflow side of the orifice is less than $\cdot 527 p_1$, this is the lowest pressure possible at the necks, as has already been pointed out, and on emerging the streams will expand again, as shown in the Fig. 4, the pressure falling and the velocity increasing, until the pressure in the streams is equal to p_2, when in all probability the motion will become unsteady.

If p_2 is greater than $\cdot 527 p_1$, the only possible form of motion requires the pressure in the necks to be p_2, at which point the streams become parallel until they are broken up by eddying into the surrounding fluid (Fig. 5).

6. There is another way of looking at the problem, which is the first that presented itself to the author.

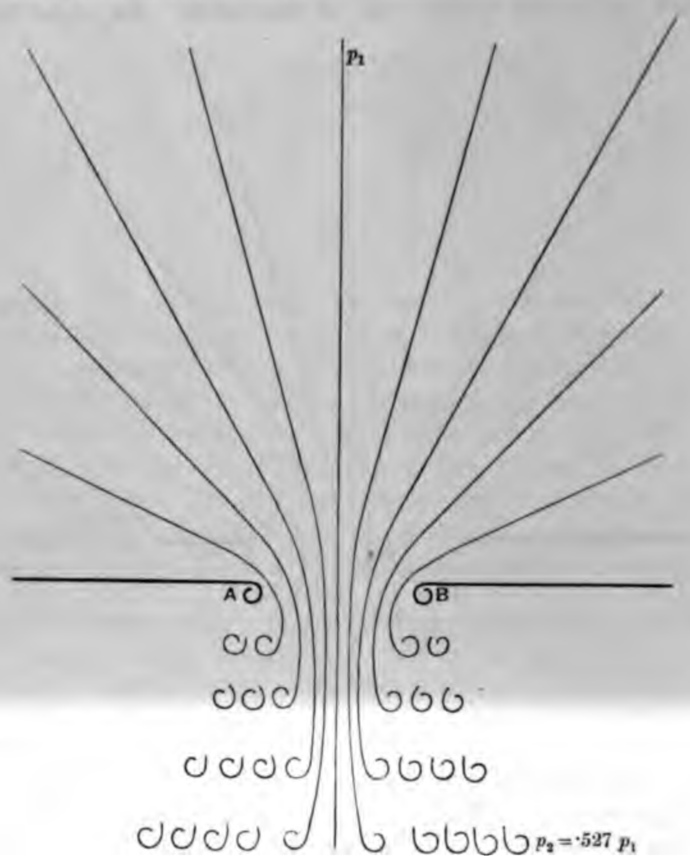


Fig. 5.

Suppose a parallel stream flowing along a straight tube with a velocity u, and take a for the velocity with which sound would travel in the same gas at rest, the velocity with which a wave of sound or any disturbance would move along the tube in an opposite direction to the gas would be $a - u$. If then $a = u$, no disturbance could flow back along the tube against the motion of the gas; so that, however much the pressure might be suddenly diminished at any point in the tube, it would not affect the pressure at points on the side from which the fluid is flowing. Thus, suppose the gas to be steam and this to be suddenly condensed at one point of the tube, the fall of pressure would move back against the motion, increasing the motion till $u = a$, but not further; just as in the Bunsen's burner the flame cannot flow back into the tube so long as the velocity of the explosive mixture is greater than the velocity at which the flame travels in the mixture.

According to this view, the limit of flow through an orifice should be the velocity of sound in gas, in the condition as regards pressure, density, and temperature, of that in the orifice; and this is precisely what it is found to be on examining the equations.

7. The following is the definite expression of the foregoing argument.

The adiabatic laws for gas are: p being pressure, ρ density, τ absolute temperature, and γ the ratio of the specific heats,

$$\frac{\tau}{\tau_0} = \left(\frac{p}{p_0}\right)^{\frac{\gamma-1}{\gamma}} = \left(\frac{\rho}{\rho_0}\right)^{\gamma-1} \quad\text{.........................(1).}$$

The equation of motion, u being the velocity and x the direction of motion, is

$$\rho u \frac{du}{dx} = -\frac{dp}{dx},$$

or
$$\frac{u^2}{2} = -\int_0^p \frac{dp}{\rho} + C \quad\text{..............................(2).}$$

Substituting from equations (1),

$$\int_0^p \frac{dp}{\rho} = \frac{\gamma}{\gamma-1}\frac{p_0}{\rho_0}\frac{\tau}{\tau_0};$$

$$\therefore\ u = \sqrt{\frac{2\gamma}{\gamma-1}\frac{p_0}{\rho_0}\frac{\tau_1}{\tau_0}\left\{1-\left(\frac{p}{p_1}\right)^{\frac{\gamma-1}{\gamma}}\right\}} \quad\text{.....................(3),}$$

$$\rho = \frac{\rho_0\tau_0 p_1}{p_0\tau_1}\left(\frac{p}{p_1}\right)^{\gamma-1} \quad\text{..........................(4);}$$

$$\therefore\ \frac{W}{g} = Ap_1\sqrt{\frac{2\gamma\rho_0\tau_0}{(\gamma-1)\,p_0\tau_1}}\sqrt{\left\{1-\left(\frac{p}{p_1}\right)^{\frac{\gamma-1}{\gamma}}\right\}}\left(\frac{p}{p_1}\right)^{\frac{1}{\gamma}} \quad\text{.........(5).}$$

Hence along a steady stream, since W is constant, equation (5) gives a relation that must hold between A and p.

Differentiating A with respect to p and making $\frac{dA}{dp}$ zero, it appears

$$2p_1^{\frac{\gamma-1}{\gamma}} = (\gamma+1)\,p^{\frac{\gamma-1}{\gamma}} \quad\text{..............................(6),}$$

or
$$\frac{p}{p_1} = \left(\frac{2}{\gamma+1}\right)^{\frac{\gamma}{\gamma-1}} \quad\text{................................(7).}$$

For air $\gamma = 1{\cdot}408$.

$$\therefore\ \frac{p}{p_1} = {\cdot}527 \quad\text{..............................(8).}$$

It thus appears that as long as p falls, the section continuously diminishes to a minimum value when $p = \cdot527 p_1$, and then increases again. Substituting this value of p in equation (3),

$$u = \sqrt{\frac{2\gamma p_0 \tau_1}{(\gamma+1)\rho_0 \tau_0}} \quad \text{.....................(9)},$$

$$= \sqrt{\frac{2\gamma p_0}{(\gamma+1)\rho_0}} \left(\frac{p_1}{p_0}\right)^{\frac{\gamma-1}{2\gamma}} \quad \text{.....................(10)},$$

$$= \sqrt{\frac{2\gamma p_0}{(\gamma+1)\rho_0}} \left(\frac{p}{p_0}\right)^{\frac{\gamma-1}{2\gamma}} \left(\frac{p_1}{p}\right)^{\frac{\gamma-1}{2\gamma}} \quad \text{.....................(11)}.$$

Hence by equation (6),

$$u = \sqrt{\frac{\gamma p_0 \tau}{\rho_0 \tau_0}} \quad \text{.....................(12)},$$

which is the velocity of sound in the gas at the absolute temperature τ.

It thus appears that the velocity of gas, at the point of minimum area of a stream along which the pressure falls continuously, is equal to the velocity of sound in the gas at that point.

8. From the equation of flow (5) it appears that for every value of A other than its minimum value, there are two possible values of the pressure which satisfy the equation, one being greater and the other less than

$\cdot527 p_1$.

It therefore appears that in a channel having two equal minima values of section A and C, as in Fig. 6, the flow from A to B may take place in either

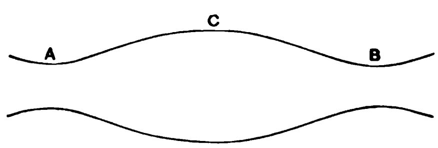


Fig. 6.

of two ways when the velocity is such that the pressure at A and B is $\cdot527 p_1$, *i.e.* the pressure may either be a maximum or a minimum at C. In this respect gas differs entirely from a liquid, with which the pressure can only be a maximum at C.

54.

ON METHODS OF INVESTIGATING THE QUALITIES OF LIFEBOATS.

[*From the* "Proceedings of the Manchester Literary and Philosophical Society," Vol. XXVI.]

(*Read* December 14, 1886.)

THE lamentable accidents to the St Anne's and Southport lifeboats on the 9th inst. seem likely to lead to steps being taken to obtain a more systematic investigation as to the qualities of these boats than has yet been undertaken.

It seems, therefore, a proper time to direct attention to certain facts and general considerations, the importance of which have impressed themselves upon me during many years' investigation.

Before entering upon this, it may be remarked that there is probably no class of boats, on the design and construction of which more attention and skill have been spent, than on lifeboats, or of which the qualities are so well adapted to the circumstances, taken all round. If we compare the results of the use of these boats with the results obtained in the use of the navies of this or any other country, it will, without a moment's hesitation, be admitted that the designers of lifeboats and lifeboat paraphernalia have arrived much nearer perfection than the designers of war vessels and their armaments.

That the high standard already obtained by these boats has not been the result of scientific investigation, or the theoretical application of any known principles of equilibrium, does not render the method less scientific, for the base of all science is observation and experiment, and these boats are the result of such a course of direct experiments and experimental observation as has not been expended on any other modern structure, nor is this method of arriving at the best form peculiar to lifeboats.

With the exception of the large modern steamers and ironclads, the peculiar construction of boats of all sizes is the result of a prolonged process of trial and failure, and that, although certain general principles, connecting the qualities of ships with their shapes, have been discovered and recognised during the last thirty years, still, the recognition of these principles has not resulted in the suggestion of any considerable improvement to be effected in what were before high class vessels, such as yachts and fast sailing vessels, but rather have confirmed the form previously arrived at in these as the best, and led to their being copied in larger vessels.

The discovery and recognition of principles have undoubtedly been of immense service in improving the types of our large modern vessels. But this is mainly because with large ships there is not the same opportunity for trial and failure as with the small, the number is so much smaller, and experiments are so much slower and more costly; but the main reason is, that the circumstances which call out the highest qualities of the large vessels become so extremely rare. There is no doubt that many large vessels pass through their lives without meeting weather which tests their sea-going qualities in the way in which those of a fishing boat are tested many times every winter. It was, therefore, an immense step in the way to study the resistance qualities of large ships, when the late Mr Froude brought into practice the rules connecting the resistance of the full-sized vessel with that of an exact model to scale.

By means of a tank 200 feet long, and models on scales of 1 to 50, or 1 to 20, the resistance and rolling qualities of all Her Majesty's ships have since been verified before they are constructed. And the same is now done by manufacturers of mercantile vessels, like Mr Denny, who have tanks of their own. The qualities of ships thus tested were originally limited to those of resistance and of rolling, and so far as I know, no extension has taken place; for although in 1876 it was pointed out by the author before Section 9 of the British Association, that by constructing models of our war ships on a scale large enough to enable them to be used as launches, say 1 to 16, and supplying these launches with power as the cube of their dimensions—then the manœuvering qualities would be similar if conducted on scales proportional to their lengths, the time occupied by the launches in executing a particular evolution, as compared with that occupied by the ships, being as the square root of their lengths. So that with such models the officers and seamen could be instructed in the handling of their ships without cost or risk. This has not been done. The Admiralty replying, so far as they did reply, that their officers were continually experimenting with the launches—disregarding the fact that the launches in use were in no sense models of the ships, and were supplied with power five or six times too great in proportion—thus ignoring the point of the suggestion, namely, that the experience gained by the models might be applicable to the ships, which with their present launches it is not, and only tends to mislead those who attempt a comparison.

Since making this suggestion, I have been much engaged in experiments with water, which have enabled me to extend this law of similarity, until I find it is possible now to lay down the conditions under which to test the seaworthy qualities of a vessel from those of its model.

Certain conditions have to be observed, but, in general, it may be asserted that provided the models are to scale, that the height and length of the waves are to the same scale, the velocity of the wind being as the square root of the scale, or in other words, the corresponding depressions of the barometer in the same scale as the models—the behaviour of the model would be similar to that of the boat.

Thus, the behaviour of a model three feet long in waves two feet high, and with a wind twenty miles an hour, would correspond with that of a boat twenty-seven feet long in waves eighteen feet high, and a velocity of the wind sixty miles an hour.

The main object of this communication is to point out that this similarity in the behaviour of models and larger boats under circumstances as regards the stress of weather, corresponding in scale to that of the models and boats, affords an opportunity of testing the seaworthy qualities of the lifeboats in a degree that they cannot otherwise be tested. For, although the size of the boats does not preclude the possibility of their qualities being actually tested under any circumstances of sufficiently common occurrence to afford opportunities, yet the circumstances which call for the highest qualities in these boats, and in which the boats are most needed, are of extremely rare occurrence; this appears at once, when it is considered that it is years since anything approaching such a storm as wrecked the two boats has been experienced, and that in order to submit any modified boat to a similar test, it may be years before there will be another chance, even if it could be made available when it did come. To make satisfactory tests on the full-sized boats, command is wanted of the extreme circumstances, and this cannot be had; while on the other hand, to test the same qualities in their models, these extreme circumstances, modified to scale, are all that is wanted, and these are of such common occurrence as to afford ample opportunity, even if they cannot be commanded by artificial means.

If the qualities to be tested involved the handling of the boats, then the models must be large enough to carry a crew; that is to say, they would have to be small lifeboats. Even with such, much experience can be and has been gained, which could not be obtained with larger boats, for bad weather for the smaller is only moderate for the larger, and is of comparatively common occurrence compared with that which affords a similar test for the larger boats.

It is, however, the self-righting qualities of these boats that is for the moment in question; this requires no crew, or at most a dummy crew, so that there is no limit to the smallness of the models, except what arises from the conditions of dynamical similarity, and these would admit of models as small as two or three feet.

It may be well to say one word as to the powers of self-righting, and the question as to how far these powers may be affected by the wind and waves. I do not know that it has ever been suggested that wind and wave have any such effect. But it is equally certain, that there is no *à priori* reason why they should not, and, short of actual experience, it cannot be said that any boat which would right itself in calm water would do so equally well in any storm that might blow. On the other hand there are reasons why wind and waves must, individually and collectively, affect the stability of an upturned boat.

In the first place, the wind will keep such a boat broadside on, which will be in the trough of the sea raised by the wind, although the swell may, of course, be running in another direction. The wind, acting on the bottom, will further drive the boat broadside on through the water. This horizontal thrust of the wind, acting on the part of the boat above water, and balanced by the resistance of the water on the submerged portion, will tend to right the boat by turning her keel to leeward, and so far it would seem that the wind would help to right her, but owing to the shape of the bottom of the boat when broadside on, there will be a vertical force resulting from the wind as well as the horizontal, and this vertical force will bear down that side of the boat toward the wind, and this effect will be enhanced by the weight of the waves breaking on this side of the boat tending to right her by turning her keel to windward, or in direct opposition to the horizontal effect; and more than this, the vertical effect of the wind and waves to turn the keel to windward will be greatest when the windward side of the boat's bottom has some definite inclination to the horizontal, while the horizontal effect to turn the keel to leeward will continually increase as the keel turns to windward, so that it is possible that in a particular wind and sea there may be a position of very stable equilibrium, towards which, if the keel is to leeward, the vertical effect of the wind and the waves predominating over the horizontal effect, will bring it back, and *vice versâ*; if the keel is turned to windward, the horizontal effect predominating will also tend to bring it back.

The fact that two boats were found stranded bottom upwards, with part of their crews underneath, and that one of these is known to have upset in comparatively deep water, and to have remained in that position during a long time while drifting into shallow water, seems altogether inconsistent with the supposition that these upturned boats were in their normal condition of instability, as when in calm water. For although in a calm sea the effect of three or four men hanging on to each side of the boat might prevent the initial motion of turning, before the weight of the iron keel and ballast obtained sufficient leverage to lift the weight of the men and so keep the boat stable, this could hardly be the case in a rough sea, when the waves would be continually altering the balance of the boat.

These are questions which can only be set at rest by experiments, and the method of models thus affords a means of testing the righting qualities of these boats under circumstances as severe or more severe than any to which they will ever be subjected, and this without waiting and without danger; while with full-sized boats such tests are impossible, for even should an extreme storm occur opportunely for making the experiment, the danger involved with full-sized boats would preclude the possibility of their being undertaken. It is this last consideration which has led to these suggestions, and not the idea that the experiments on models would be more satisfactory; while the fact that the experiments on models could be made at much smaller cost, is too small a matter to be considered, when, as in this case, the lives of some of the most heroic of our fellow countrymen, and the sentiments of the entire nation, are involved.

55.

ON CERTAIN LAWS RELATING TO THE RÉGIME OF RIVERS AND ESTUARIES, AND ON THE POSSIBILITY OF EXPERIMENTS ON A SMALL SCALE.

[*From the* "Report of the British Association," 1887.]

1. THE object of this communication is to bring before Section G certain results and conclusions with respect to the action of water to arrange loose granular material over which it may be flowing. These results and conclusions were in the first instance arrived at during a long-continued investigation, undertaken with a view to bring the general theory of hydrodynamics into accord with experience, rather than with any special reference to the subject in hand, but have since been to some extent made the subject of special investigation.

2. A systematic study of the *régime* of rivers naturally divides itself under three heads, which may be stated as follows:—

(1) The more general facts observed as regards the regimen of the beds.

(2) The movements of sand consistent with these observed facts.

(3) The necessary actions of the water to produce these movements in the material of the beds.

Observed facts.—Amongst the most general facts to be observed as to the arrangement of the material forming the beds of estuaries are—

(1) The general stability or steadiness of these beds, so far as is shown by their outline or figure, while, at the same time, as is shown by the obliteration of all footprints and markings casually placed upon them, also by the ripple mark, the material at the surface of these beds is being continually shifted.

(2) The almost absolute steadiness in figure of some of these beds.

(3) The gradual changes in the position and form of others—the growth or accumulation of sand-banks in some places, and the wasting of banks or removal of sand in others.

Movement of sand.—As regards the movement of sand consistent with these changes, in the first place the movement, whatever it may be, is one of the surface, and not one in bulk; and in the next place such movement of the surface must be continually going on, whether it produces any change in the figure of the banks or not. The invariable obliteration of footprints and marks which may have been left on the sand at low water, as well as the ripple marks, are absolute evidence of a general disturbance of the surface, and it requires but little observation to show that the disturbance is of the character of a drift of sand, in whatever direction the water may be moving.

Uniform drift.—Where the outline of the banks is not altered, this drift or motion of the sand must be uniform, as much sand being deposited at each point as is removed from that point. Although there may be a general flow of the sand in some direction, if the drift is uniform this movement will not alter the figure of the bed, which, like the balance in another kind of bank, does not depend on the rate of deposit and withdrawal, but on the excess of one of these over the other. The gradual accumulation or diminution of sand at any point is clearly not due to a simple action of deposit or removal, as it is always attended with the same evidence of the drifting of the surface, and is clearly the result of a difference in the quantities of sand deposited or removed by the drift.

Movement of water.—The manner in which a current of water acts on the granular material forming the bed of the current has been the subject of an investigation by various experimenters. It has been found that the primary action is not so much to drag the grains along the bottom, but to pick them up, hold them in a kind of eddying suspension, at a greater or less height above the bed, for a certain distance and then drop them, so that, when the water is drifting the sand, there is a layer of water adjacent to the bottom, of a greater or less thickness, charged to a greater or less extent with sand. The faster the current and the finer the sand the greater will be the thickness of the charged layer, as well as the denser is the charge in the layer.

A certain definite velocity, according to the size and weight of the grains, is required before the water will raise the grains from the bottom, and for all velocities above the minimum necessary to raise the sand the suspended charge increases with the velocity, and the rate of drift or the quantity of sand which passes a particular section increases much faster than the velocity. Attempts have been made, with greater or less success, to determine exact laws connecting the minimum velocities at which the sand begins to drift, with the weight of the grains and other circumstances; also to determine the exact law of rate of increase of the drift with the velocity.

For my present purpose, however, it is not necessary to enter upon such considerations.

From the facts already mentioned, it will appear that the effect of a uniform current of water over a uniform bed of sand will not be to raise or lower the bed; for, as the charge of sand in the water remains uniform, it must drop as many particles as it raises everywhere on the bed. This is the action of the water in causing a uniform drift.

It is also evident that, if the charge in the water as it comes to any particular place is less than the full charge due to its velocity, it will pick up from that place more sand than it drops, and so increase its charge at the expense of the bed, which will there be scoured or lowered. And conversely, if the water as it arrives at any place is overcharged, it will relieve itself by depositing more than it picks up, and so raise or silt up the bed.

As regards the circumstances which can cause the water to be charged to a greater or less extent than that which it would just maintain with such velocity as it has, the most important are—

(1) An increasing or diminishing velocity. When the water is moving in a stream from a point where the velocity is less to one where it is greater, the velocity of the actual water as it moves along is increasing, as will also be its normal charge of sand; hence it must be continually picking up more than it deposits. And conversely, when moving from a point of greater velocity to one of less, its normal charge will be continually diminishing through deposits on the bed.

(2) Another circumstance which affects the charge of sand with which the water may arrive at a particular point is a variation in the character of the bed. If, for instance, water flows from a rocky bed on to sand, it may arrive on the sand without charge, and immediately charges itself at the expense of the bed. Or again, where water flows from a sandy bottom on to a clean or grassy rocky bottom, it gradually loses its charge, silting up the bottom.

The direction in which the sand is moved by the water is sensibly in the direction in which the water which holds the charge is moving. But, as was first pointed out by Dr James Thomson as affording an explanation of the generally observed fact that the beds of rivers are scoured on their convex sides and silted on their concave, the layers of water adjacent to the bed do not always move in the general direction of the stream. There are often steady cross currents at the bottom, as in the case mentioned, though such cross currents do not exist except under circumstances which may be readily distinguished. The most important of these is that pointed out by Dr Thomson—curvature in the general direction of the stream, in which case the centrifugal force of the more rapidly moving water above overbalances that of the water retarded by the bottom, and forces the latter back towards the centre of the curve.

This action is universal, where even the lateral boundaries are such as to require the water to move in curved streams; the drift at the bottom does not follow the general direction of the stream, but sets towards the centre of the curve.

The result of the foregoing consideration is to lead to the conclusion that the *régime* of each part of the bed as to maintenance in steady condition, lowering or raising it any time, depends solely on the character of the motion of the water, which if straight and uniform, neither acquiring nor losing velocity, causes a uniform drift in the direction of the stream, which maintains the condition steady. If losing velocity, causes a depositing drift and raises the bed; if gaining velocity, causes a scouring drift and lowers the bed; while if curved, the direction of the drift is diverted towards the centre of the curve, with its attendant effect to lower the convex side and raise the concave side of the bed. This conclusion seems to be of the utmost importance in dealing with this subject. For if it is correct, not only can the character of the action going on at the bed be inferred from the observed motion of the water, and *vice versâ*, but since, according to this conclusion, the character of the action is independent of the magnitude or velocity of the stream, the results will be the same on a small scale as on a large one, provided only that the character of the motion of the water is the same at all points. In this latter respect this conclusion affords an explanation of a fact that cannot fail to have struck every one who has observed the sand-beds of the streams running over sands which have been left by the tide, viz., what an almost exact resemblance they bear to each other, whether having the size of a moderate river or of the smallest rivulet.

On the large scale of actual estuaries we can only test the conclusion by actual observation, but on a small scale we can experimentalise in whatever condition of motion we want to test, and readily observe the effects produced; a possibility of which great use has been made in this investigation, and which will be again referred to.

As applied to a *non-tidal* river, in which the direction of the motion is always the same, the foregoing conclusion would lead us to expect that the *régime* would be steady except at the bends, the sources, and the mouth, which is exactly what is observed, so that the conclusion so far agrees with experience. The most striking feature about rivers is the way they wriggle about in the alluvial valleys; a phenomenon pointed to by Lyell as one of those causes still in progress which had produced the present conditions of the valleys, and which, as already stated, was explained by Dr Thomson. From the source of the river, as the rain-water acquires the velocity, it charges itself with deposit, which charge it maintains with continual taxes and drawbacks until it reaches the ocean or lake, when its water in again losing its velocity deposits its charge, continually carrying forward the bar and extending its delta.

In non-tidal rivers, whether large or small, fast or slow, the characters of these actions are invariable, however much they may differ in intensity. The case of tidal estuaries is, however, by no means so simple. Here we have not, as in a river, a continuous progression of the same character of action at the same point. On the contrary, at every point the action is changed twice a day. For the change in the tidal current does not merely change or reverse the direction of the sand-drift at each part of the bed, but it changes and often reverses the character of this drift, changing what has been a scouring drift during the ebb-tide into a depositing drift during the flood; so that the question as to whether the *régime* is stable, depositing, or scouring is not simply a question as to whether the current at this point is uniform, accelerated, or retarded, but whether the action of the ebb to cause, say, scour is equal to, less than, or greater than the action of the flood to cause deposit.

As there is no likelihood that the resultant effect as regards the general *régime* of two opposing influences will resemble what would have been the simple effect of either of the influences acting alone, this dual control affords abundant reason why the configuration of the beds of these tidal estuaries should differ in character from the configuration of the sand-beds of continuous streams.

There is, however, another and an equally important difference between the general motion of the water in rivers and tidal estuaries.

The function of the estuary is by no means that of a simple channel to conduct the tidal water up and down. It equally discharges the function of a reservoir or basin, to be filled and emptied by each tide.

In consequence of this action as a reservoir, the directions of the motions of the water during flood and ebb, and particularly towards the top of the flood and commencement of the ebb, are generally very different from what they would be were the estuary acting the simple part of a channel conducting the water from one place to another.

When a vessel is filled by a stream entering on one side, the forward motion of the water is stopped before reaching the opposite side. But if, as is always the case, the motion which the water has on entering is more than sufficient to carry it as far as is necessary, the remaining momentum is spent in setting up eddies, or a general circulation in the water, so that when the vessel is full the water within it is not by any means at rest, but may be circulating round or have any other motion. If, then, the water is allowed to flow out, the initial motion will not be a steady movement towards the outlet from all parts of the vessel, but those portions of the water which are moving towards the outlet will have their motion accelerated, while those which are moving in the opposite direction will have first to be stopped before they begin to approach the outlet. And thus the ebb will begin earlier at some points in the vessel than at others.

It was the observation of such an effect as this in one of our largest estuaries that first directed my attention to the subject of this paper.

Having investigated this point sufficiently for my own satisfaction nothing further was done until 1885, when my attention was directed to the inner estuary of the Mersey.

This estuary may be described as a crescent-shaped shallow pan, eleven miles long by three broad, lying north-west and south-east, having its upper horn pointing east and its lower horn north; the northern horn, being prolonged for five miles into a narrow deep channel, runs north to the outer estuary or sandy bay of the sea. One of the most marked features presented by the configuration of the bed of this inner estuary is the invariable preference of the low-tide channels for the concave or Lancashire side; whereas, were the estuary acting merely the part of a river, whether during flood or ebb, it would be expected to follow the usual law, and have the deepest water on the convex or Cheshire side.

That this prevalence of the deepest water on the concave side must be the result of the momentum left in the water by the flood at once seemed to me probable; for if the bottom were level or deepest on the Lancashire side the effect of the curved shape would be to cause the flood entering at the northern horn to follow the south-eastern or Cheshire shore, and the momentum of this water would tend to carry it round the head of the estuary and back along the Lancashire side; would, in fact, tend to set up a circulation before the top of the flood was reached; so that on the Lancashire side the water would be moving down the estuary before the ebb commenced; whence, considering that the flood tends to raise the bottom and the ebb to lower it (for the reasons already pointed out), it seems that the stronger flood on the Cheshire side would raise this side, while the stronger ebb on the Lancashire side would lower this. This is supposing the bottom to be level.

In order to verify these conclusions a vessel was constructed having a flat bottom and a vertical boundary of the same shape as the high-tide line of the inner estuary from the rock to the same distance above Runcorn. The horizontal scale was 2″ to a mile, and the vertical scale 1 inch to 80 feet, $\frac{1}{31800}$.

A shallow tin pan was hinged on to the otherwise open channel at the rock, by raising and lowering which, when full of water, the motion of the tide could be produced throughout the model through the narrows; the true form of the bed of the channel was given to the model by means of paraffin. And in order to obtain approximately the proportional depth in the inner estuary, sand was placed level on the bottom so that the high-tide depth was reduced to the equivalent of about twenty feet. The idea in making this model was not so much to obtain a shifting of the sand, as to show the circulation of the water as resulting from the flood tide with a level bottom. In the first instance the tide pan was raised and lowered by hand, but as at the first trial it became evident that the model was not only going to show the expected circulation, but was also capable of showing, by the change in the position of the sand, the effect of this circulation on the configuration of the estuary and other important effects, it was arranged that the model should be worked from a continuously running shaft. The working of the model by hand at once showed that there was only one period of working at which the motion of the water in the model would imitate the motions of the actual tide in the Mersey, which period was found to be about forty seconds; a result that might have been foreseen from the theory of wave motions, since the scale of velocities varies as the square roots of the scales of wave heights, so that the velocities in the model which would correspond to the velocities in the channel would be as the square roots of the vertical scales—about $\frac{1}{33}$—and the ratios of the periods would be the ratio of horizontal scales divided by this ratio of velocities, or

$$\frac{33}{31800} = \frac{1}{950}.$$

Hence, taking 11·25 hours or 40,700 seconds as the tidal period, the period of the model

$$= \frac{40700}{950} = 42 \text{ seconds (about).}$$

This period was adopted for working the model from the shaft.

It was then found that the circulation at the top of the flood, which was very evident while the bottom was flat, caused a general rise of the sand on the Cheshire side and lowering on the Lancashire, which went on for about 2,000 tides. That during this time, owing to the increase of flood up the Lancashire side and the diminution of that on the Cheshire side which followed from the deepening of the one and the shoaling of the other, the circulation steadily diminished until its character was so changed that it could no longer be called a general circulation, and that after this, although there were further changes in detail going on in the estuary, the two sides maintained a steady condition as regards depth for low tides.

During this time banks were formed and low-tide channels, which resembled in all the principal features those actually in the Mersey; the eastern bank, with the deep sloynes on the Cheshire side, the Devil's Bank and the Garston Channel, the Ellesmere Channel and the deep water in Dungeon Bay and at Dingle Point—all these were very marked in character and closely approximate in scale.

And, what is as important, the causes of these as well as all minor features could be distinctly seen in the model.

The eastern and Devil's Bank are seen during the process of their formation to be simply an internal bar formed by carrying the sand brought down by the ebb out of the narrows and sloyne, until debouching into the broad estuary; its velocity is so far diminished that it can no longer carry its charge, just as happens at the mouth of every river. The peculiar configuration of these banks is explained by the existence of two lines of eddies from about half-tide to the top of the flood: the first of these is caused by the sharp corner at Dingle, and lies between Dingle and Garston, the eddies having their centres over the Devil's Bank; and the second, caused by the divergence of the Cheshire Bank towards Eastham, having the lines of centres over the Eastham Bank. These eddies, which during the most rapid part of the flood only effect a diminution of the velocity of the flood, cause, as the velocity slackens toward the top of the flood, back water to set in along both shores, which back waters, starting the ebb, cause this to be strongest over the Garston and Eastham Channels, which are thus kept open.

The lateral configuration of the shores at Dungeon Bay and at Ellesmere is seen to cause back waters to exist in these bays during the whole of the flood in the latter, and from one to two hours before the top of the flood in the former, which fully accounts for the deep water at these points. The existence of these back waters in the actual channel has been verified. There are many other circumstances brought to light by this model, which it is impossible for me here to notice without unduly extending the length of this paper, if, indeed, I have not already done so. I will therefore only remark that a second start was made with the sand flat in this second model, and that the result obtained was the same as regards the general features of the estuary. So interesting were these results that it was decided to try a larger scale. A model, having a horizontal scale of 6 inches to a mile, and a vertical scale of 33 feet to an inch, was therefore made, and the tide produced as before. The calculated period of this model is 80 seconds, and experiment bears this out, any variation leading to some tidal phenomena, such as bores or standing waves, which are not observed in the estuary.

The disadvantage of the larger model is the time occupied—a little more than a minute a tide—which means about 300 tides a day, or 2,000 tides a week. On one occasion the model was kept going for 6,000 tides, and a survey was then made of the state of the sand. And this will be seen to present a remarkable resemblance in the general features to the charts of the Mersey, of which three—1861, 1871, 1881—are shown; in fact the survey from the model presents as great a resemblance to any one of these as they do to each other.

It is impossible for me to enter upon all the points of agreement. Taking into account that in both the estuary and the model there are always changes going on within certain limits, and these changes do affect the currents to a certain extent, it is not to be supposed that there will be exact agreement between the currents at all points and at all states of the tides on the model and estuary. Still there is a general agreement, and in the few verifications I have made I have found that the current found in the model at a particular point and state of tide is also to be found in the estuary.

In one respect the great difference between the model and the estuary calls for remark: this is the much greater depth of the model as compared with its length and breadth. The vertical scale being 33 feet to an inch, and the horizontal scale 880 feet to an inch, so that the vertical heights are nearly twenty-seven times greater than the horizontal distances, such a difference is necessary to get any results at all with such small scale models; and it is only natural to suppose that it would materially affect the action. As a matter of fact, however, it does not seem to do so. And, further, it would seem that, notwithstanding the general resemblance on the *régime* of the beds of large and small streams running over sand, there is in these a similar difference in vertical scale, the smaller streams not only having a greater slope, but also having greater depth as compared with their breadth and steeper banks. So far as the theory of hydrodynamics will apply, it seems that in the model the effects of the momentum of the water would be greater, as compared with the bottom resistances, than in the estuary, and I think that they are. But the effects of momentum in the estuary greatly preponderate on the resistances, as shown by the fact that the tide at the top of the flood rises some 2 to 3 feet higher at high spring tides than it does at the rock; nor does it do much more than this in the model. In the model it certainly seems that the general *régime* is determined by the momentum effects, and from the almost exact resemblance which this *régime* bears to that of the estuary, it would seem that, although the momentum effects may be diminished by the greater resistance on the bottom, they are still the prevailing influence in determining the configuration of the banks. Further investigation will doubtless explain this, and also determine the best proportional depths. From my present experience in constructing another model, I should adopt a somewhat greater exaggeration of the vertical scale. In the meantime I have called attention to these results, because this method of experimenting seems to afford a ready means of investigating and determining beforehand the effects of any proposed estuary or harbour works; a means which, after what I have seen, I should feel it madness to neglect before entering upon any costly undertaking.

I have only to say that, as it was not practical to exhibit the model to the Section, I have had it working in the new engineering laboratory at the college. Unfortunately it could not be started before Monday, and it will not yet have run more than 1,000 tides, since the sand was put in flat, so that it is not probable that the *régime* is yet quite stable; still the principal features have come out*.

* For continuation see papers 57, 58, and 59.

56.

ON THE TRIPLE-EXPANSION ENGINES AND ENGINE-TRIALS AT THE WHITWORTH ENGINEERING LABORATORY, OWENS COLLEGE, MANCHESTER.

[*From the* "Proceedings of the Institution of Civil Engineers," 1889—90.]

(*Read* December 10, 1889.)

IN designing steam-engines to take their place amongst the appliances of an engineering laboratory, at the present stage of the development of these institutions, many considerations present themselves.

The primary purpose of the engines is to afford the students opportunities of practice in making the various measurements involved in steam-engine-trials, and to afford them an insight into the action of steam in the engine, as well as of the mechanical actions; also to render them familiar with good examples in steam-engine design.

Another purpose, however, which it is very desirable such engines should serve, is that of supplying a means of research by which knowledge of the steam-engine may be extended. A systematic and experimental investigation of the steam-engine involves two sets of conditions which, unless it be in a laboratory, can hardly exist together, namely, the time and attention of the scientific investigator, and the assistance of a considerable number of trained observers. In the engineering laboratory these conditions should exist; the first being supplied by the permanent staff, and the second by the students as their training advances.

The making and repeating of the individual observations involved in a scientific engine-trial, as well as reducing the results, demands an amount of patience and perseverance which is severe on one so young and inexperienced as a student; but the importance and reality which the research adds to all the detail of the work, as well as the complete attention and overlooking which it ensures from those responsible, constitute very great advantages.

Having regard to these two purposes, the Committee, Mr John Ramsbottom, Mr John Robinson, and the Author, appointed by the Council of Owens College to select, amongst other appliances, the steam-engines best adapted for the special purposes of the laboratory, decided that the engines, while as far as possible representing in their principal members the most approved existing practice in steam-engine construction, should be specially designed to afford the utmost facilities for experiments on the use of steam throughout the entire range, and, if possible, beyond the limits hitherto accomplished in practice.

As best meeting this demand it was decided to have three engines working on separate brakes[1]. All engines to be of the inverted-cylinder type, with the walls and covers separately jacketed with steam at boiler-pressure, and so arranged that they could be worked with or without steam in any or all of the jackets. Each engine to work with steam at any pressure up to 200 lbs. per square inch, to run at any piston speed up to 1,000 feet per minute, and to have expansion-gear to cut off from zero up to $\frac{2}{3}$ of the stroke. One engine to be supplied with air-pump and surface-condenser, the other two engines to be furnished with alternative exhausts, either into the atmosphere, or into steam-jacketed receivers supplying steam to the next engine, each of the receivers also having an alternative supply of steam direct from the boiler. The boiler to be of the locomotive type, having 5 square feet of grate, to be set in a hot chamber with an economizer and alternative chimney and forced draught, on the closed stoke-hold system. The condenser to have 200 square feet of cooling surface. The dimensions of the engines to be somewhat as follow:

Engine	Diameter of Cylinder	Stroke	Diameter of Crank-Shaft
	inches	inches	inches
No. I (high-pressure)	5	10	$2\frac{3}{4}$
No. II (intermediate)	8	10	$2\frac{3}{4}$
No. III (low-pressure)	12	15	4
Air-pump on No. III	9	$4\frac{1}{2}$	
Feed-pump „	$1\frac{1}{2}$	2	

In addition to the brake, each engine was to be furnished with a fly-

[1] The advantage of having the engines on separate brakes was suggested to the Author by Mr J. I. Thornycroft, M. Inst. C.E.

wheel, to act as a belt or rope-pulley, weighing about 1,200 lbs., carried on a separate shaft with a coupling to the crank-shaft.

The firm of Messrs Mather and Platt, Salford Iron Works, undertook the preparation of the designs and the construction of special engines and boiler to meet in all respects the wishes of the Committee, and spared neither trouble nor expense in carrying out the work. It was entirely owing to the zeal and liberality of this firm that the College was enabled to meet the expense of an undertaking involving so much special work.

The design of the engines, shown in Figs. 1 and 2, contains many novelties. These were not adopted without what appeared to the Committee to be sufficient reason, as it was unanimously desired to adhere as far as possible to ordinary types.

As regards the cylinders, pistons, and valves, there are three noticeable departures; these were adopted with a view—

1. To ensure the completeness and efficiency of the steam-jackets.

2. To diminish the resistance to the passage of steam as much as possible.

3. To keep down the clearance.

4. To obtain an adjustable cut-off from zero at any speed.

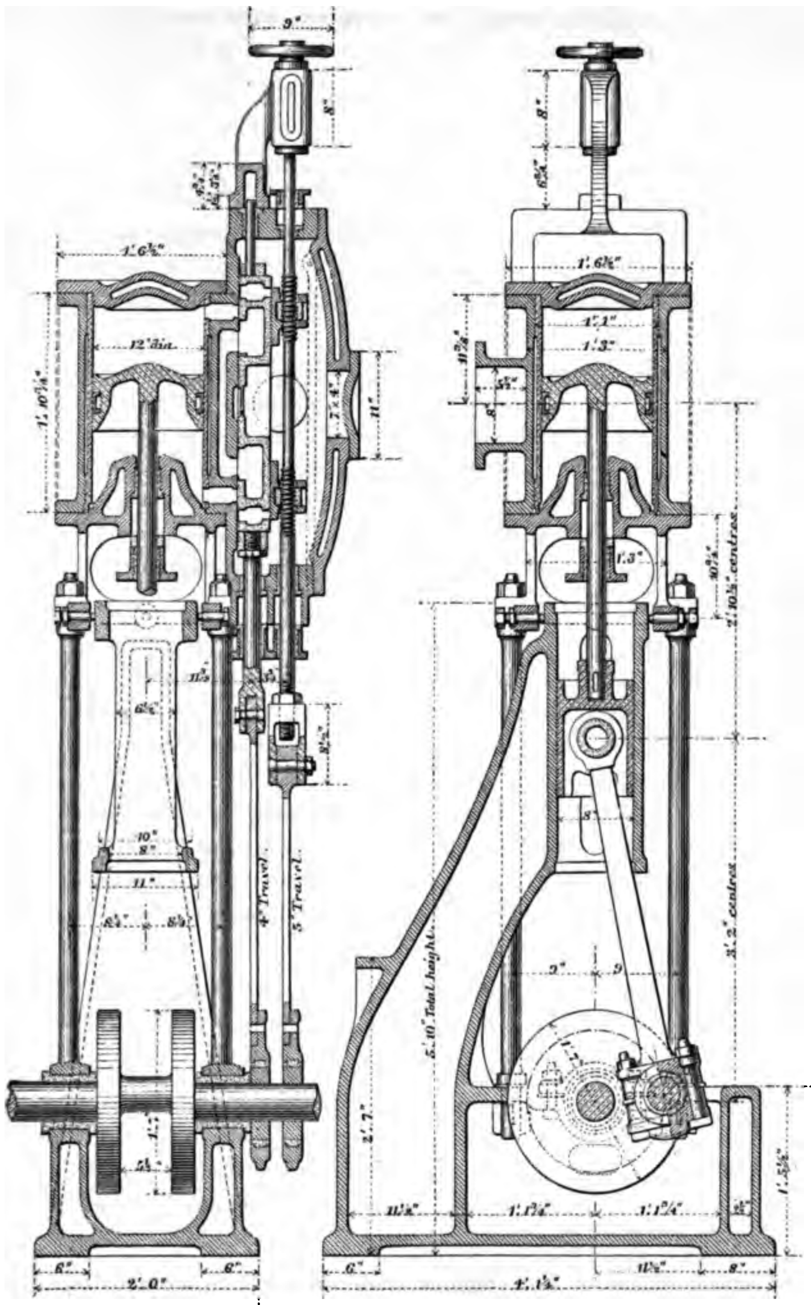
1. To obtain completeness in jacketing, both ends (or covers) were jacketed as well as the walls. To ensure efficiency of the jackets steel liners were used and the covers were domed, so that the surfaces should free themselves by gravitation from the water resulting from condensation, the water being drained from the lowest point in the jacket spaces.

2. To diminish the resistance of the passages, these were abnormally large, the area of the ports being 13 per cent. or $\frac{1}{7\cdot5}$ the area of the piston, and the steam-chests were very large.

3. To diminish clearance, the ports were made straight, and the valves brought as close as possible to the cylinder, double valves being used. The pistons were formed to occupy the space in the cylinder, except $\frac{1}{8}$ inch clearance at the ends. The result is that in engine No. I the clearance space shut in by the main valve is 4 per cent. and 1·7 per cent. more by the rider, and in engines II and III the clearances shut in by the main valve are 6 per cent. and 2·5 per cent. more by the riders.

4. To obtain an adjustable cut-off, since at the higher speed the engines were intended to run 400 revolutions per minute, it was practically impossible to use any form of trip cut-off. Meyer expansion-valves were used on the backs of the main valves.

Fig: 1. *Fig: 2.*



Scale 1/20th

The engines are exceptionally strong, being all of them designed to work safely with a pressure of 200 lbs. on the square inch, so that the effect of expansion in one cylinder might be compared with compound or triple expansion.

The frames of the engines are of a somewhat novel form, and their purpose may not be immediately apparent. It will be seen, however, that the front cover is cast with a kind of entablature or box, connected with the base-plate by four wrought-iron columns placed symmetrically as regards the piston-rod. The function of these columns is to withstand the vertical forces arising from the steam-pressures on the cylinder covers, and to maintain the axis of the cylinder vertical against any forces; they are not calculated to maintain a horizontal position against lateral forces such as might arise from the action of the slide-block. To meet such lateral forces the base-plate is prolonged upwards in the form of a strong box standard, the upper portion forming the slide-bars, which at the top encircle the piston-rod and pass within, but not touching the box cast on the cylinder cover. Through the sides of this box are four horizontal set-screws, which grip the top of the standard, and so transmit any lateral force directly to the standard, as well as admitting of the adjustment necessary to maintain the cylinder co-axial with the slide-bars.

In this way the vertical forces are taken symmetrically, and cause no distortion of the engine. The cylinder is held very rigidly by the four columns, and the horizontal forces arising from the pressures of steam in the pipe, and particularly from the expansion and contraction of the pipes under a variation of temperature of more than 300°, are taken by the cast-iron standard. And, what led more than anything else to this design, all distortion arising from heat is avoided. The heat-connection between the cylinder cover at 400° is cut, except for the four columns which are heated symmetrically and the four set-pins which conduct very little heat to the slide-bars.

The result appears very satisfactory, the engines running with the slide-bars cool at 400 revolutions per minute, doing 100 H.-P. with great steadiness.

The somewhat peculiar general arrangement of the engines, Figs. 3, 4, seems to require a word of explanation. Vertical engines were adopted on account of the much greater accessibility they afford to all the parts; also because they allow of the water from the steam-jackets being drained back into the boiler with a less difference of level between the floors of the boiler-house and the engine-room.

The crank-shafts of the engines were raised 3 feet above the floor in order to allow of the floor being kept level and to admit of pulleys 5 feet in diameter; also because 3 feet is a convenient height for working the brakes, oiling and adjusting the gearing. The most noticeable feature in the arrangement of the engines—the distance between them—was necessitated by the alternative shaft connections which it was decided to give them, and particularly by the room required for the belt and rope-gearing, and for working the three separate brakes.

Fig: 3.

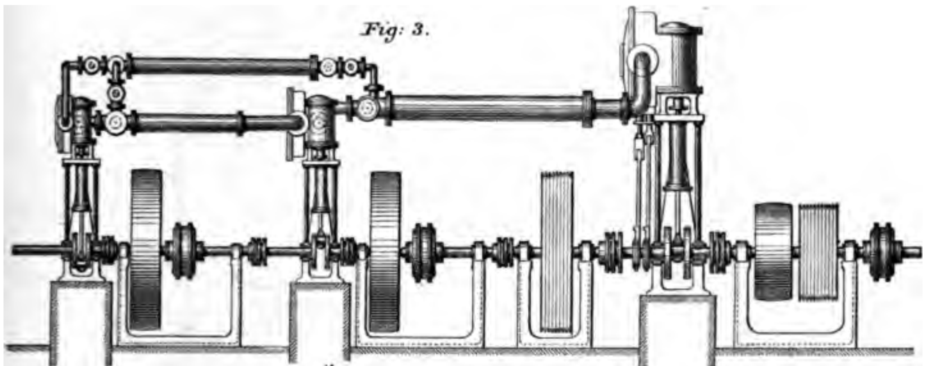
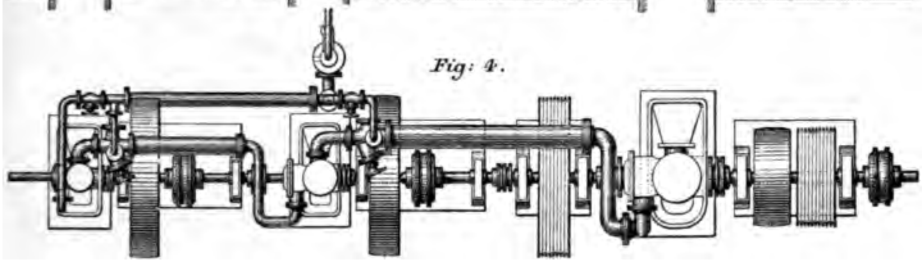


Fig: 4.



The complete shaft consists of seven separate shafts on separate bearings, which can be connected into a single shaft by six special coupling-boxes. The shaft immediately on the right of each engine carries a brake, and these brake-shafts of the two smaller engines carry 11-inch belt-pulleys, 5 feet in diameter, weighing 11 cwt., while the brake-shaft for the low-pressure engine carries two 15-inch pulleys, 3 feet in diameter, weighing 9 cwt., one for a belt and one for ropes. These pulleys act as fly-wheels when the engines are working separately; and, in addition to these, there is between the brake-shaft of the intermediate engine and the crank-shaft of the low-pressure engine an intermediate shaft carrying a 12-inch rope-pulley, 5 feet in diameter, weighing 12 cwt., which may be used as an auxiliary fly-wheel on this engine.

When the crank-shafts are working coupled, as a single shaft, at more than 200 revolutions per minute, these larger wheels must be removed from the shafts.

A first-motion shaft, 16 feet distant and 12 feet high, carries pulleys 3 feet in diameter corresponding to those on the engine-shafts, so that the engines can be geared conjointly or separately on to the first-motion, and this again geared on to one of the brakes, by which means the efficiency of the gearing may well be tested.

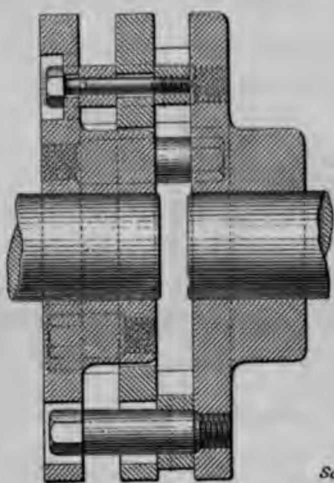


Fig. 5.

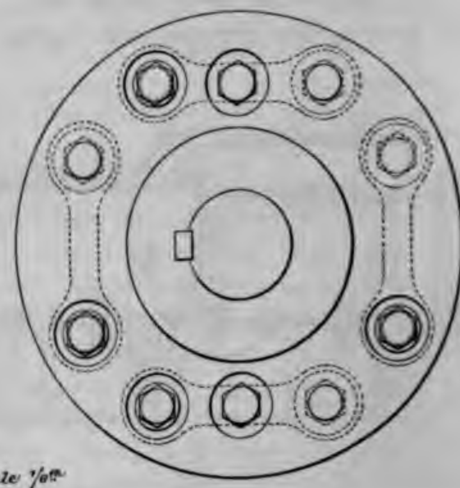


Fig. 6.

The coupling-boxes, Figs. 5 and 6, on the main shaft, are intended to serve two purposes. (1) To afford a ready means of connecting or disconnecting the several shafts. (2) To allow of any side-play which may arise from the proximity and number of the bearings.

To serve these purposes it was necessary to have a special flexible coupling, which led to the design of a modified form of Oldham's coupling, with an intermediate disk, to which the flanges on the shafts are separately connected, each with two parallel drag-links at equal distances on each side of the shaft. The drag-links, which connect one shaft with the disk, being at right angles to those which connect the disk to the other shaft, so that the shafts are perfectly free to play laterally. The links are held by pins screwed into the flanges and disk. To disconnect the shafts all that is necessary is to remove four of these screws and the two links they hold, which leaves the shafts free with a considerable interval between them. These couplings, while very flexible, transmit a perfectly uniform motion and throw no forces on to the bearings.

The intervals between the engines necessitated by this intermediate gearing are, 7 feet between No. I and No. II, and 12 feet between No. II and No. III. These intervals entail no evils in the working of the shaft except the increased friction arising from the additional weight and number of the bearings. This friction may be accurately measured and taken into account in determining the brake H.-P.

The Arrangement of the Intermediate Steam Connections, Figs. 3 and 4. page 341.—This was adopted in order—

(1) To allow of the engines—

Nos. I, II and III being worked as a triple-expansion condensing engine.
„ II and III being worked as a compound condensing engine.
„ I and II „ „ „ non-condensing engine.
„ III „ „ single condensing engine.
„ I or II „ „ „ non-condensing engine.

(2) To secure that the steam-supply to each engine, under whatever circumstances it might be working, should be dry without intermediate drainage, so that the weight of water discharged by the air-pump might measure the steam admitted to each engine as steam.

(3) To bridge over the intervals between the engines without allowing the changes of temperature to cause undue stresses in the pipes and the supports of the engines.

The exhaust-passages from No. I and No. II engines are closed respectively by a 4-inch and a 6-inch steam-valve, while an alternative exhaust-passage, which may be connected directly with an exhaust-pipe in the floor or closed by a blank flange, is provided. The steam-valves in the exhaust-passages open into receivers which supply steam to No. II and No. III engines respectively, which receivers also have alternative connections with the main steam-pipe, so that each engine can have a separate steam-supply.

The jacketed receivers, which are the intermediate steam-passages between the engines, are cast-iron pipes 6 and 8 feet long respectively, lined with wrought-iron pipes 4 inches and 6 inches in diameter, the space between the pipe and casting constituting the space for the steam at boiler-pressure. These receiver pipes are connected with the engines which they supply by S copper pipes of 4 inches and 6 inches diameter respectively, the copper pipes serving as expansion-joints; the expansion in the 12-foot interval between No. II and No. III engines amounting, with 200 lbs. of steam in the jackets, to 0·25 inch.

The arrangement of the steam-pipe which supplies the receivers was adopted in order that the steam might be dry. This pipe leads from a water-separator, as a 2½-inch pipe which enters a jacketed receiver No. I, 4 feet long, lined with a 2½-inch wrought-iron pipe, to prevent condensation of the steam after leaving the separator. The receiver leads to a point near No. I engine, and is connected with a casting in which are two steam-valves opening into 2 inch copper pipes which lead to the steam-chest of No. I and the receiver between No. I and No II. The other end of the receiver is connected through a steam-valve with the receiver between No. II and No. III. In this way, whichever engine is receiving steam from the boiler, the steam has to traverse a steam-jacketed receiver.

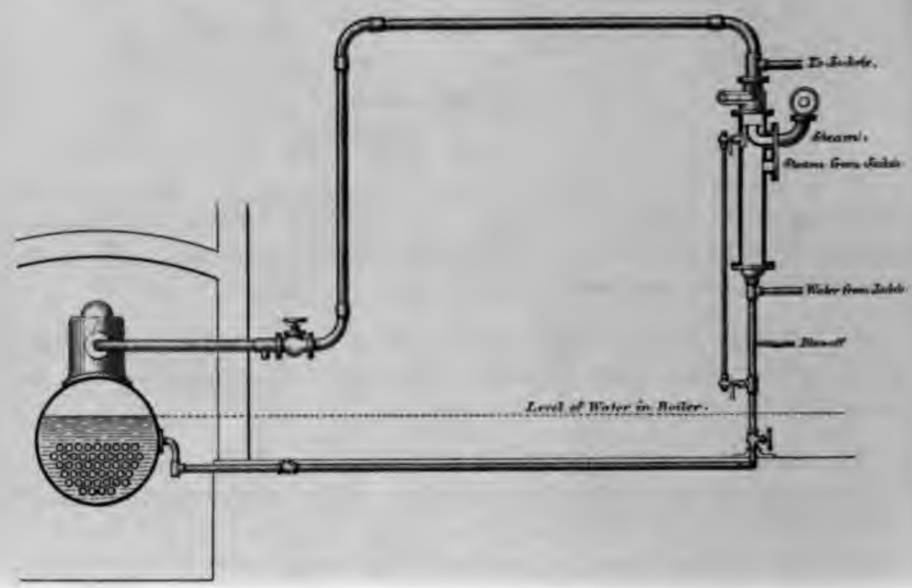


Fig. 7.

The positions of the boiler and engines, Fig. 7, was adopted to allow not only of the water from the jackets on the cylinders, steam-chests, and receivers draining back into the boiler, but also to allow of its doing so when the pressure of the steam in the separator was 3 lbs. per square inch below that in the boiler.

To ensure this, the level of the water in the boiler is kept 6 feet below the lowest jacket to be drained. The boiler-house, which is separated by a glass partition from the engine-room, has a floor 5 feet below the engine-room, and the level of the water in the boiler is 1 foot above the engine-room floor, the boiler being 20 feet distant horizontally from the engines.

The steam-pipe, $2\frac{1}{2}$ inches in diameter, takes the steam from the top of the dome on the boiler and enters the engine-room $2\frac{1}{2}$ feet above the floor; immediately in the engine-room is a steam-valve; 2 feet from the wall the pipe rises vertically 8 feet, then turns horizontally for 10 feet, and then again turns down vertically until it enters the separator. At a height of 10 feet there is a branch 2 inches in diameter, without a valve, which supplies all the jackets with steam at the pressure of the boiler less the resistance of the pipe, which is always less than ½ lb. on the square inch. The main pipe then enters the water-separator through a reducing-valve which lowers the pressure 2 lbs.; below this reducing-valve is the steam-pipe leading to the receivers, and below this again the steam-drain from the jackets enters the separator, and 3 feet below this the water drains from the jackets. The separator now descends as a vertical pipe 1½ inch in diameter to the floor, and then proceeds horizontally until it joins the feed-pipe from the economizer just before entering the boiler, having a back valve and a stop-valve, and also a blow-off valve.

The separator for 3 feet at its upper end consists of a vertical cast-iron cylinder 6 inches in diameter; it is then reduced to a 1½-inch pipe. Communicating with the separator at its top, and at a point 1 foot from the engine-room floor, is a water-gauge of ordinary construction except that the tube is 6 feet long. This gauge shows the level of the water in the separator. When the engines are standing with the blow-off shut, the water remains at the bottom of the gauge. Any water from the jackets drains back into the boiler. If the blow-off is opened the pressure in the separator falls and the water rises to balance the excess of pressure in the boiler, which is shown by the water-gauge; steam is drawn through the jackets as it cannot pass the reducing-valve until the pressure has fallen 2 lbs. below the boiler; in this way the engines are heated.

When the engines are running they draw steam out of the separator below the reducing-valve, and hence all the steam is drawn through the jackets until the resistance in the passages reaches 2 lbs. on the square inch; the water in the gauge shows the level at which it stands in the separator. When the pressure in the separator is 2 lbs. below that of the boiler, the water in the separator stands about 5 feet above the floor, which is just the bottom of the 6-inch cylinder; the water then as it enters the separator gravitates to the boiler. If, however, the stop-valve at the bottom of the separator is closed, the water is collected in the 6-inch cylinder, and, as its level is shown on the gauge, this furnishes a ready means of measuring the condensation from jackets and radiation, which measurements may be checked by draining off the water through the blow-off.

In this way the total condensation from jackets and radiation is determined, and, on consideration, it will appear that herein is an exact measure of all the heat supplied from the boiler over and above that which leaves the engines as steam. It will also be seen that the separator ensures complete water drainage of the jackets and a draught of steam through the jackets and jacket-pipes.

The arrangement of jacket-pipes and drains, which is very complex, was necessary in order that the walls, back and front covers, steam-chest covers, and receiver-covers for each engine might be separately jacketed, and drained both of steam and water. In all there are fifteen separate jackets.

To ensure an equal passage of steam through all these jackets, it would have been desirable, had it been practicable, to supply them in series, so that the steam should pass from one to the other; but this, for obvious reasons, was impracticable, and it was necessary to so arrange the pipes that the head of steam to cause circulation through each jacket should be nearly equal.

This is accomplished by carrying the distributing-pipe, 1½ inch in diameter, throughout the entire length of the engines, as high as practicable. Also the steam-collecting drain, 1½ inch in diameter, and the water-collecting drain, 1 inch in diameter, and arranging them so that there might be a fall all the way in the direction in which the steam was moving. A branch from the steam-pipe with a valve supplies each receiver-jacket on the top, and a drain from the bottom of each receiver-jacket branches into two, one branch falling to the water-drain, and the other rising to the steam-drain, these branches being ⅜-inch and ½-inch in diameter.

Each engine has a branch from the distributing-pipe and from each of the drains, which can be closed by valves. The branches from the two drains unite into one drain before branching to the jackets. Then from the distributing branch on each engine are four branches leading respectively to the four jackets on the engines, and in the same way four drains from the four jackets unite in the one branch from the drain. The jacket-pipes are of copper with iron screwed joints, except the unions, valves, and flanged-joints to the covers, which are of brass. The system is extremely complex, but nothing short of this would suffice for the special purpose of these engines. There are twelve steam-valves, thirty flange connections, and more than forty unions, and about one hundred elbows, tees, and running-joints. The use of running joints was a mistake; they were adopted for simplification, but they should have been unions, it being found very difficult to make the back nuts stand. They were first tried with red-lead and hemp in the ordinary way; this stood a pressure of 200 lbs. per square inch for about two days. The couplings were then faced, and nothing but a little putty was used, but these failed. Then another method was tried which has answered well, and the whole system has been working practically tight.

The Covering of Cylinders, &c.—The temperature of the steam-jackets, about 400° Fahrenheit, rendered the covering of the steam-pipes and cylinders a matter of first importance, not only to prevent loss of heat by radiation, but to render it possible to operate near the engines. In the first instance, the cylinders and receivers were surrounded with 2 inches of glass-wool, and lagged with 2 inches of baywood, but the glass-wool, being found to create gritty dust, was removed, and an inner lagging of soft pine substituted for it. The steam-chest covers and the water-separator were also lagged in the same way; while all the steam-pipes, except the copper expansion-pipes and jacket-pipes, which could not be brought under cover of the wood lagging, have been covered with 2 inches asbestos cement.

The Surface-condenser is of the torpedo-boat type of thin copper, 14 inches in diameter, and 4 feet long. It has about 160 square feet of heating-surface, and receives the steam by an 8-inch exhaust-pipe from the 12-inch engine.

The Air-pump, working by side levers from the slide-block of the 12-inch engine, is 9 inches in diameter, with a 4½-inch stroke, with foot-valve, piston-valve, and cover-valve, and is designed to work up to 400 revolutions per minute.

The condenser and air-pump are conveniently placed on a bracket on the standard of the 12-inch engine, which also forms a stage for indicating the engine. This stage is 5 feet from the floor, which gives sufficient but not too much room for conveniently measuring the water from the hot-well, and the condensing water.

The Feed-pump.—This was adopted in order to maintain a regular feed in the boiler, as well as to enable the water from the hot-well to be returned to the boiler. It is worked from the rocking-shaft of the air-pump levers; it has a plunger 1½ inch in diameter with a 2-inch stroke, and draws water from a feed-tank 3 feet below it, discharging into a feed-pipe, which, together with the economizer or water-heater, leads through 70 feet of 1¼-inch pipe to the boiler. The inertia of this column of water becomes very considerable when the speed is as great as 400 revolutions per minute, and this, together with the 200 lbs. pressure, seemed to render it doubtful whether the pump would answer. However, by means of a special device, a cushion of air or steam was provided about 4 feet from the pump, and by another device the pump was made to start itself, notwithstanding the 3-feet draw, so that the pump works silently and without trouble up to 400 revolutions.

The Governors.—For the special investigations into the action of steam, governors were unnecessary. The load on the engines being constant, the cuts-off fixed, and the supply of steam regular, small variations of speed would be of no moment; while any alteration of the pressures of steam or cut-off by the governors would only confuse the trials; besides which, the problem of governing engines working in conjunction as regards steam, but on separate brakes, was altogether a new one. At the same time, as a matter of safety, the complexity of the system, the number and inexperience of the observers engaged at any time on the engines, the extreme circumstances as regards the steam-pressure and speed under which the engines were designed to work, rendered it imperative that the engines should be so far governed, that under no circumstances could the speeds exceed a safe limit, which, with the 5-foot cast-iron fly-wheels on the shafts, would be about 600 revolutions per minute.

To meet both these considerations, what seemed to be necessary was a safety-governor, which, while it would interfere in no way with the passage of steam at speeds below the limit, would with the utmost certainty cut off steam at some definite speed before the limit was reached.

To ensure certainty of action, it was necessary that the governor should be permanently geared to the engine, and not merely engaged by a belt. And to secure rapidity of action when once the limit of speed was reached, it was desirable that there should be as little room as possible for steam between the governing-valve and the piston; in other words, that the governor should close the expansion-valve.

The Meyer expansion-valves, which had been selected as peculiarly suitable for the purposes of these engines, actuated as they are by screws of such moderate pitch that it requires five or six turns to close the valves, are not susceptible of being opened and closed by the direct force of governor-balls. It therefore became necessary to adopt some form of engagement-governor which, instead of acting on the valve, should act on a clutch which engaged the crank-shaft of the engine with the valve-spindle when the limit of speed was reached. The clutch here adopted is the Author's spiral steel band-clutch. This clutch, which requires almost an insensible force to engage it, is absolutely certain in its hold.

In order to operate on the valve-spindle it was necessary to use two pair of bevel-wheels, which could not be made less than 4 inches and 6 inches in diameter. To throw this train of wheels suddenly into gear with a shaft making 400 revolutions per minute seemed a doubtful proceeding, but such is the softness of action of the clutch, although there is no slipping, that there is neither noise nor shock. The engagement is silent and instantaneous, so that unless special attention is directed to it the movement of the 10-inch hand-wheel will probably escape notice. The clutch is as good in disengagement as in engagement, and will release the shaft before it has turned more than 5° or 10°.

Although the main object of these governors was that of a safety-governor, opportunity was taken to so design them that they should, if required, open the valve as the speed fell, as well as close it as it rose, arrangements being made to prevent hunting. The governors so obtained are extremely efficient, and afford an excellent means of studying the action of governors. During the steam trials, however, they are simply set to act as safety governors, which they have done to perfection, never having been out of action, or having allowed the speed of the engine to exceed the limit to which they are set.

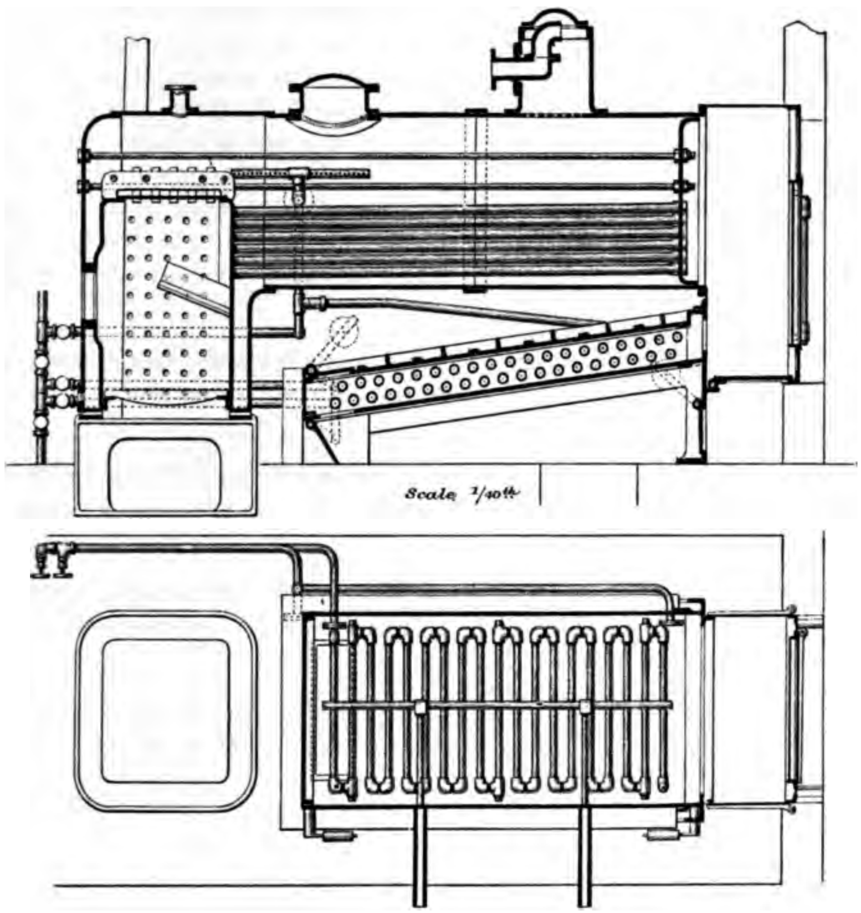


Fig. 8.

The boiler (Fig. 8) is of the locomotive type with iron tubes and fire-box, the shell being of steel $\frac{9}{16}$ inch thick. The tubes are 2 inches in external diameter and 8 feet long, giving 160 square feet of tube surface. The fire-box is $\frac{8}{16}$ inch thick, 2 feet 3 inches by 2 feet 4 inches, 4 feet high, giving 42 square feet of heating-surface.

The area of the grate as used is not more than 4 square feet.

The boiler is furnished with a dome, from the top of which the steam-pipe descends and passes out at the side.

The feed enters the boiler just below the water-level and in front of the fire-box.

There is an iron smoke-box at the end of the boiler from which there are several passages for the gases. The usual passage is beneath the barrel of the boiler, 3 feet broad and 6 inches deep, and about 6 feet long, proceeding at a slight inclination downwards towards the fire-box; across this passage the feed-pipe ranges backwards and forwards, and a series of scrapers are worked to keep the pipes clean. The pipes cross forty times, and give about 50 square feet of heating-surface, 40 square feet of which is kept clean by the scrapers. In this arrangement the water ascends in the opposite direction to that in which the gases descend. The gases, after emerging from the water-heater, descend into a flue leading to the chimney, which is 100 feet high, and takes the gases from other furnaces, affording generally about ⅝ inch draught.

The boiler and water-heater are enclosed in a brick chamber arched over. This chamber is 6 feet wide and 9 feet high, extending from the front of the fire-box to the end of the smoke-box.

At the fire-box end a second chamber is built 6 feet by 6 feet and 8 feet high. This, by shutting a door, becomes a closed stoke-hold, into which a fan can be used to force air at any pressure up to 2 inches of water.

In this chamber is an injector, a feed-tank, and water-supply, a window looking at the safety-valves, and a window into the engine-room, also a tumbling-hopper for admitting coal.

There are two 1-inch dead-weight safety-valves on the boiler, loaded to 200 lbs. on Schaffer and Budenberg's gauges, *i.e.*, 400 inches of mercury, as well as the usual fittings.

THE MEASURING APPLIANCES.

These, in respect of the brake-dynamometers, the indicating gear, the gauge for jacket-water, and the tumbling-bay and tank for the condensing water, are of a permanent character. Provision is also made for measuring the temperature of the gases in the smoke-box as they emerge from the tubes, and in the flue as they leave the water-heater, and for measuring the temperature of the feed before passing the pump, as it enters the boiler after passing the water-heater.

The condensing water is drawn from an iron tank 20 feet by 10 feet by 10 feet, about 116 feet above the engine-room floor. A permanent mercurial gauge in the engine-room always shows the level of water in this tank.

The great head, although, of course, a waste of power, is of advantage in securing regularity of flow. The water after leaving the condenser enters a cast-iron tank, 4 feet by 18 inches by 18 inches, from which it issues over a tumbling-bay 4 inches wide; in the tank are bafflers and a float, with a scale graduated to show in lbs. per minute the quantity running over the bay. The water is then caught in a second receiving tank and conducted to an underground concrete tank 20 by 9 feet by 11 feet, the level of water in which is shown in the engine-room by a water-gauge, and also indicated outside by a float. This tank, which has been accurately measured, affords a very exact means of checking the indications of the float in the tumbling-bay.

The upper tank holds 12,000 gallons of water, which can be passed through the condenser before the tank is empty. When the upper tank is empty, if more water is required the quadruple centrifugal pump is set in motion, which raises the water at the rate of 10,000 gallons an hour from the lower to the upper tank; but it is seldom necessary to resort to this. The temperature of the condensing water is measured by a thermometer in the pipe leading to the condenser, and after leaving the condenser by a thermometer in the float-tank.

The water from the hot-well flows into an oil-separating tank, from which it overflows on opening a cock, and is caught in a 100-lb. tip-can after Mr Bryan Donkin's pattern, from which it may be tipped into the feed-tank, so that the feed and hot-well discharge is measured at one operation.

The condenser is furnished with a mercurial gauge, which shows the absolute pressure in the condenser; also by a Bourdon vacuum-gauge, and the temperature of the discharge from the hot-well is measured by a thermometer in the hot-well. The water, resulting from radiation and jacket condensation, is measured in the water-separator.

The pressures in the receivers are shown by Bourdon gauges, graduated to lbs., which, on the authority of Messrs Schaffer and Budenberg, means 2 inches of mercury—a fact which it is important to know in comparing these pressures with the indicated pressures.

Each engine is provided with a counter for recording the revolutions.

The Indicating Gear (Fig. 1).—The indicator cocks have a clear ½-inch way into the cylinder, the cock being placed at the end of a stiff brass tube screwed horizontally into the cylinder, and reaching through the 4 inches of lagging. The cock itself forms an elbow, to allow the indicator to have a vertical position.

The cocks from the back, and from the front of each cylinder are in the same vertical line, so that the indicators stand vertically over each other in a convenient position to receive the motion for the drums. This is obtained in the 12-inch engine from the air-pump levers, and in the other engines from levers specially connected by a link with the slide-blocks.

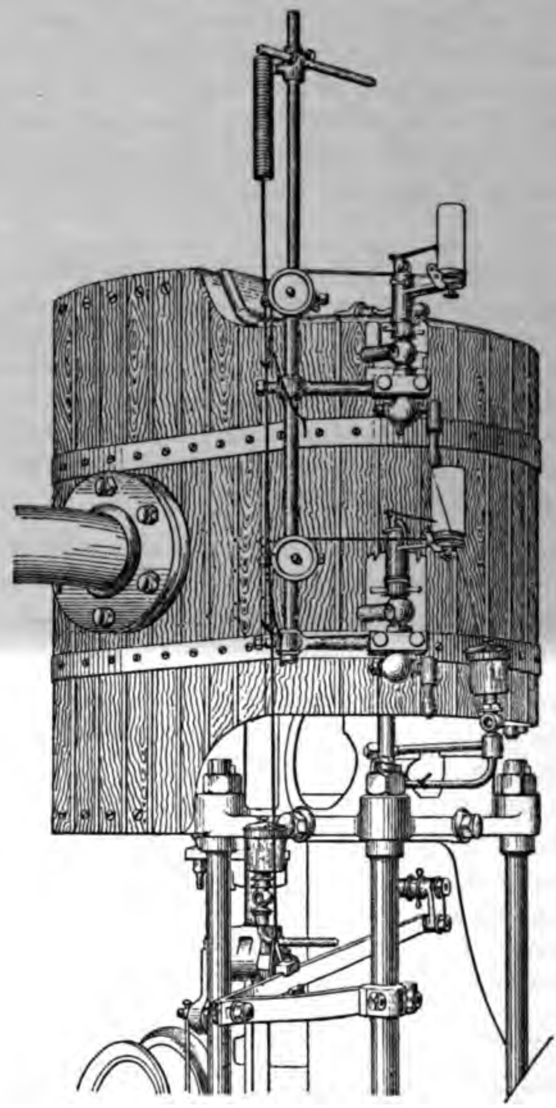


Fig. 9.

In all cases the indicators are some feet above the levers, and while the motion of the levers is vertical, that of the drums is horizontal. The connection of the drum with the lever could be made by a simple cord or wire passing over the roller on the indicator drum down to the lever; but considering that the chief function of the engines was to be regularly indicated, and this by inexperienced hands, and that the speeds would sometimes be such that the ordinary method of hooking up would be impracticable, some more convenient and permanent arrangement seemed desirable. The Author was thus led to a device which, from its simplicity and convenience, particularly in the matter of hooking up, as well as its effect in diminishing errors arising from the stiffness and stretching of the cord, seems likely to be generally useful.

This method consists of a ⅜-inch pin with a head in the side of the lever, a light brass plate ½-inch thick, with a button-hole to permit its passing over the head of the pin, and, when pulled up against the pin, allowing of considerable wear. To this brass is attached a steel wire 19 B.W.G., long enough to reach beyond the furthest indicator, that on the back of the cylinder, the wire being held up by a spiral wire spring of such length and stiffness that it will stretch 6 inches under a force of 25 lbs. without causing undue stress in the wire.

The wire connecting the lever with the spring passes the indicators, and is furnished in convenient positions with two buttons for hooking on the cords of each of the indicators. This is effected by having a light forked hook attached to the end of the cord, which has only to be pulled beyond the button, and one limb of the fork placed on each side of the wire and then let go, when the spring of the drum pulls the hook up against the button. Thus hooking up can be accomplished with facility and certainty at whatever speed the indicator is running. The length of the cord is reduced to a minimum at both ends of the cylinder.

In these engines, where the pistons of the indicators have a motion parallel to that of the pistons of the engines, the cord has to turn a right angle between the drum and the hook. This might be effected by the rollers on the indicator; but as they are usually very small and not adapted for wear, two clips are made to pinch on to the indicator cocks on the cylinder. The clips have circular sockets in line with the motion of the piston of the engine with a set-screw; through these passes a ½-inch steel rod, long enough to carry an adjustable arm to hold the end of the spring, and two adjustable rollers 2 inches in diameter for the cords to pass over.

The Hydraulic Brake Dynamometers (Figs. 10 to 14).—These are a very important feature of the system. They are the result of a special investigation as to the possibilities afforded by hydraulic brakes, undertaken by the Author during the time when the engines were under the consideration of the Committee and before anything was decided.

Having had a great deal of experience with almost every conceivable

form of friction brake, the Author had arrived at the conclusion t
although it is possible to construct such brakes to work with almost

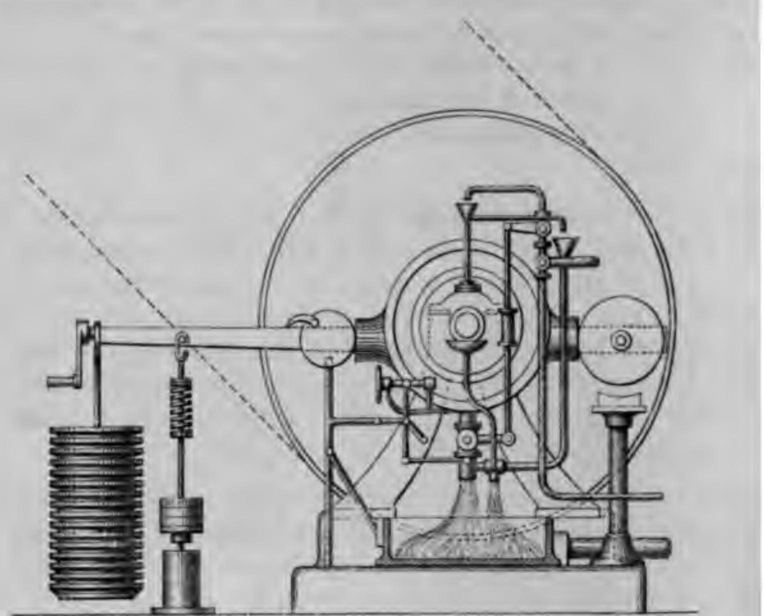


Fig. 10.

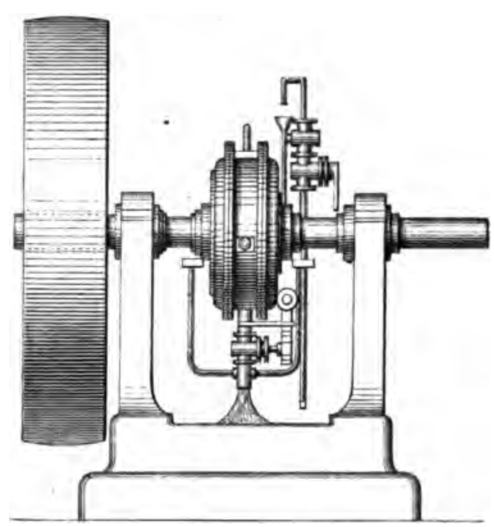


Fig. 11.

degree of accuracy, certain inconveniences and drawbacks attend their use

in all cases leave much to be desired, particularly where, as in a case
ıis, work on the brake is the sole object of the engines.

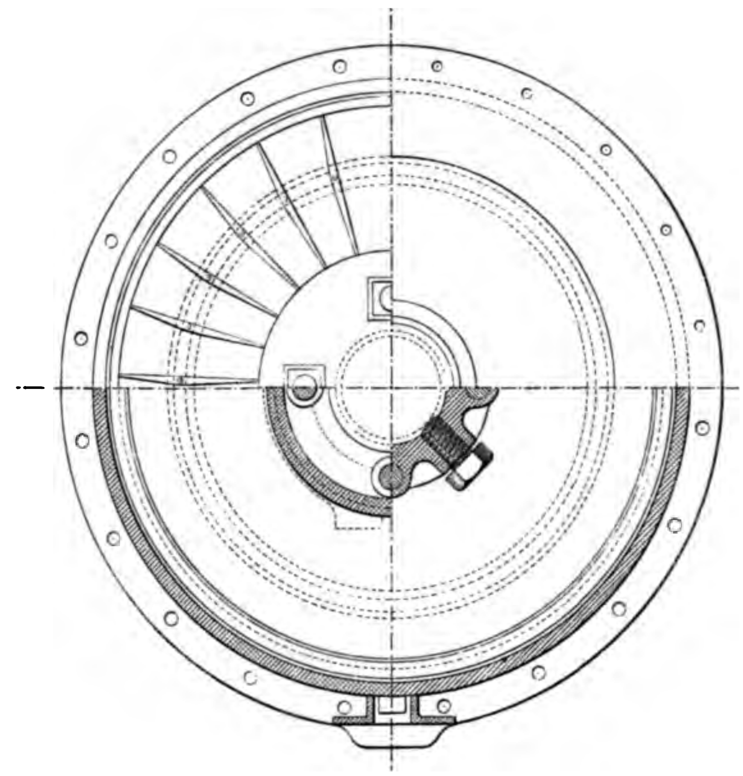


Fig. 12.

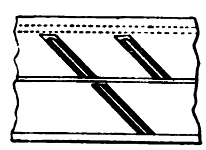


Fig. 13.

Such brakes require constant observation and watching.

A single engine cannot be started without relieving the load.

Such brakes are cumbersome and are not easily adapted to measure
different powers.

Any particular brake cannot without considerable pulling about, such
gether removing the brake and brake-wheel, be rendered altogether
ry. It was desirable :—

1. That the brakes should be certain in their action without any attention while the engines were running.

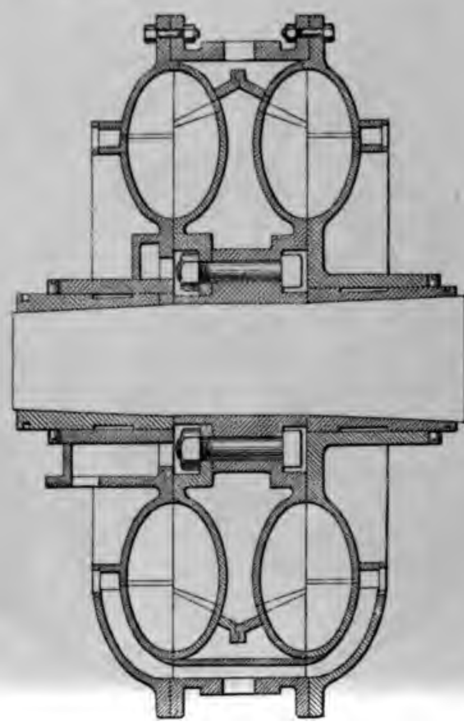


Fig. 14.

2. That they should leave the engines free to start, and then take up their load without attention.

3. That they should be put on and off by a simple operation.

4. That when turned off they should offer no sensible resistance to the engines.

5. That they should be capable of being so adjusted as to impose any particular resistance, from zero to the greatest, at any speed at which it was desired to run the engines.

6. That the resistance of the brake, when once adjusted, should be independent of the speed of the engine.

7. That the necessary size and structure of the brakes should not be such as to incommode or hamper the engines.

8. That the resistance of the brake should admit of absolute determination from a single observation.

Of these attributes 1 and 2 belong to all fluid resistance, such as that of the screws of steam-ships or centrifugal pumps, in which cases the resistance, varying as the square of the speed, is zero when the engines start.

If the casing of a centrifugal pump, or the tank in which a paddle or screw works, be suspended on the crank-shaft, making a complete balance when the shaft is at rest, then, when the shaft is in motion, the moment of resistance on the shaft will be exactly equal to the moment to turn the casing round the shaft. This can be readily and absolutely measured by suspending weights at a definite horizontal distance from the shaft. The first published account of this form of brake having been made use of for dynamometric measurement was by Hirn*, in his investigation for the verification of Joule's mechanical equivalent of heat, and was subsequently adopted by Joule in his second determination.

In neither of these cases, to the Author's knowledge, was there any attempt to vary the resistance at a constant speed.

Having occasion to use a dynamometer for measuring the resistance on the shaft of a multiple steam-turbine at speeds of 12,000 revolutions per minute, which was engaging his attention in 1876, the Author made use of a brake, having a centrifugal pump suspended on the shaft and working into itself. The resistance, or head against which the pump was working, was regulated by a valve between the exit and inlet passages, that is, in the external circuit made by the water. This was brought before the Mechanical Section of the British Association in 1877. At the same meeting, Mr William Froude gave an account of his hydraulic brake, for measuring the power of large engines, in which the resistance was regulated on the same principle as that adopted by the Author, namely, by adjusting diaphragms or sluices in the passages between the revolving wheel and the casing. In other respects Mr Froude's brake differed essentially from any of those previously used, being designed to obtain a maximum resistance with a given sized wheel. For this purpose Mr Froude invented an internal arrangement which affords a resistance out of all comparison with any other form.

Since great resistance, admitting of small brakes, was of extreme importance for these engines, the first step in the special investigation was the construction of a model Froude's brake with a 4-inch wheel; the object of which was to ascertain how far the sluices would act in maintaining a constant resistance at any particular speed, and what was the minimum resistance when the sluices were closed.

With this brake it was found that the minimum resistance was about

* *Théorie mécanique de la Chaleur*, 2nd edition, 1865, p. 65.

0·08 of the maximum; a hardly satisfactory range, considering it was desired to run the engines at a constant load at from 100 to 400 revolutions per minute, the maximum resistance of the brake ranging from 1 to 16, so that the minimum at 400 would be 26 per cent. greater than the maximum at 100 revolutions, apart from the fact that closing the sluices would not render the brake nugatory.

This, however, was of small importance compared with another fact revealed by these experiments. When the speed of the brake-wheel exceeded a certain small limit, determined by the head of water under which it was working, the maximum resistance gradually fell off in a surprising and somewhat irregular manner. This falling off was found to be owing to the brake partially emptying itself of water, due to the air from the water gradually accumulating in the centre of the vortex—a fact which, if not dealt with, threatened to render such brakes useless for the purpose of these engines.

The argument was simple: in a vortex the pressure at the centre is less than the pressure at the outside. The pressure at the outside in these brakes is determined by the atmosphere, and the small head under which they are working; and the outside forms a closed surface. The pressure at the centre will therefore, at different speeds, fall below the pressure of the atmosphere. Air will be drawn from the water and accumulated in the centre, occupying the space of the water and diminishing the resistance; and, owing to various causes, the action will be irregular. This would be prevented if passages could be carried through the outside to the axis of the vortex, carrying a supply of water at or above the pressure of the atmosphere, so as to prevent the pressure at this point falling below that of the atmosphere. This was accomplished by perforating the vanes of the wheel, and supplying water through the perforations. It also appeared that, by having similar perforations in the casing open to the atmosphere, the pressure at the centre of the vortex could be rendered constant, whatever the supply of water and speed of the wheel; so that it would then be possible to run the brake partially full, and regulate the resistance, from nothing to the maximum, without the sluices. These conclusions having been verified on a model, it was decided to arrange the engines with the shafts in line, with three brakes on the shafts; and the brakes, with 18-inch wheels, were designed according to the resistance given by the model. The brakes promised all the attributes desirable, except that of running with a constant load under varying speeds. This matter was considered during their construction, and an automatic arrangement was devised acting on cocks regulating the supply and exit of the water to and from the brake necessary to keep it cool, the lifting of the lever opening the exit and closing the supply, so as to diminish the quantity in the brake, and *vice versâ*.

The danger of such an arrangement hunting was carefully considered, and precautions were taken. The brakes were constructed by Messrs Mather and Platt at the same time with the engines, and the engines started with the brakes and automatic gear complete. During the twelve months they have been running the brakes have demanded and received no attention whatever. They are easily tested for balance. They have neither fixed nor spring attachment, except the bearing on the shaft. They are loaded on a 4-foot lever, with 2-inch play between the stops. When the speed of the engines reaches about 20 revolutions per minute, the levers rise (whatever load they have on), and, though always in slight motion, they do not vary ½-inch until the engines stop; during the run the load on the brakes may be altered at will, without any other adjustment.

THE ENGINE TRIALS.

Before commencing the trials, the object to which they were to be directed, and the manner in which they should be conducted, were carefully considered, and it was decided:—

1. That the purpose of the trials should be the elucidation of the general laws of the action of steam in the steam-engine, and the more general circumstances on which these laws depend.

2. That, from the commencement, the trials should be systematic; certain definite conditions being aimed at, and the trials under each set of conditions continued until consistent results should be obtained, showing how far the conditions had been achieved.

3. That there should be no casual nor unrecorded trials, but that all trials should be considered of the same degree of importance.

4. That observations should be noted and reduced on special forms according to a definite system, to be carefully preserved for future reference; and that a synopsis of the mean results of each trial should be entered forthwith in a special record for ready comparison.

The trials have all so far been conducted as part of the regular work of the laboratory, under the superintendence of the Author, Mr Foster (assistant in the laboratory) having general charge of the appliances, and the fireman (Mr Joseph Hall) firing and driving the engines. The detailed observations were taken and reduced by students (about fourteen on each trial) under the supervision of Mr Mackinnon, demonstrator of the laboratory.

Diagrams are taken every half-hour simultaneously from the six ends by six students, who have charge of their respective indicators for the trial.

The same students also reduce the diagrams in the intervals. The three counters are read every ten minutes by three students, who have respectively charge of the counters and running of the three engines, calculating the brake H.-P. as the trial proceeds, and noting any circumstance connected with the resistance or running of the engine.

One student has charge of the 100 lb. tip-can, which measures the water from the hot-well; and another has charge of the condensing water, noting the temperature and quantity given by the float every ten minutes. Another student measures the rate of discharge from the jackets every half-hour. A student watches the coal-weighing and firing. A student takes the temperatures of the hot-well and feed before and after passing the economizer, and the temperature of the air in the smoke-box and flue before and after passing the economizer. Each student reduces his observations as he proceeds, so that within a few minutes of the end of the trial the reduction is completed.

The results are then examined by Mr Mackinnon, checked and entered in the permanent record, the original diagrams and notes of each trial being carefully preserved.

Two series of trials have been conducted, the one by regular students between 9.30 A.M. and 5.30 P.M. The other by evening students between 6.30 P.M. and 9 P.M., one of each series being made every week.

In the day trials the fire is lighted the first thing in the morning, and steam is got up quietly. As the steam rises it is blown freely through the jackets to heat the engines. If the trial is to be made with jackets, the blowing through all the jackets is continued until the boiler-pressure reaches 200 lbs. on the gauges. Should the trial be without jackets, the jacket-covers on the low-pressure engine are closed when the pressure has reached about 40 lbs., and the air-cock is opened; those on the intermediate cylinder when the pressure reaches about 80 lbs., and those on the high-pressure cylinder at 200 lbs. In all cases the engines are started, and are allowed to run just as required for the trial for one hour. The engines are then stopped fifteen minutes before the trial, the fire is drawn, and the readings of the counters and level of the water in the boiler and tanks are taken; 14 lbs. of wood and 14 lbs. of coal are allowed for the waste of relighting, starting, and stopping. The run then commences; the coal is weighed out in charges of 100 lbs., each charge being shot from the scale-pan into the hopper in the firing-chamber, and completely consumed before the next weighing is admitted.

The boiler is fed continuously by the feed-pump, either from the water from the hot-well or, in some trials, from the water from the condenser.

The runs have generally been for six hours, except when forced draught is used, in which case they are about four hours.

After the last coal has been put on the fire, the engines are run as long as steam can be kept up, care being taken to bring the level of the water in the boiler at stopping exactly to that at starting, any difference being allowed for as 15 lbs. for each $\frac{1}{10}$ inch.

The ashes which fall through the bars are burned during the trial, and the ashes after the trial are generally weighed, but no account is taken of them, nor of any fuel that may be left in the grate.

This was adopted, after trying several systems, as being workable and very definite; nor does it appear, on comparing the results from the long with those of the short trials, that the one has any sensible advantage over the other. During the experiment the regulator is fully open, and a definite quantity of water run through the condenser. The engines, therefore, take all the steam the boilers will produce, the load on the brakes just balancing the pressure of steam, so that the speed is regulated by the rate at which steam is made in the boiler, that is, by the draught-gauge. As it was intended that the scope of these trials should include as far as possible all conditions under which steam may be used, there was no particular reason for commencing with one set of conditions rather than another, except such as arose from convenience, and out of consideration for the engines themselves. The fact that the engines were new, and wanted running to bring the bearings into order, as well as the number of students to be employed, led to the first series of trials being made with triple expansion and full pressures of steam.

THE RESULTS OF THE TRIALS.

The trials commenced in March 1888, and were continued at the rate of two a week till June; in all twenty trials were made and recorded, the engines being then complete with the exception of lagging.

These early trials with 200 lbs. pressure triple expansion, with and without steam-jackets, and various degrees of expansion, gave very definite results. But they also revealed the fact that the linings of the cylinders leaked at pressures above 170 lbs. per square inch, and that the joints in the jacket-pipes could not be made to hold. They also showed that, notwithstanding the precautions taken, the jackets were liable to fall off in efficiency. The effect of the leaks was not great on the general economy of the engines, and might easily have passed unnoticed but for the rigour of the tests to which they were subjected.

At 250 revolutions per minute the thermal efficiency of the engine with jackets was

$$\frac{\text{Heat equivalent of indicated work per minute}}{\text{Heat discharged} + \text{heat equivalent of indicated work}} = 0{\cdot}175$$

Coal per H.-P. .... = 1·48 lb.

The leaks, however, tended to confuse the diagrams, and opportunity was taken of the long vacation, during which the trials were discontinued, to reset the linings of the cylinders. The lagging of the engines was completed as far as it was thought desirable.

The trials were continued in October, when the linings proved to be perfectly tight, and although at first the jacket-pipes leaked occasionally, the leakage was not of any sensible magnitude. The jackets were, however, still found liable to fall off in effect at low speeds. The trial with the jackets was therefore repeated many times, small alterations being made in the jacket-pipes, until consistent results were obtained with speeds of 250 revolutions per minute, giving thermal efficiency, calculated as before, 0·20, coal per indicated H.-P., 1·33 lb. Corresponding trials without the jackets were then made, followed by trials at higher and lower speeds with and without the jackets. These furnish a complete series of trials of triple-expansion engines working with about 200 lbs. boiler pressure, at piston speeds from 250 to 1000 feet per minute.

Appendix, Table I, shows the mean results as recorded for three trials at different speeds with and without jackets. Only one trial at each speed is given, though several trials have been recorded, the results not differing by 1 per cent.

Lines 4 to 29 contain the mean results from the engines.
„ 30 to 42 „ the heat discharged from the engines.
„ 43 to 48 „ „ received by the engines.
„ 49 to 59 „ „ received from the furnace.
„ 60 to 76 „ the general relations between the coal, heat, water and power.

It will be noticed that the three engines do not run at the same speed in the same trial. This is a matter of great importance, and shows the advantage of having for such trials as these the engines working on separate brakes.

The cut-off in each cylinder regulates the fall of pressure in that cylinder, but the pressure in the receiver into which it discharges is determined so as to equalize the steam received, and the steam drains off into the next engine.

If, then, the shafts are coupled, there can be only one ratio of expansion, which will make the terminal pressures in the cylinders correspond with the pressures in the receivers. But when the shafts are free the engines adjust themselves so that they pass the same quantity of steam, and the cuts-off are easily arranged to bring the terminal pressure into accordance with the pressure in the receivers. Thus, with these three separate engines, the full economic advantage of all degrees of expansion can be obtained. To do this with coupled engines would require a different ratio of cylinder volumes for each degree of expansion, these trials showing distinctly what should be the cylinder volume for each degree with coupled engines.

The Checking of the Results.—The system rendered possible by the use of a surface-condenser, of accurately measuring the water which has passed through the engines, as well as the heat discharged from the condenser, and the feed-water, gives a certainty to the results of the trials not otherwise to be obtained. There will be always a loss between the water supplied to the feed-pump and that received by the engines; hence, unless the loss is definitely known, the actual water received by the engines can only be surmised.

In the first forty of these trials the water discharged from the engines, after being measured, has been returned to the boiler, the deficiency being carefully ascertained; and in no case where this has been done has the deficiency amounted to less than ½-lb. per minute, although there were no visible or perceivable leaks of any sort from joints or glands, and the boiler, when tested with water-pressure before and after the experiment, has shown no leak. Great pains have been taken to find where this water went, but without success, though it certainly did not go through the engines.

The importance of this point in determining the action of steam in the cylinder is fundamental. It is only by knowing the quantity of water passing through the engines that it is possible to compare the actual diagrams with a theoretical diagram; and the difference between the feed and the hot-well discharge would in these engines generally amount to from 5 to 10 per cent., and would vitiate any such comparison. As it is, all comparisons have been made from the water discharged from the hot-well.

Since each lb. of dry saturated steam condensed would give up about 1000 thermal-units to the condensing water, the measures of water from the hot-well and heat from the condenser keep a useful running check upon each other. It is found that the heat measured (in 1000 thermal units) is about 4 per cent. greater than the water measured in lbs. when the jackets are on, and from 1 to 2 per cent. less when the jackets are off.

An exact calculation, as to the heat discharged per lb. of water, must involve certain assumptions, of the accuracy of which a careful comparison with the measured heat affords a valuable test. Such a comparison is shown in Appendix, Table II.

For the trials with the jackets on, the calculations are made on the assumption that the steam is released as dry saturated steam, and carries with it into the condenser the heat of evaporation at release pressure from the temperature of the hot-well, less the external work of evaporation and plus the work done by the piston in discharging the exhaust. This expressed in quantities from Professor Rankine's Table is

$$\frac{H_2 - h_3 - (P_2 - P_3) V_2}{772}.$$

In this calculation no account is taken of the additional heat received by the steam, during its passage from the cylinder into the condenser, from the hot walls of the passages.

For the trials without jackets, the calculations are made on the assumption that the steam is admitted into the low-pressure cylinder as dry saturated steam, carrying into the cylinder the total heat of evaporation from the temperature of the condenser at the temperature of admission, and that it carries this heat, less the heat equivalent of the indicated work done in this cylinder per lb. of steam, into the condenser, which, expressed in Professor Rankine's quantities, is

$$\frac{H_1 - h_3}{772} - \frac{(\text{I. H.-P.}) \times 42{\cdot}7}{\text{lbs. per minute from the hot-well}}.$$

This calculation, therefore, takes no account of the heat that must be lost by the steam in supplying the heat to be radiated from the exterior of the cylinder.

Since important actions are not taken into account in these calculations, the resulting quantities cannot be considered an absolute check upon the observed quantities; they constitute, however, a valuable relative check. Thus in Trials 44, 33, 56 (with jackets) the observed discharges of heat are greater than the calculated by amounts which diminish slightly as the speed increases. These differences, about 5 per cent. of the total heat discharged, which will be the subject of further remark, reveal no inconsistency in the observed results, which so far check each other. On the other hand, in the trials 41, 35, 40 (without jackets), while the observed discharges (for trials 35 and 40) are from 1 to 2 per cent. below those calculated, allowing a margin for external radiation, the observed discharge for trial 41 is about 5 per cent. larger than the calculated, an inconsistency which shows error of observation somewhere. Table II does not supply sufficient evidence to locate the error, but this is found in Table I in the quantities given under the head radiation (line 41).

This radiation is obtained as the balance of the total heat received from the boiler (in the water from the hot-well as dry steam, and in the jacket water), and the total heat discharged as heat and work; hence any error in measuring the heat discharged, or the water from the hot-well, would affect the apparent radiation. Since all the trials without jackets are made under approximately the same radiating conditions, and these conditions are such as would cause slightly less radiation than the trials with jackets, the actual radiation in the trials without jackets must have been nearly the same, and somewhat less than in the trials with jackets. In Table I the radiation for trial 41 is 503 thermal units per minute, 897 for 35, 1170 for 40, and 1266 for the trials with jackets, so that the radiation in trial 41 is clearly some 500 thermal units per minute too small. This might be due to an error either in the hot-well discharge or in the heat discharge; but as the former would affect the heat per lb. of coal (line 62), and so bring this trial out of accord with the others, it seems that the error is in the heat discharged.

The correction that would bring the observed heat discharged in Table II, trial 41, into accord with the others is 60 thermal units per lb., or 460 thermal units per minute, which heat, transferred to the radiation, would bring this to 963, or nearly the mean of that for trials 35 and 40. This shows the completeness of the check throughout these results.

The Radiation.—The slight differences which are shown in this quantity, Table I, line 41, for all the trials with jackets, may have been due to differences of temperature in the engine-room. The mean radiation with 200 lbs. steam in the jackets is 1266 thermal units per minute, and the mean radiation in the trials with the cylinder jackets shut off (omitting 41) is 1037, the difference being 229, with or without jackets, at a pressure of 200 lbs. per square inch. This is exclusive of radiation from the boiler.

The Heat Abstracted during Exhaust.—That during the exhaust the water in the cylinder, which has resulted from condensation, is re-evaporated by heat from the walls is well established, and it has been often suggested that the steam leaving the cylinder may be somewhat superheated by the hot walls of the passages. The excess of the observed heat discharged over that calculated in Appendix, Table II, might be explained by the second of these causes, but not by the first, since the diagrams all show that the steam was in the condition of dry saturated steam at release; besides which, the calculated heat takes account of all the heat it could so possess. To account for this difference, which amounts to 5 per cent. of the total heat discharged, by supposing the steam superheated would be to suppose the temperature of the steam raised from 70° to 100° above the temperature of the condenser. Considering that the temperature of the steam in the jackets was 250° higher than that in the condenser, there would be nothing apparently impossible in thus superheating the steam while passing through the ports and exhaust passage heated by the jackets. Such a rise of temperature would, however, be apparent in the exhaust pipe if sought for; and as thermometers showed that the temperature of this did not rise at any time to more than 140° Fahrenheit, which temperature corresponded with the pressure of steam in the condenser, it is evident that this heat did not go to raise the temperature of the effluent steam. The fact that the difference varies so little with the speed of the engine suggests that this absorption of heat is consequent, in some way, on the mechanical action to which the steam is subject during exhaust, in a similar manner to that in which the heat supplied by the jackets to the cylinder is consequent on the expansion, and this appears to be the case.

The steam in the cylinder at release expands down to the pressure of the condenser. The expansion takes place partly in the cylinder, partly in the passages, and will be attended by liquefaction similar to that which results from ordinary expansion. The liquid, thus formed, may be re-evaporated, from the hot walls of the cylinder and the passages, without raising the temperature of the steam above that of the condenser. This expansion is from the volume (per lb.) at release to the volume (per lb.) at the pressure in the condenser, and the amount of heat for re-evaporation can be definitely estimated. In trials 44, 35, 56 respectively, this heat amounts to 84, 87, 71 thermal units per lb. of steam. Some considerable portion of the heat would be supplied from the work done by the steam against the resistance in the passages, which would be directly reconverted into heat: but the greater portion would have to be obtained from the surfaces, or else the steam would enter the exhaust in a supersaturated condition. The excesses of the observed heat over the calculated, Appendix, Table II, are 64, 29, 31 thermal units per lb., being well within the heat necessary to re-evaporate the water, after making allowance for the friction of the passages. This heat, it is to be noticed, is acquired by the steam from the walls after the steam has done its work in the cylinder, and must be supplied either by the jackets or by the condensation in the steam-chest, ports, and cylinder. It therefore represents heat which passes direct through the engine, without effecting any work, and is a loss of between 3 and 6 per cent. of the theoretical efficiency of the steam.

The Diagrams have been taken with six Crosby indicators, and with springs as low as the speeds and pressures would admit.

The reduction is effected by measuring ten breadths, the pressure and back-pressure from the atmospheric line, and then the effective pressure, so that the results check, and may be directly used to obtain a mean diagram. These results have been several times checked by a planimeter, without establishing any sensible difference. As regards the diagrams themselves, every precaution has been taken to ensure accuracy, and there is no reason to suppose that there are any prevailing errors of 1 per cent., although errors of the instruments, and, indeed, of all indicators, when subjected to certain particular tests, are much greater than this. The check afforded by the brake-power, although it would not reveal a prevailing error of 2 or 3 per cent., has this important effect, that it does away with any possible bias that might result from enthusiasm to obtain high indicated power, for by so doing the effect would be to lower the mechanical efficiency of the engine.

It is, however, the consistent agreement of the curves of expansion, as indicated, with the theoretical curve for the weight of absolute steam shown by the water discharged to have passed through the engine, that gives the greatest confidence in the indicated results.

The Reduction of the Diagrams to a mean Compound Diagram.—Considering the important place which must be occupied by mean compound diagrams, in comparing the results of the various trials in such an extended investigation of the steam-engine, it was necessary that some system of reduction should be adhered to, and the choice of this system was a matter of the first importance. There was one peculiarity about the working of these engines which necessitated a departure from any methods previously adopted, namely, the unequal speeds of the three engines. This fact had great influence in determining the system adopted. Except as affected by this, the methods of reduction did not differ from one or other of the plans usually followed.

The reduction of the twenty-four diagrams, taken during a trial from each engine, is effected by finding the means of each of the twenty measured distances from the atmospheric line, which are then reduced to a common scale, 10 lbs. to an inch. These ordinates are then plotted, so as to project the diagram to a length determined, as will be subsequently described. The volumes of clearance, 4 per cent. on engine I, and 6 per cent. on engines II and III, valve-clearance 1·65 per cent. on engine I, and 2·5 on engines II and III, are then added to obtain the line of zero volume. Thus, a compound diagram is obtained showing the relation of volumes and pressures of the whole steam in each of the cylinders. To reduce this diagram, to show the relation of volume and of pressure of the steam discharged from the cylinder, an ideal compression-line is drawn through the point of the actual compression-curve which corresponds to the closing of the exhaust. Horizontal lines are next drawn across the diagram, cutting the expansion-curve, the compression-line, and the ideal line, and each of these horizontal lines is set back until the point which was the ideal compression-curve reaches the line of zero volume. Then the positions taken by the points from the expansion-line and the actual compression-line show the volume of steam in the cylinder over and above the volume of that which is shut in at exhaust. All this reduction may be done arithmetically, or by plotting. The result is that, while the area of the diagram has not been altered, the actual expansion and compression-line for the steam passing through the engine is obtained; Rankine's curve of saturation for the weight of steam discharged is then drawn. On account of the varying difference between the speeds of these engines, the lengths for the compound diagram could not be obtained by simply projecting the lengths of the separate diagrams, so that they should be proportional to the effective volumes of the several cylinders. It was necessary to project them so that they should be proportional to the products of the effective volumes of each engine multiplied by its revolutions per minute. Slight as this necessary modification may appear, it does away with the idea of a relation between the area of a diagram and the size of the engine, which, once got rid of, leaves it apparent that the separate diagrams express nothing but the relation which holds between the pressures and volumes of a certain quantity of steam, which quantity may be changed by altering the scale of length of the diagrams. Having once realized this, the advantage becomes apparent, in instituting comparisons between a number of engine trials, of taking the common scale of length for the diagrams to be such that they all express the relation between the volume and the pressure of the common unit (1 lb.) adopted for the weight of steam. This common scale is readily obtained by dividing the product of effective volumes, multiplied by revolutions, by the weight of steam passing through the engines per minute, and taking the result as the length of the diagram in any uniform scale; $\frac{1}{2}$-inch to the cubic foot has been that adopted for the first reduction in these trials, the pressures being plotted to 10 lbs. to an inch.

The diagrams, Fig. 15, p. 370, are such mean diagrams, showing the lbs. per square inch pressure and cubic feet volume for each lb. of steam passing through the engines, also Rankine's curve for saturated steam to the same scale. In these diagrams:—

The extreme length of the diagram = {the effective volume swept by the piston for each lb. of steam through the engines.

{The distance from the line of zero volume to the expansion or compression-curve at any particular pressure ....} = {the volume of the steam in the cylinder at that pressure, less the steam shut in at compression per lb. of steam through the engine.

The area enclosed in the diagram = effective work per lb. of steam.

The distance to the right between the compression-line and that of no volume measures .... = the volume of initial steam per lb. of steam rendered non-effective by clearance.

The distance between the expansion-line and the saturation-curve .... = the volume of steam per lb. of steam through the engines absent on account of condensation, priming and leakage.

The ratio of the horizontal distances from the line of zero volume to the curve at cut-off and release .... = the effective ratio of expansion.

The clearness and simplicity of the comparison which these diagrams institute between the areas actually occupied, and those which would have been occupied had the steam been saturated, renders it possible, as well as desirable, to state exactly in what relation the areas stand as regards the theory and economy of the engine.

The area enclosed between the limits of pressure and volume by the line of zero volume, the line of condenser pressure, and the saturated curve, expresses in foot-lbs., the greatest possible amount of heat that can be converted into work, through the agency of 1 lb. of steam maintained in a state of saturation between these limits. The areas included in the measured diagrams represent the heat which has been so converted by the agency of each lb. through the engines, and the various intervening areas represent loss in conversion.

These are facts which it is important to bear in mind in dealing with jacketed engines, in which 1 lb. of steam through the engines does not represent a certain quantity of heat, which will be the same whether it is realized or not. For such engines it is impossible to make the diagrams represent the comparative efficiencies actual and theoretical. With unjacketed engines, the case is different, as the lb. of steam represents, at a particular pressure, a definite quantity of heat through the engine, however much of it is converted, and if a special adiabatic line be substituted for the saturated line, the relation of areas will be the relation of efficiencies. In the present case, however, it seemed better to treat all the diagrams in the same way, and to make a separate comparison of the efficiencies with the highest theoretical efficiency between the same limits. With the unjacketed as well as with the jacketed trials, the theoretical efficiency has been calculated as for saturated steam. This comparison for all the trials is given in Appendix, Table III.

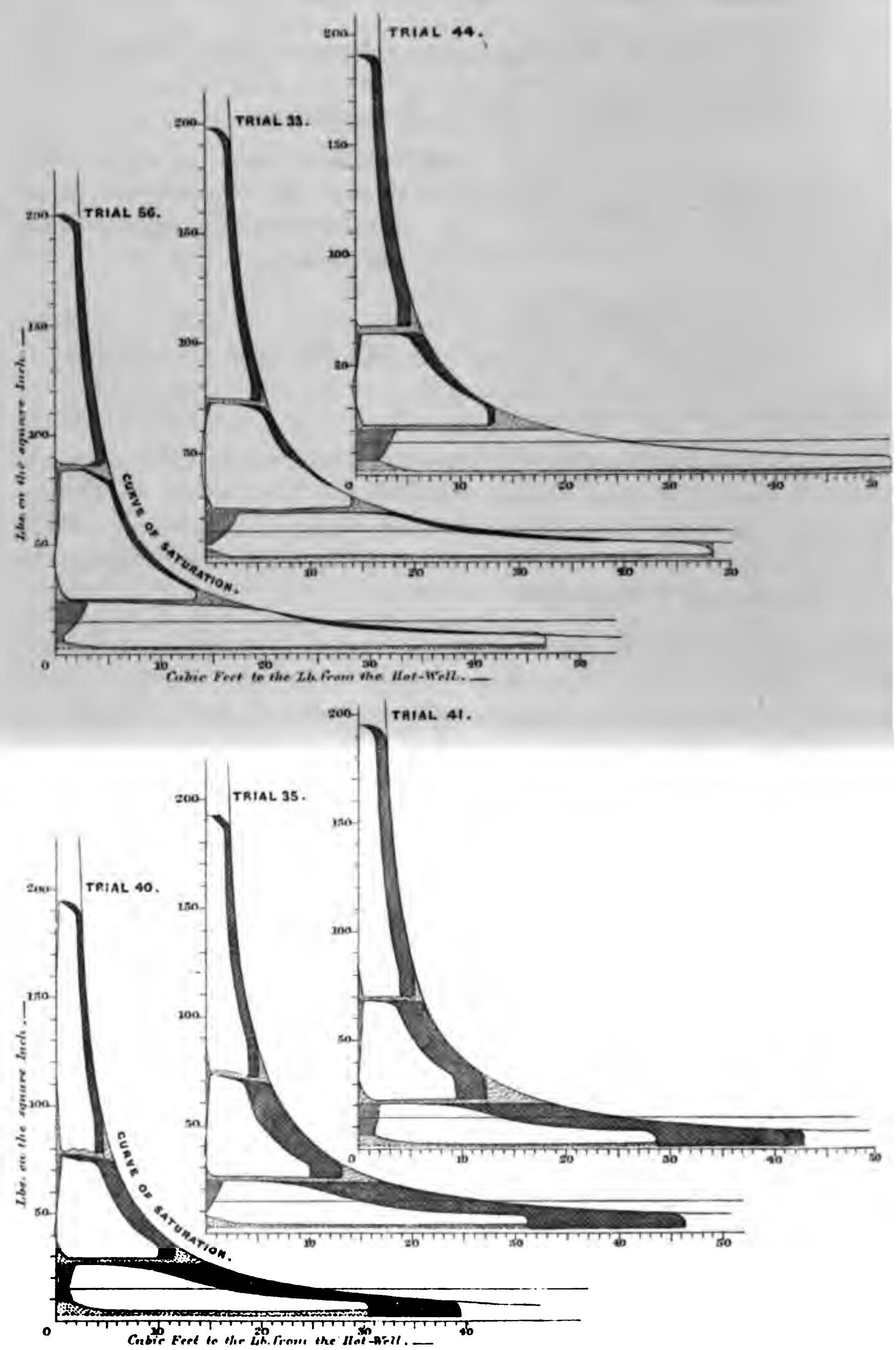

TRIAL 44.
TRIAL 33.
TRIAL 56.
CURVE OF SATURATION.
Lbs. on the square Inch.
Cubic Feet to the Lb. from the Hot-Well.
TRIAL 41.
TRIAL 35.
TRIAL 40.
CURVE OF SATURATION.
Lbs. on the square Inch.
Cubic Feet to the Lb. from the Hot-Well.

Fig. 15.

The Condensation, Priming and Leakage of Steam in the Cylinders, as shown in the Diagrams.

There are two quantities which it is almost impossible to separate by the inherent evidence of the diagrams.

The missing quantity, to use Mr Willans' expression, which is here shown by the horizontal breadth of the black band, may equally well arise from the steam having escaped by the piston, or having been temporarily converted into water.

This much, however, is evident from the diagrams, that with steam in the jackets, in whatever manner the steam has vanished in the high-pressure engine, it has all reappeared before the end of the stroke in the intermediate engine, and though some of it has disappeared at the cut-off in the low-pressure cylinder, it has reappeared again before the end of the stroke. Hence it seems that there is no escape of steam by the pistons of these two engines.

The question remains, however, as to whether steam has not escaped by the pistons of the high-pressure engine, and through the valves, during expansion into the cylinders of the intermediate and low-pressure engines.

Certain differences in the diagrams taken from No. II engine, when working with different cuts-off, suggested that the rider valves were held somewhat off the back of the main valve by the spindle, so that they leaked steam until the pressure in the cylinder was sufficiently lower than that in the steam-chest to spring the spindle and force the valves home. It became particularly evident in the fifty-fifth trial, and then the cover was removed and the conclusion verified. This source of error was put right, and the fifty-sixth trial, as compared with the earlier ones, shows what has been the effect of leakage in these, namely, the breadth of the black band towards the tops of the diagram from No. II engine.

When the covers were last put on, in August, 1888, the cylinders and valve-faces were all in equally good condition, and there has been no leak from the jackets, while the engines were standing with full pressure in the jackets. The regulators opening into the intermediate receivers were made tight in August, 1888, and were not again opened till after the forty-sixth trial. There was then occasion to open them, and as the engines were standing preparatory to starting the fifty-sixth trial, it was seen that steam was leaking into No. II receiver, probably about $\frac{1}{2}$-lb. per minute; as the valve was found to be shut, there was nothing to be done, so the trial was run; and, as was to be expected, the diagrams from No. I engine show what, considering the circumstances, is an unusually large black band.

In the absence of definite evidence of leakage, the Author concludes that the missing quantity shown by the black band is everywhere due to condensation.

It is not the intention in this Paper to endeavour to establish a complete theory of cylinder-condensation. Though it may be well to state that, before designing the engines, the theory was carefully considered and formulated, leaving only the arbitrary constants to be determined from the experiments. For anything like a complete determination of these constants, the experiments have not sufficiently advanced; but this is not necessary to show that in the case of a series of cylinders, all jacketed up to boiler-pressure, the law of condensation would be precisely that which is shown in the diagrams.

Whenever the bounding surfaces are colder than the steam adjacent to them condensation occurs. To prevent condensation it is therefore necessary to maintain all parts of the cylinder surfaces, and port passage surfaces, at a temperature at least as high as that of the initial steam.

To do this, in the case of expansion, it is not sufficient (as seems to be commonly assumed) to keep the outside of the metal constituting the walls and covers, merely at the temperature of the initial steam. That, of course, would be sufficient if there were no condensation other than what results from the temperature of the surfaces.

Forty years ago no such other cause of condensation was known. It was revealed, however, by the discoverers, Rankine and Clausius, in 1849, that the expansion of steam reduces its temperature below that corresponding to saturation unless some of the steam is condensed. The manner of action of this supersaturation, caused by expansion, in absorbing heat from the walls of the cylinder maintained at a higher temperature than the steam, does not appear to have been yet ascertained with any degree of certainty; but it is certain that steam in this state of supersaturation does absorb heat with immense rapidity, when the walls are at a higher temperature than the expanded steam. Also the amount of heat necessary to prevent supersaturation is definitely known, though it is, perhaps, well to recall the fact that it is not, even approximately, the heat equivalent of the work done by the steam during expansion.

If the walls of the cylinders are maintained at the temperature of the initial steam, the expanding steam will absorb heat. This heat must pass through the walls; and as heat only flows through metal down the gradient of temperature, the temperature on the outside must be greater than that on the inside. Hence it follows that either the steam in the jackets must be hotter than the initial temperature of the steam in the cylinder, or the mean temperature of the internal surface of the cylinder will be below that of the initial steam, in which case there will be cylinder-condensation.

How important this degradation of temperature through the walls is, will, perhaps, best be rendered apparent by stating an actual case.

In expanding 1 lb. of steam from a pressure of 203 lbs. to a pressure of 79·3 lbs., the heat per lb. necessary to prevent supersaturation is

55·1 T.U.

or about 5 per cent. of the total heat in the initial steam In a cylinder passing 600 lbs. of steam per hour, to prevent supersaturation there should pass through the walls of the cylinder

33,060 T.U.

Now the jacketed surface of the cylinder of the H.-P. engine is less than 1·5 square foot, and the thickness of the metal is more than 0·4 inch. Hence the heat would have to flow through this thickness of metal at a rate of

22,000 T.U. per square foot per hour.

From the known laws of conductivity of iron, this would require a difference of temperature of 38° Fahrenheit.

Thus it appears that, to prevent supersaturation, the temperature of the steam in the jackets of No. I engine must be 38° higher than the mean temperature of the internal steam; or, in other words, that the mean temperature of the internal surfaces will be 38° lower than that of the initial steam, which is at the same temperature as that in the jackets.

What amount of surface-condensation this difference of temperature would cause may be, to some extent, inferred by comparison with the difference between the mean temperature of the surfaces and that of the initial steam when the jackets are empty. Here the initial temperature is about 383°, and that of the exhaust, 302°; the mean, 342°; difference of mean and initial, 41°; so that in this engine the mean temperature of the walls would only be affected to the extent of about 3° Fahrenheit by the jackets, supposing the whole of the heat to prevent supersaturation were supplied by the jackets. But this would not be quite the case, as some heat is obtained from the difference in the heat given up and absorbed by the cylinder-condensation; and there is no proof that the steam may not be discharged with a certain degree of supersaturation.

However, the reasoning leads to the conclusion that, with steam at initial pressure in them, the jackets would produce a comparatively small difference on the cylinder-condensation in this engine when passing 10 lbs. of steam per minute.

In No. II, the intermediate engine, the case is different. Here the heat which has to flow into the cylinder through the walls is nearly the same as in

APPENDIX.

TABLE I.—MEAN RESULTS OF TRIPLE-EXPANSION TRIALS ON SEPARATE BRAKES.

	State of the steam-jackets	Cylinder jackets at boiler pressure Receiver „ „ „			Cylinder jackets empty. Receiver „ at boiler-pressure.		
1	Number of the trial	44	33	56	41	35	40
2	Date of the trial	Feb. 12, '89	Dec. 4, '88	April 2, '89	Jan. 29, '89	Dec. 11, '88	Jan. 22, '89
3	Time of trial	9.52 to 3.51 P.M.	9.45 to 4 P.M.	9.57 to 2 P.M.	9.54 to 3.55 P.M.	9.54 to 4.6 P.M.	10.32 to 2.37 P.M.
4	Lbs. on the square inch mean absolute pressure in the boiler	200·0	201·0	207·0	205·0	206·0	203·0
5	„ „ „ „ „ „ receiver No. I	199·0	198·0	203·0	204·0	205·0	201·0
6	„ „ „ „ „ „ „ No. II	65·0	74·0	85·0	67·0	73·0	78·0
7	„ „ „ „ „ „ „ No. III	21·8	22·7	22·6	21·7	23·1	25·6
8	„ „ „ „ „ „ condenser	1·5	1·7	2·2	1·3	1·8	2·5
9	Lbs. on the sq. inch mean effective pressure indicated in engine No. I	73·7	70·53	71·1	72·61	73·8	75·56
10	„ „ „ „ „ „ „ „ No. II	29·12	28·99	33·27	30·66	28·2	29·81
11	„ „ „ „ „ „ „ „ No. III	12·02	11·71	12·52	12·15	11·19	11·61
12	Revolutions per minute of engine No. I	115·0	206·0	230·5	146·0	229·0	322·0
13	„ „ „ No. II	135·0	241·0	298·0	127·0	215·0	320·0
14	„ „ „ No. III	152·0	249·0	299·0	109·0	184·0	276·0
15	I.H.-P. engine No. I	8·06	13·82	15·6	10·07	16·11	23·11
16	„ „ No. II	9·85	17·54	24·8	9·73	15·17	23·86
17	„ „ No. III	15·32	24·4	31·7	11·12	17·23	26·90
18	Lbs. × feet load on brake No. I	300·0	300·0	300·0	300·0	300·0	300·0
19	„ „ „ „ No. II	320·0	320·0	320·0	320·0	320·0	320·0
20	„ „ „ „ No. III	400·0	400·0	420·0	400·0	400·0	400·0
21	Brake H.-P. engine No. I with intermediate shaft	6·56	11·74	13·13	8·32	13·05	18·35
22	„ „ No. II „ „	8·21	14·65	18·12	7·72	13·07	19·46
23	„ „ No. III „ „	11·55	18·93	23·9	8·29	13·98	21·00

24	Mechanical efficiency of engine No. I with intermediate shaft	0·812	0·848	0·844	0·825	0·810	0·794
25	„ „ „ No. II „ „	0·835	0·835	0·73	0·793	0·861	0·815
26	„ „ „ No. III „ „	0·753	0·775	0·754	0·75	0·81	0·780
27	Total I.H.-P.	33·23	55·76	72·1	30·92	48·51	73·87
28	Total B.H.-P.	26·32	45·32	55·15	24·33	40·1	58·81
29	Mechanical efficiency of engines I, II, III with intermediate shafts	0·792	0·813	0·765	0·79	0·826	0·80
30	Lbs. per minute of water passing through the condenser	209·5	434·0	453·1	264·0	337·5	403·0
31	Degrees Fahrenheit initial temperature „ „	49·9	68·0	55·45	51·83	64·0	52·26
32	„ „ final temperature „ „	79·83	91·03	83·87	83·03	98·03	94·94
33	T.U. per minute taken up by „ „ „	6,271·0	9,994·0	12,862·0	8,236·0	11,484·0	17,200·0
34	T.U. discharged as I.H.-P. per minute	1,421·0	2,384·0	3,085·0	1,325·0	2,074·0	3,158·2
35	T.U. per minute discharged as I.H.-P. and in condensing water	7,692·0	12,378·0	15,947·0	9,561·0	13,558·0	20,358·0
36	Thermal efficiency as given by heat discharged in condensing water	0·185	0·192	0·194	0·141	0·153	0·155
37	Lbs. water discharged from hot-well per minute	5·83	9·58	12·34	7·73	11·47	17·58
38	Degrees Fahrenheit temperature in the hot-well	102·0	114·0	112·7	108·0	114·0	132·5
39	„ „ „ „ „ above temperature of feed	22·0	0·0	28·9	25·0	0·0	0·0
40	T.U. per minute discharged from the hot-well	128·0	0·0	356·0	200·0	0·0	0·0
41	„ „ „ by radiation	1,227·0	1,394·0	1,176·0	503·0	897·0	1,170·0
42	„ total per minute discharged from engines	9,047·0	13,772·0	17,479·0	10,264·0	14,455·0	21,528·0
43	T.U. necessary to evaporate 1 lb. of feed to economizer	1,151·0	1,117·0	1,147·0	1,148·0	1,117·0	1,098·0
44	„ per minute received as dry steam (=lbs. discharged from hot-well)	6,710·0	10,700·0	14,154·0	8,874·0	12,812·0	19,303·0
45	„ received from each lb. of water condensed at boiler-pressure	844·0	844·0	842·0	843·0	843·0	843·0
46	Lbs. per minute of water from the jackets, &c.	2·77	3·64	3·95	1·65	1·95	2·64
47	T.U. per minute received from the water from the jackets, &c.	2,337·0	3,072·0	3,325·0	1,390·0	1,643·0	2,225·0
48	Total T.U. received per minute by the engines	9,047·0	13,772·0	17,479·0	10,264·0	14,455·0	21,528·0
49	Degrees Fahrenheit temperature of feed to the economizer	80·0	114·0	83·8	83·08	114·0	132·5
50	Degrees added to temperature of feed in economizer	95·5	83·8	130·7	98·5	76·1	107·7
51	Lbs. per hour of water discharged from the hot-well	349·8	574·8	740·4	463·8	688·2	1,054·8
52	T.U „ received by water „ „ in the economizer	33,500·0	48,200·0	96,800·0	45,700·0	52,500·0	113,600·0

TABLE I.—*continued.*

	State of the steam-jackets	Cylinder jackets at boiler-pressure Receiver " " "			Cylinder jackets empty Receiver " at boiler-pressure.		
	Number of the trial	44	33	56	41	35	40
	Date of the trial	Feb. 12, '89	Dec. 4, '88	April 2, '89	Jan. 29, '89	Dec. 11, '88	Jan. 22, '89
	Time of trial	9.52 to 3.51 P.M.	9.45 to 4 P.M.	9.57 to 2 P.M.	9.54 to 3.55 P.M.	9.54 to 4.6 P.M.	10.32 to 2.37 P.M.
53	Lbs. jacket-water per hour returned to the feed-pipe into the boiler	166·2	218·4	237·0	99·0	117·0	158·4
54	Degrees Fahrenheit temperature of the boiler	383·0	383·0	383·0	383·0	383·0	383·0
55	Degrees mean temperature observed after mixture with feed	244·0	250·0	257·0	218·0	219·0	260·0
56	Lbs. per hour of mixed feed to boiler	516·2	793·2	977·4	562·8	805·2	1,213·2
57	T.U. to evaporate 1 lb. of mixed feed	987·0	981·0	974·0	1,013·0	1,012·0	971·0
58	" per hour taken up in the boiler	509,300·0	778,100·0	951,900·0	570,100·0	814,800·0	1,178,000·0
59	T.U. per hour received from the furnace (exclusive of steam lost)	542,800·0	826,300·0	1,048,700·0	615,800·0	867,300·0	1,291,600·0
60	T.U. per lb. of coal taken up in the economizer	670·0	646·0	1,006·0	898·0	651·0	950·0
61	" " " " " boiler	10,186·0	10,436·0	9,895·0	9,961·0	10,103·0	9,850·0
62	" " " " total	10,856·0	11,082·0	10,901·0	10,759·0	10,754·0	10,800·0
63	T.U. per I.H.-P. per hour for radiation	2,216·0	1,500·0	978·0	976·0	1,110·0	950·0
64	" " " for the engines	14,120·0	13,320·0	13,567·0	18,969·0	16,768·0	16,535·0
65	" " " total	16,336·0	14,820·0	14,545·0	19,945·0	17,878·0	17,485·0
66	Lbs. per I.H.-P. per hour of feed-water to supply as dry steam the heat for radiation	2·0	1·34	0·84	0·84	1·0	0·86
67	" the engine	12·2	11·92	11·83	16·45	15·0	15·04
70	" total	14·2	13·26	12·68	17·3	16·0	15·9
71	Lbs. of coal per hour (Nixon's Navigation mixture) for radiation	7·0	7·5	6·4	2·8	5·0	6·4
72	" working the engines	43·0	67·06	89·8	54·44	75·65	113·2
73	" total	50·0	74·56	96·2	57·24	80·65	119·6
74	Lbs. of coal per I.H.-P. per hour for radiation	0·21	0·13	0·09	0·09	0·10	0·09
75	" " " " working the engines	1·29	1·24	1·24	1·76	1·56	1·53
76	" " " " total	1·50	1·33	1·33	1·85	1·66	1·62

TABLE II.

		Jackets at Boiler-Pressure			Jackets Empty		
Number of the trial............		44	33	56	41	35	40
Thermal units from the condenser per lb. of water from the hot-well...	Calculated	1,011	1,014	1,011	1,014	1,009	990
	Measured	1,075	1,043	1,042	1,065	1,001	978
	Differences	−64	−29	−31	−51	8	12

TABLE III.—RELATIVE AREAS OF DIAGRAMS PER LB. OF STEAM THROUGH THE ENGINES, AND THERMAL EFFICIENCIES OF ENGINES.

	Number of trial	44	33	56	41	35	40
1	Theoretical area, ft. & lb.	238,645	233,545	228,420	235,500	233,000	221,860
2	Measured area „	188,096	192,067	192,000	127,545	139,546	144,350
3	Percentage of theoretical area	79·0	82·0	84·0	54·0	60·0	65·0
4							
5							
6							
7							
8	Theoretical efficiency, p.c.	23·3	23·2	22·7	23·3	23·2	22·4
9	Measured efficiency, p.c.	18·5	19·2	19·4	13·8	15·3	15·5
10	Percentage of theoretical efficiency	79·4	82·6	85·4	59·2	65·9	69·4

TABLE IV.—CONDENSATION WITHOUT JACKETS.

	Number of the Trial	Revolutions per Minute	Ratio of Expansion	Proportion of Total Steam condensed at		
				Cut-off	Mid-Stroke	Release
Engine No. I ...	41	146	2·7	0·40	0·39	0·30
	35	229	2·3	0·29	0·27	0·22
	40	322	2·0	0·22	0·21	0·17
Engine No. II...	41	127	2·4	0·41	0·345	0·29
	35	215	2·4	0·38	0·34	0·26
	40	320	2·2	0·30	0·27	0·14
Engine No. III...	41	109	2·7	0·51	0·48	0·37
	35	184	3·05	0·48	0·47	0·33
	40	276	2·6	0·32	0·36	0·23

57.

REPORT OF THE COMMITTEE APPOINTED TO INVESTIGATE THE ACTION OF WAVES AND CURRENTS ON THE BEDS AND FORESHORES OF ESTUARIES BY MEANS OF WORKING MODELS.

[*From the* "British Association Report," 1889.]

THE Committee held its first meeting in the Central Institution of the City and Guilds of London Institute. It was then resolved that the Committee should avail itself of the permission of the Council of the Owens College, and conduct its experiments in the Whitworth Engineering Laboratory.

At the suggestion of Prof. Reynolds it was arranged that the first experiments should be directed to determine in what respects, and to what extent, the distribution of sand in the beds of model estuaries of similar lateral configuration is affected by the horizontal and vertical dimensions, and the relation which these bear to one another and to the tide period so as to place the laws of similarity on which the practical applications of the method depend, on as firm an experimental basis as possible.

It was suggested provisionally that two working tanks should be constructed, one as large as the circumstances of the laboratory would admit, and one of half the linear dimensions of the larger tank. Prof. Reynolds was empowered to appoint an assistant to make the necessary observations.

At a second Committee, held at Owens College, Manchester, the models constructed were examined, and it was arranged that Prof. Reynolds should draw up a report on the results so far obtained.

On Model Estuaries. By Professor Osborne Reynolds, F.R.S.

Having carefully considered and sketched out designs for the tanks and appliances in accordance with the resolutions of the Committee on February 6, I obtained the assistance of Mr H. Bamford, B.Sc., from Easter up to the date of the meeting at Newcastle. The working drawings for the appliances were commenced immediately after Easter, and the work put in hand, the experiments being commenced in each tank as soon as it was ready.

The General Design of the Appliances.—A great object in designing the tanks was to make the most of the facilities in the Whitworth Engineering Laboratory, Owens College, in respect of a continuously running shaft, a supply of town's water and wastes, also a water supply (13,000 gallons) from a storage tank at 116 feet above the floor of the laboratory, and a discharge into a similar tank below the floor, with pumping power to raise the water back when required, also floor space.

The available floor space, although very conveniently placed with respect to the water and power, was strictly limited by resisting structures to 10 feet wide and 22 feet long. This admitted of an extreme length for the larger tank of 16 feet, and an extreme width of 4′ 8″, leaving 2′ 6″ for the width of the smaller tank, the remainder of the space being the least possible that would admit of access to all parts of the tanks. The internal dimensions of the tanks as designed are:—

TANK A.

	Length	Breadth	Height
Fixed rectangular tray having one end open	11′ 10½″	3′ 9½″	—
From laboratory floor of the tray...............	—	—	2′ 6″
Sides and end above the bottom	—	—	9″
Tide generator, one end open	3′ 10½″	3′ 9½″	—
Sides at open end	—	—	9″
At closed end	—	—	1′ 7″

TANK B. Half the dimensions of A.

The proportions of the tide-generators and fixed pans were determined, so that in tank A the greatest rise of tide over the whole tank should be 2″; which was double the tide used in my previous experiments, and that consistently with this the generators should be as short as possible. This tide in tank A required that the generator should displace 10 cubic feet, and as the greatest rise and fall that could be conveniently obtained for the end of the generator was 16″, giving a mean rise of 8″, the area required was 15 square feet.

A period of 30 seconds was adopted for tank A as the shortest period likely to be required, and the gearing arranged accordingly. With this period, and a 2″ tide, the horizontal scale would be 1 in 20,000 of that of a tank with a 30-foot tide, and a period of 12 hours 20 minutes. So that the 12-foot pan would represent 45 miles.

Provision was made for the production of waves with periods $\frac{1}{200}$th the tidal period.

Provision was also made for the introduction of land water into the tank at any point that might be required; also for scumming the water by an adjustable weir, which would serve to keep the level of low water constant, water being supplied into the generator when no land water was required.

The drawings (fig. 1, p. 401) show the tanks and apparatus as they have been constructed. The pans and tide generators are of pine-boards fastened with screws. The former rest in a fixed cradle formed by six legs with cross-bearers, bottom ties, and braces. The floor boards of the pan are screwed to the cross-bearers, but are left free to expand, the joints being made with marine glue, after the manner of the decks of ships. The sides are screwed to the floor only; they receive lateral support against the pressure of the water from the prolongations of the legs upwards. The pans are lined with calico saturated with marine glue, and put down with hot irons, then covered with a coat of paraffin. The pans of the generators are constructed in the same way as the others, only instead of the cross-bearers being attached to legs, they are suspended from two side levers, which are supported on cast-iron knife-edges resting in cast-iron grooves on the top of the legs at the end of the pan. These knife-edges are at the exact level of the top of the floor of the pan, and in line with the joint in the floor between the pan and the generator, so that there is no opening and closing of this joint. This joint is, however, covered with indiarubber, which extends up the sides, and by stretching allows for the opening and closing of these joints.

In tank A these side levers extend 4 feet along the sides of the pan, beyond the joint, and to their ends is attached a large box for holding balance weights. These weights are considerably below the knife-edges, and consequently their moment diminishes as the box descends, *i.e.* as the tide rises, but this diminution by no means compensates the diminution of the water in the generator.

If, therefore, sufficient weight were put into the box to balance the generator when the tide is low, it would much overbalance it when the tide is high. To meet this the weights in the box are used mainly to balance the dead weight of the generator, which requires about 300 lbs., and a varying balance is arranged for the water.

This varying balance consists, in tank A, of a cast-iron cylinder of 500 lbs. weight, suspended by links from the side levers across and under the tank. The cylinder is also suspended by two links from the frame, and this second suspension is so arranged that when the generator is down the links from the levers are vertical, and when the generator is up they are horizontal. In this way a varying balance is obtained, which as far as possible effects a complete balance in all particulars. In tank B, arrangements which have the same effect have been carried out in a somewhat different manner, which will be clear from the drawings.

The glass covering for tank A consists of eight glazed frames, each having two panes of sheet glass 3′ 10″ × 10″, with ¼″ bearing on the frame all round; the external dimensions of the frames are 4′ × 2′, so that they are easily handled. The glass is let in flush with the top of the wood, and each pane is fixed by four small brass clips screwed to the frame. In this way, except for the clips, the top of the glass cover over the pan presents a level surface. The frames over the tide generator are connected with those over the pan by a hinge joint, made of two strips of pine hinged to each other and to the frames.

A somewhat similar arrangement exists in tank B, except that there are only four frames each with a single pane 2′ × 2′. In both tanks the glass frames are fastened by screws to the sides, which screws have to be taken out before the frames can be removed.

The gearing, which is arranged to be driven either from a small water-engine or the running shafting, is shown in the drawings.

The crank is adjustable so as to give any required tide up to the maximum. In tank A, the pulley driven by the belt from the motor or shaft makes 700 revolutions for one of the crank, and has a fly-wheel which considerably helps the motor over any little irregularities in the balance. The gearing in tank B is driven either direct from the motor or from a pulley on the second shaft in the gearing of A, in which way a fixed relation in speed is obtained when the tanks are working together. The motor was obtained from Alderman Bailey, Albion Works, Salford; it is a double-acting oscillating water-engine with a ¾″ piston and 4″ stroke. The available pressure of water is 50 lbs. steady pressure; the consumption is about 1 gallon per 100 revolutions. At the highest speed, 2 tides a minute, the motor only makes about 200 revolutions per minute, so that the 13,000 gallons will keep it going over three days, and has done so from Saturday till Tuesday, Monday being Bank Holiday. It has run day and night and

Sunday, since starting in June, without once stopping, making over 12,000,000 revolutions, and is none the worse. If it used the full pressure it would, when run at 100 revolutions, do about ·044 horse-power. Owing to the careful balance of the tanks and the use of spur instead of worm gearing, the work required is not more than ·008 horse-power, so that five-sixths of the pressure is spent in overcoming the fluid resistance, which, increasing as the square of the speed, affords a very important means of regulating the speed, which, indeed, is thus rendered very regular.

Surveying Appliances.—Since the configuration of the sand produced under different circumstances can only be compared by means of records such as charts or sections, the practicability of the investigation depended on the finding of some means by which the sections or contour-lines on the sand could be rapidly and accurately surveyed.

The floor of the estuary was made flat and carefully levelled, so that the depth of sand at any point could be at once ascertained by sinking a fine scale through it to the bottom; and for this purpose scales were constructed of strips of sheet brass ·01′ broad and ·01″ thick. On these the alternate ·01′ were painted white, and the intermediate spaces in the first 0′·1 were painted red, in the second 0′·1 black, and so on, the scales being then varnished with paraffin.

These scales would stand in the sand edgeways to the current, and so be made into permanent sand-gauges, which could be read periodically without removing the glass or stopping the tide. For tank B the scales were half the size of those for tank A.

The resistance which a few such thin obstructions offered to the water would be very small, but if the gauges were numerous the resistance would be a serious matter, so that a more general method of taking a final survey was necessary.

The ease and simplicity with which the contour-line could be found when the tides were not running, by adjusting the level of the still water and observing its boundary on the sand, reduced the question of making a contour survey to the providing of the means—

1. Of adjusting the level of still water to any required height.

2. Of rapidly and accurately determining the horizontal position of points on the edge of the water.

The tide-gauge, shown in the drawing on the top of the tank, which would stand on the glass which gave a level surface, answered well to determine the level of the water.

For the purpose of surveying the contours a system of horizontal survey-lines were set out in the covering frames, consisting of black thread stretched immediately beneath the glass in the frames. The lines are 6″ apart; those parallel with sides are called lines, and those at right angles sections. The first section is 3″ from the end of the tank* and the lines are so placed that one of them bisects the tank.

These survey-lines divide the entire surface of the fixed tray into equal squares. They are, however, 11″ from the bottom and about 8 from the sand; besides, they are six inches apart, so that to make accurate use of them for surveying the sand it was necessary to use some means of projecting a point vertically up to the level of the glass and scale its distance from a line and a section. This is accomplished by a little instrument, which may be called a projector, shown on the top of tank A.

It has a foot which consists of two scales placed at right angles, so that the zero-lines on both, if produced, would meet in a point about half an inch from the edge of the scale. About this point there is a hole through the foot, with cross-wires so placed that they intersect in the point of intersection of the zero-lines. Vertically above this is a horizontal plate with a pin-hole, so that, when placed on the horizontal glass, any point below, seen through the pin-hole on the cross-wires, is vertically below the intersection of the zero-line of the scales; and hence if these scales are parallel to the lines and sections, the distances of the point from these are read at once on the scales. This method of surveying lends itself readily to plotting on section paper. This may be done directly, the glass cover of the tank serving for a table; each point may be plotted as it is observed; and in this way Mr Bamford is now able to survey and plot a complete contour-line in from fifteen to thirty minutes, and requires only about five hours to make a complete survey plotting the charts.

One very great desideratum has been a graphic recording tide-gauge. So much depends on the manner of rise and fall of the tide that it does not seem sufficient to know that it is produced by a simple harmonic motion; the curve should be recorded for each experiment at different parts of the tank. The want of means and time have prevented any attempt to supply such a gauge.

Curves have been obtained for most of the experiments by means of the simple tide-gauge. The crank-wheel being divided into sixteen equal arcs, one observer observes the wheel and another the gauge. When a particular number comes to the index the observer at the wheel calls, and the other observer reads the gauge, and then shifts the sliding pointer to the point at which the tide-index was, so that on the next revolution, when the call

* This somewhat awkward arrangement was necessary on account of the wood in the frames.

comes again, he can observe if the pointer coincides exactly with the index or requires adjustment. Having brought about coincidence, he then proceeds to the next number. In this way it takes about half an hour to read the curve. Time, however, is not the only objection, a greater one being that any irregularities in the motion of the wheel do not appear in the curve. The motion of the wheel has been as far as possible checked by the clock, but still there is room for important errors, which a chronograph would obviate.

The Selection of Sand.—Sir James Douglas having informed me that clean shell sand could be obtained, and having sent me samples which, from the tests to which I subjected them, seemed to be quite as readily moved by the water as the finest Calais sand, I asked him to procure a quantity—fifteen bushels of Huna Bay shell sand—and in the meantime I procured a similar quantity of Calais sand, so that I might be prepared with whichever showed itself best in actual experiments.

Selection of the Experiments.—It having been decided that in the first instance the purpose of the experiments should be the comparison of the distributions of sand produced under particular lateral configurations, and with different relations between the vertical and horizontal scales in the same model, and with similar relations in these scales in the two models, the only matters left for selection in starting these experiments were the scales and particular configurations.

There was apparently no reason for attempting the very difficult operation of modelling any actual estuary, and, setting this aside, the question of choice mainly turned on whether it was best to begin with complex or simple circumstances. There was considerable temptation to commence with complex, *i.e.* boldly irregular boundaries, so that the influence of the boundaries might predominate over such other influences as exist; in which case the influence of the boundaries would be tested by the similarity of the distributions produced with different ratios of horizontal and vertical scales. On the other hand, however, it appeared that as the main object of these researches is to differentiate and examine the various circumstances which influence the distribution of the sand, it was desirable, in starting, to simplify as much as possible all the circumstances directly under control, and so afford an opportunity for other more occult causes to reveal themselves through their effects, and to determine the laws of similarity of these effects.

The simplest of all circumstances would be that of no lateral boundaries whatever—a straight foreshore of unlimited length with a shelving sandy beach, up and down which the tide runs until it has brought the beach to a state of equilibrium.

This being an impossibility, the nearest approach to it is that of a beach or estuary with vertical lateral boundaries parallel to the direction of flow of the tide. And the broader such estuary is in proportion to its length the less would be the effect of the lateral boundaries. The effect of the tide running straight up and down such an estuary might tend to shift the sand up or down according to the slope at each point, and the period and height of the tide, or until some definite relation between these three quantities was attained. If such a relation exists, its elucidation would seem to be fundamental to a full understanding of the *régime* of estuaries.

Further, there was the very important question how far such a tidal action would leave the bed beach-like, with uniform slope and straight contours, or groove it with low-water channels as in the mouths of estuaries, *i.e.* whether a parallel estuary without land water, having a uniform slope and straight contours, would be stable under the action of a tide of which the general motion was straight up and down.

Considering that the new rectangular tanks with their clean paraffined sides were admirably adapted for such experiments, and that any internal modelling would have required further time, which was already very short, if a report was to be presented at the Newcastle meeting, it was decided to commence with a series of experiments on the general slope and configuration of the sand with parallel vertical sides, after making some preliminary experiments while the tank A was having a preliminary run to test the working of the motor.

Following is an abstract account of these experiments and the results obtained. It has not been thought desirable to introduce into this report a complete copy of the note-book. The initial conditions of each experiment are given, together with the date, the number of tides run, and the mean period of the tide; also notes made during the running on circumstances which are likely to have affected the general results. The final results are contained in the charts (or plans as they are headed), the longitudinal and cross sections, which have been taken from the charts, and the diagram of mean slope obtained from the areas of contours. These are all appended to the report.

Preliminary Experiments with Balls.—Little balls of paraffin the size of peas were prepared, colouring matter having been first mixed with the paraffin to distinguish the balls, and to so load some that they would just sink while others floated. Then, before the motor was started, the water being quite still, the balls were placed in rows across the tank at definite distances down the tank, and from the centre line—one set of balls on the bottom and another set floating above. The motor was then started, and the change in position of the balls noted.

It was supposed that the floating balls would move with the water and show by any change of their mean position if there was any circulation in the water. This was what they did when the surface of the water was perfectly clean, but the slightest scum very greatly diminished the range of the motion of the floating balls. This matter of scum, if it can be so called, when it is entirely unperceivable by the eye, is very important in these model experiments; for, however slight it is, it tends to prevent the horizontal motion of the immediate surface, and indirectly to modify the internal motion of the water; the only test of perfect freedom from surface impurity is that small drops caused by a splash falling on the surface float along. When the surface was in such a state the floating paraffin balls oscillated up and down with the water, and kept the position for many oscillations both up and down and across the channel.

The sinking balls are subject to the constant resistance of the bottom, so that their motion is not equal or proportional to the motion of the water —for a sufficiently slow motion of the water the ball would not move; so that if the ebb were just not sufficient to move the ball, and the flood were stronger, the ball would be moved up each tide, or *vice versâ*, and the same resultant motion would follow, even though the ball might be moved somewhat by both ebb and flood; the strongest would carry the ball farthest. In this way they furnished a very delicate test as to the symmetry of the tides and the sufficiency of the balancing.

Experiments on the Movement and Equilibrium of Sand in a Tide Way.

Series 1.—Tide running in a uniform rectangular pan with vertical sides and end, and a level bottom for the sand to rest on.

Experiment 1 (tank A), commenced June 22.—Three cubic inches of Calais sand was placed in a heap on section 17 and line 1_r and 3 cubic inches of Huna Bay shell sand similarly on section 17 and line 1_r, the tank being otherwise clean and empty. Then, with low water 0·083 of a foot and high water ·23′ from the bottom, the tide was set running with a period of 55 secs. After 3000 tides, the white sand spread upwards from section 16·5 to 12·7 in 7 ripples, having just painted the bottom 1 grain thick down to section 22.

The Huna Bay shell sand spread upwards from section 18 to 14·25 in 4 ripples, also having painted the floor down.

Experiment 2 (tank A), commenced June 24.—Calais sand was introduced as a uniform bank across the channel.

Experiment 3 (tank A).—The Calais sand was arranged exactly as for Experiment 2.

Experiment 4 (tank A).—A repetition of Experiment 3, observations being directed more closely to the motion of the sand after starting.

Experiment 5 (tank A), July 5 (Plans III. to VI.).—Calais sand passed through a sieve into the tank, in which there was sufficient water to cover the sand until there was enough sand to fill the tank from the upper end to section 18, to a depth of 0·25 foot, terminating in a natural slope from section 18 to the floor. Then the sand, which was in excess at the upper end, was carefully levelled by means of a wooden float guided on the sides of the tank, and having its straight edge completely across the tank 0·25 foot from the bottom. The scummer was then adjusted to keep the low water at 0·181 from the floor, and the crank adjusted to give a rise of 0·166 over the whole tank. The actual rise at starting, owing to the sand above low water, was 0·2′ over the whole surface.

The tank was then started, and ran 12,607 tides at a period of 53 secs., when the speed was increased to 50 secs. and continued for 3589 tides; then, as the condition seemed very steady, the survey for Plan I. was made.

At the starting of the tank careful note was taken of the progressive appearances, and during the interval of running, which occupied from July 5 to July 15, sand gauges were introduced and read daily, as well as other notes of progress made. The periods of rise and fall of the tide were ckecked, and a curve taken which showed the rise and fall at the generator to be symmetrical and nearly harmonic.

The sand was found to descend down the tank towards the generator in a steadily diminishing manner, while at the same time it rose towards the head of the tank at a steadily diminishing rate, until both these changes ceased to be observable. The configuration of the surface also changed at a steadily diminishing rate. The chief features in this configuration were the banks, which gradually formed at the head of the tank in a very symmetrical form, and then extended down the tank past low-water level, losing their symmetry as the low-water channels between them began to take effect. These banks and channels appear clearly in the plan. The minor features were numerous minor channels caused by the water running off the banks. These covered the surface with very beautiful detail, which, however, it is quite impossible to record. Also ripple bars across the channel below low water.

After the first survey was taken the tank was started again July 17 at a somewhat slower period, viz., 66·7 seconds, and ran for 7815 tides, when the second survey was made. The daily observation taken as before showed considerable changes of detail—so much so that it was a matter of surprise to find that Plan II. corresponded almost exactly with Plan I., the only to show a greater rate of downward progression, at the middle of the tank, towards the generator than at the sides, and this was followed by a somewhat more rapid descent of the lower edge of the sand, which after 5000 tides began to accumulate in the generator, from which about seven pounds was removed.

From this stage the lower end of the tank B differed considerably from that of tank A in the same stage. At the upper end the appearances were almost identical, and the reading of the sand gauges agreed well.

As the experiment progressed, the sand, instead of having a nearly uniform downward slope from the head to the generator, had a uniform slope down the middle of the tank, with two large banks extending from section 8 to section 17 on each side, that on the right being longer. The experiment was continued for 11,013 tides, when it was found that the water was much too low, owing to misadjustment of the scummer; then as there was no possibility of saying how long this had been going on, the experiment was stopped.

Experiment 2 (tank B, Fig. 7, p. 407), August 28. Plan 1.—In this the conditions of Experiment 1 were repeated, the edge of the float having been replaned. The results from starting were almost identical with those observed in Experiment 1. The sand again came down fastest in the middle, and faster than in tank A. Seven pounds were removed from the generator, and subsequently the condition of the model as regards the lateral banks was nearly the same, except that the longer bank was on the left. The experiment was continued with speeds exactly corresponding to those of Experiment 5, tank A, until 16,344 tides had been run; then Plan I. was taken. The tank was then set running again at 35·5 seconds and continued for 6757 tides, when considerable changes had taken place towards the lower end of the tank. A partial survey was then made and recorded, and the experiment stopped.

Experiment 3 (tank A, Fig. 8, p. 408, and tank B, Fig. 9, p. 409), Sept. 2.—The sand in both tanks was arranged as before, a new float straightened to a surface plate being used for B, and the level of the sand in both tanks tested by water, as in Experiment 6 A, which tests showed that the sand in A was perhaps ·01″ highest on the left, while in B it was to something like the same extent highest on the right.

The tanks were coupled, A being driven from the motor and B from A. Both were set to low tide at starting, and the start made at full speed, 33 seconds tank A. The progressive appearances simultaneously observed were identical, with the same exception as before noted. Immediately after starting, the periods of rise and fall of the generator of A were observed, and the fall being slightly the larger, 25 lbs. was removed from the balance weight, which restored the equality. After 77 tides it was observed that the sand in A was coming down much faster than in B, and had already begun to come into the generator; the periods of rise and fall were noted, and it was found that the rise was 17 seconds and the fall 15 seconds. The weight was replaced, the tanks stopped, and 56 lbs. of sand removed from the generator and lower end of the trough of A which left the end of the sand the same in both tanks. The tanks were then started, and the rise and fall in A were equal.

It may be well to remark that though the tank B is driven from A, the periods do not synchronise, so that the unequal motion caused by imperfect balance of A eventually affects all stages of the tide in B equally, while the resistance of B is so small compared with that of A, that any want of balance hardly affects the motor when driving both tanks. In starting there would be the same disturbance of balance in both tanks owing to the slow descent of the water from the flat sand, but it would be only that of A that would affect the balance.

After running 1653 tides, tank A, it was seen that the sand had come into the generators of both tanks, so a stop was made, and all sand below section 20 again removed from both tanks—120 lbs. from A and 12 lbs. from B, making altogether 176 lbs. from A against 12 lbs. from B. Considering that 1 lb. in B is equivalent to 8 lbs. in A, and that altogether in A there would be 1100 lbs., B was left with about 7 per cent. more sand, in proportion, than A.

The experiment was then continued, the sand coming down in both tanks, but not so as to get into the generators. The motion of the sand in the two tanks followed almost exactly the same course, B gradually taking the lead. In this case there was not the least sign of the middle channel in B, the sand keeping level across and following the same course as had previously been observed in Experiments 5 and 6 A.

When B had run 16,570 tides it was stopped for surveying, while A was allowed to run on to make up the number of tides.

Surveys were then made.

Discussion of the Results Obtained.

Since the experiments have been arranged in accordance with the law of kinetic similarity, followed in my previous experiments, it may be well to restate this law before proceeding to discuss the results.

If h be the depth of water in a uniform trough, it is well known that the velocity of a wave, of which the length L compared with h is great, and of which the height is proportional to h, varies as the square root of h.

For geometrical similarity at any instant the lengths of the troughs must be proportional to L.

The period of rise and fall, p, will thus be inversely proportional to

$$\frac{\sqrt{h}}{L}.$$

Hence for the law of kinetic similarity,

$$\frac{p\sqrt{h}}{L} \quad \text{.....................................(1),}$$

has a constant value for all scales.

This law takes no account of the resistance of the bed, for a first approximation to which the law would be

$$\frac{p\sqrt{h} + A\left(1 - B\frac{\sqrt{h}}{p}\right)}{L} \quad \text{............................(2),}$$

constant, where A and B are constants to be determined by experiments.

Since the comparative periods of the two tanks have been made proportional to the square roots of their dimensions, *e.g.* the period of tank A, $\sqrt{2}$ times the period of tank B, the bottom resistances produce dynamically similar results.

In comparing the results obtained with the same values of h in the same tank with different periods, the bottom resistances would be different, and this difference should appear in the results unless too small to be appreciable, in which case the results would compare with the simple period.

There are four other sources of possible divergence from the simple dynamic law, which will become larger as the periods become slower and the tide lower :—

1. The drainage of the sand after the tide has left it supplies the low-water channels with a constant stream at low water; the velocity of this stream will depend on the slope and quantity supplied, and supposing the quantity to be proportional to hL^2, the depth of the water in the low-water channels (not the depth of the channels) will be proportional to the cube root of the slope;

2. The size of the grains of sand, which require a certain velocity before they move;

3. The fouling of the sand by growth, &c., which increases as the shifting of the sand diminishes; and

4. The viscosity of the water, which causes a definite change in the internal motion of the water when the velocity falls below a point which is inversely proportional to the dimensions of the channel.

The effect of 1 would be confined to the channels; 2 and 3 would tend to diminish the rate of action; the 4th might seriously alter the rate of action at different parts of the estuary, and would also affect the appearance of the sand surface.

The ground so far covered by the experiments has been confined to one initial arrangement and to one height of tide in each tank, these being similar. Two periods have been tried in each tank, the relation between the periods in the different tanks being as the square roots of the dimensions. Six experiments have been started:

2	in tank A	with a period of		53	secs.
1	"	"	"	33	"
2	in tank B	"	"	36·5	"
1	"	"	"	23·3	"

Of the two experiments started at 53 seconds in tank A the first was continued for 12,697 tides, and then for 3589 tides at a period of 50 secs., and a survey made (Fig. 3, p. 403). It was then continued 7815 tides at 65·1 secs., and the plan marked, Fig. 4, p. 404; it was then continued 17,750 tides with intermittent waves at a period of 60·6 secs., and a survey made (Fig. 5, p. 405).

It was then continued for 12,705 tides at periods varying from 33 secs., having a mean 43, and Plan 4 (Fig. 6, p. 406) made, then continued at a period of 33·3 secs. with intermittent waves, when it was re-surveyed (dotted on Plan 4).

The second experiment at 53 secs., tank A, was continued to 8700 tides with the same results as the first.

Of the two experiments in tank B the first was continued to 11,013 tides as in A, then stopped. The second was continued to 12058 tides at 36·8 secs.; then at 4280 tides at 36 secs., and surveyed (Plan 1 B, Fig. 7, p. 407); then continued to 6769 more tides at 36, and again surveyed.

The experiments started at 33 secs., tank A, and 23·3 secs., tank B, were continued to 16,603 tides and then surveyed (Figs. 8 and 9, pp. 408, 409).

In all these six experiments the manner in which the water commenced and proceeded to redistribute the sand was essentially the same, the general appearances of the surface being, with the exception of one or two particulars, the same at the same number of tides up to 1200. After this the two low-speed experiments in B began to present more noticeable differences from the other experiments, which continued to present similar appearances at corresponding tides to the end.

It thus appeared:—

1. That the rate of action was proportional to the number of tides;

2. That the first result of the tide-way was to arrange the sand in a continuous slope, gradually diminishing from high water to a depth about equal to the tide below low water;

3. That the second action was to groove this beach into banks and low-water channels, which attained certain general proportions (plans 5 and 7 A and 2 B, and cross sections, Figs. 5, 8 and 7);

4. That the slope arrived at after 16,000 tides was the same at the high speed in both models working at corresponding periods, $\sqrt{2}$ to 1 (sections, Figs. 8 and 9);

5. That in both models the steepness of the actual slope increased as the tidal period diminished (sections, Figs. 5, 8, 7 and 9).

Owing to the grooving of the surface, the exact slopes at the various speeds cannot be exactly compared. One way of effecting a comparison has been to take the highest points on each cross section down the slope, and plot them as a longitudinal section, and in the same way to take the lowest points and plot them as another. These are shown in the two longitudinal sections which accompany each plan.

The increase of the slope with the diminution of the tidal period, both as regards the banks and channels, is thus rendered apparent; but these sections do not admit of an accurate comparison.

Some definite and accurate method of comparing these slopes was essential before any definite conclusions could be arrived at respecting the laws of similarity. To meet this the areas above the successive contours have been taken out. These areas respectively divided by the breadth of the plan give the mean distance of the respective contours from the head of the estuary, and the heights of these contours plotted to this mean distance give a definite mean slope of the sand. There are certain minor objections to this method, but it is eminently definite and practical, and admits of great accuracy, the areas being readily taken out with a planimeter with very great accuracy even for the most complicated contours. The slopes thus taken out are more readily compared if plotted to scales such that the vertical distances between high and low water are all equal, the horizontal scales being determined so that the vertical exaggeration is the same in all cases.

The slopes thus taken out from 5 A (Figs. 3 and 6), 7 A (Fig. 8), and from 3 B (Fig. 9) are shown in (1) Fig. 2. They present a great degree of regularity; and it is seen at once that the result of corresponding periods (33 secs. tank A, and 23 secs. tank B, Figs. 8 and 9) agree very closely.

In order to compare the slopes with the conditions of kinetic similarity, all that is necessary is to reduce the horizontal distances in the inverse ratio of the periods, when the slopes should become identical. In doing this the horizontal distances have all been reduced to represent (according to the kinetic law) a 30-feet tide with the natural period 44,400 seconds, namely, the ratio of the lengths of the estuaries made equal to the ratio of the periods multiplied by the square root of the ratio of the heights. The actual rise and fall of the tide in the models being taken:—

The horizontal and vertical scales for the five experiments as thus reduced to a 30-feet tide are given in Table I.

TABLE I.

Reference	Period in Seconds	Horizontal Scale	Inches to Mile	Vertical Scale	Rise of Tide
V. A, Plan 1 ...(3)	50	{·0000862 / 1 in 11,600}	5·45	{·00587 / 1 in 170}	·176
V. A, Plan 4 ...(6)	33·3	{·000055 / 1 in 18,200}	3·49	{·00533 / 1 in 187}	·161
VII. A, Plan 1 (8)	33·6	{·000056 / 1 in 17,900}	3·55	{·0055 / 1 in 182}	·165
II. B, Plan 1 ...(7)	35·4	{·0000431 / 1 in 23,200}	2·72	{·00293 / 1 in 341}	·088
III. B, Plan 1 ...(9)	23·7	{·0000299 / 1 in 33,400}	1·895	{·00313 / 1 in 317}	·094

Table II. shows the measured height from low water for each of the contours, together with its mean distance from the contour at the height which, reduced, is 30 feet above low water. Also the corresponding heights of the contours of the 30-feet natural tide, and the corresponding mean distances of the contours measured in miles, from which the curve of reduced mean slopes shown in (2) Fig. 2, have been plotted.

Considering the character of the investigation, the agreement between the slopes is quite as close as could be expected, and there is nothing to argue from the divergences, except that the effect of the bottom resistances has here been too small to affect the results.

Table II.

Measured Heights of Contours shown on the Plans	Tank A — Experiment V — Plan 1: Height (reduced to a 30-feet Tide) of Contours from L.W.	Mean Horizontal Distance of Contours from the Contour at 30 feet above L.W.	Horizontal Distances reduced to a 30-feet Tide	Experiment V — Plan 4: Heights (reduced to a 30-feet Tide) of Contours from L.W.	Mean Horizontal Distance of Contours from the Contour at 30 feet above L.W.	Horizontal Distances reduced to a 30-feet Tide	Experiment VII — Plan 1: Heights (reduced to a 30-feet Tide) of Contours from L.W.	Mean Horizontal Distance of Contours from the Contour† at 30 feet above L.W.	Horizontal Distances reduced to a 30-feet Tide
Feet	Feet	Unit 6 inches	Miles	Feet	Unit 6 inches	Miles	Feet	Unit 6 inches	Miles
*	—	– ·69	– ·76	—	– ·975	– 1·65	—	– ·93	– 1·67
·176	30·	·00	0	30·	0	0	28·05	·25	·42
·146	24·9	·91	1·	24·39	·79	1·355	22·58	·99	1·67
·116	19·8	2·13	2·34	18·68	1·86	3·2	17·11	1·76	2·98
·086	14·62	4·29	4·7	13·0	2·96	5·07	11·64	3·65	6·18
·056	9·52	6·47	7·1	7·46	4·64	7·95	6·17	5·3	8·95
·026	4·43	9·26	10·15	1·87	6·63	11·38	0·70	7·36	12·44
– ·004	– ·68	11·51	12·52	– 3·74	8·43	14·5	– 4·77	9·07	15·37
– ·034	– 5·8	14·58	16·00	– 9·35	10·30	17·8	– 10·24	11·00	18·60
– ·064	– 10·9	19·41	21·3	– 15·	12·17	21·6	– 15·71	13·20	22·32
– ·094	– 16·	21·31	23·4	– 20·8	13·60	23·4	—	—	—
– ·124	—	—	—	– 26·2	15·88	27·3	—	—	—

Measured Heights of Contours shown on the Plan	Tank B — Experiment II — Plan 1: Height (reduced to a 30-feet Tide) of Contours from L.W.	Mean Horizontal Distance of Contours from the Contour at 30 feet above L.W.	Horizontal Distances reduced to a 30-feet Tide	Experiment III — Plan 1: Height (reduced to a 30-feet Tide) of Contours from L.W.	Mean Horizontal Distance of Contours from the Contour at 30 feet above L.W.	Horizontal Distances reduced to a 30-feet Tide
Feet	Feet	Unit 3 inches	Miles	Feet	Unit 3 inches	Miles
*	—	– 7·	—	—	– 1·82	– 2·88
·088	30·	0	—	30·	0	0
·073	24·9	·5	—	25·2	·73	1·16
·058	19·8	2·	—	20·5	1·50	2·37
·043	14·62	3·	—	15·7	2·20	3·49
·028	9·52	5·8	—	10·9	3·53	5·6
·013	4·43	9·	—	6·3	5·25	8·32
– ·002	– ·68	11·8	—	– 1·36	7·06	11·2
– ·017	– 5·8	14·3	—	– 3·4	9·24	14·61
—	—	—	—	– 8·2	10·14	16·1
—	—	—	—	– 13·	12·43	19·8

distan* The distances in this row are the mean of the Contour at 30 feet from the ends of the tanks.
being a 't having been found that in measuring the heights which are shown on this plan the datum
n taken ·0106 feet below L.W., the mean distances in the table have been obtained by
ion between the mean distances as obtained from the areas of the contours on the plan.

The Length of the Foreshore.—The interval between mean high and low water, about 12·5 miles according to the kinetic scale for a 30-feet tide, cannot readily be compared with any actual case, since there are no sandy foreshores subject to a 30-feet tide-way except those which are in a sea-way and subject to longitudinal currents, while in the deep bays and mouths of estuaries, slopes are cut up with low-water channels besides a want of regularity in the lateral boundaries. In such bays as Morecambe Bay, Lynn deeps, Solway Firth, the mean distance from the shore to the foot of the sands at low water must be 8 or 10 miles, and even taking this as the actual length it leaves no great margin for the resistance of the bottom, which would be 50 or 100 times greater in the actual case than with a model with a distortion of 50 or 100 times.

The only divergences of importance occur at the top and bottom of the slopes. That at the bottom of the curve for Experiment 5 A, Plan 1, is probably owing to the proximity of the generator, as in this plan the survey was continued to the end of the pan.

Such results, with regard to low-water channels, as have been obtained from the experiments already made, are not discussed in this report, because they have been incidental to the immediate purpose of the experiments; they have, however, been carefully recorded for future reference. The same might be said of the manner of action of the water on the sand, were it not that these experiments have revealed a part taken by one of these actions, the importance of which does not appear to have been hitherto observed. This is the action known as rippling of the sand. In these experiments this action is seen to play an essential part in determining the rate at which the distribution of the sand is effected, while the result of this action—the ripple marks—forms a most conspicuous feature in the final distribution, as seen on the plans, as well as at all preceding stages.

The ripple marks on the strands are too well known to need description, and there is nothing surprising that similar ripple marks should appear in the beds of the models. But although presenting a very similar appearance, and being about of the same size, the ripple marks seen in all the plans are essentially different in their origin and in the position they take in the *régime* of the sand in the models from that held by the observed ripple marks on the shore sands. This last is caused by the alternating currents produced by the small swell running inshore, while that in the model is produced by the alternating action of the tide. There may seem nothing remarkable in this, considering that these currents in magnitude and velocity are not dissimilar—but if the models are similar to the results obtained in estuaries, the converse should hold, and the estuaries should be similar to the models. In which case we are face to face with a very striking conclusion, that in the estuaries there should be—call it ripple mark or wave mark, produced by the action of the tide, similar to that on the models and on a scale proportional to the height of tide in the estuary. Thus some of the ripples in the models are from hollow to crest as much as one-fourth the mean rise of the tide, the distance between them being 12 times their height. This, in an estuary, would mean 7 or 8 feet high and 80 to 100 feet in distance.

These ripples in the model are almost confined to the surface of the sand which is below low-water mark, though in places their somewhat eroded ends protrude up the slope from the low-water channels. The existence of these ripples very much enhances the effect of the water to shift the sand—this was noted in the experiments 2 and 3 on the bars, tank A; on the smooth walls of the sand the current, which would be about 6 inches a second, did not drift the sand at all, except close to the ridge, and then there was no apparent effect till after 1700 tides, when ripples were just beginning, yet when the ripple once formed in another 1200 tides the top of the bar had spread to 12 inches.

The ripples also serve to show in which way any shift of the sand is taking place, as they have a steep side looking in the direction of motion, and when the slopes are equal it is an indication of equilibrium.

Conclusions.—So far as these experiments have gone they have shown that similar results as to the general slope and rate of action of the sand can be obtained by models working according to the kinetic law as low as tides of

1 inch with a vertical exaggeration of 100,

or 2 inches " " 64.

They have not shown, however, that the limit has been reached. Although the results obtained with a tide of 1 inch with a vertical exaggeration of 64 in tank B presented peculiarities which appeared in two experiments, it is still open to question whether these might not have been owing to something in the initial circumstances. This first series is therefore yet incomplete; it should include experiments to show the smallest vertical exaggeration at which similar results can be obtained with tides as small as half an inch and as large as 2 inches. This would give the law of the limits; this would conclude the first series. Then, if the experiments are continued, another series might be undertaken to determine whether similar effects can be obtained from land water acting on such slopes as have been already obtained; and again, as to the law of slopes and cross sections on V-shaped estuaries, and then, though this has been already established in my previous experiment, as to the effects of irregular lateral configuration in the shores*.

* For continuation, see p. 410.

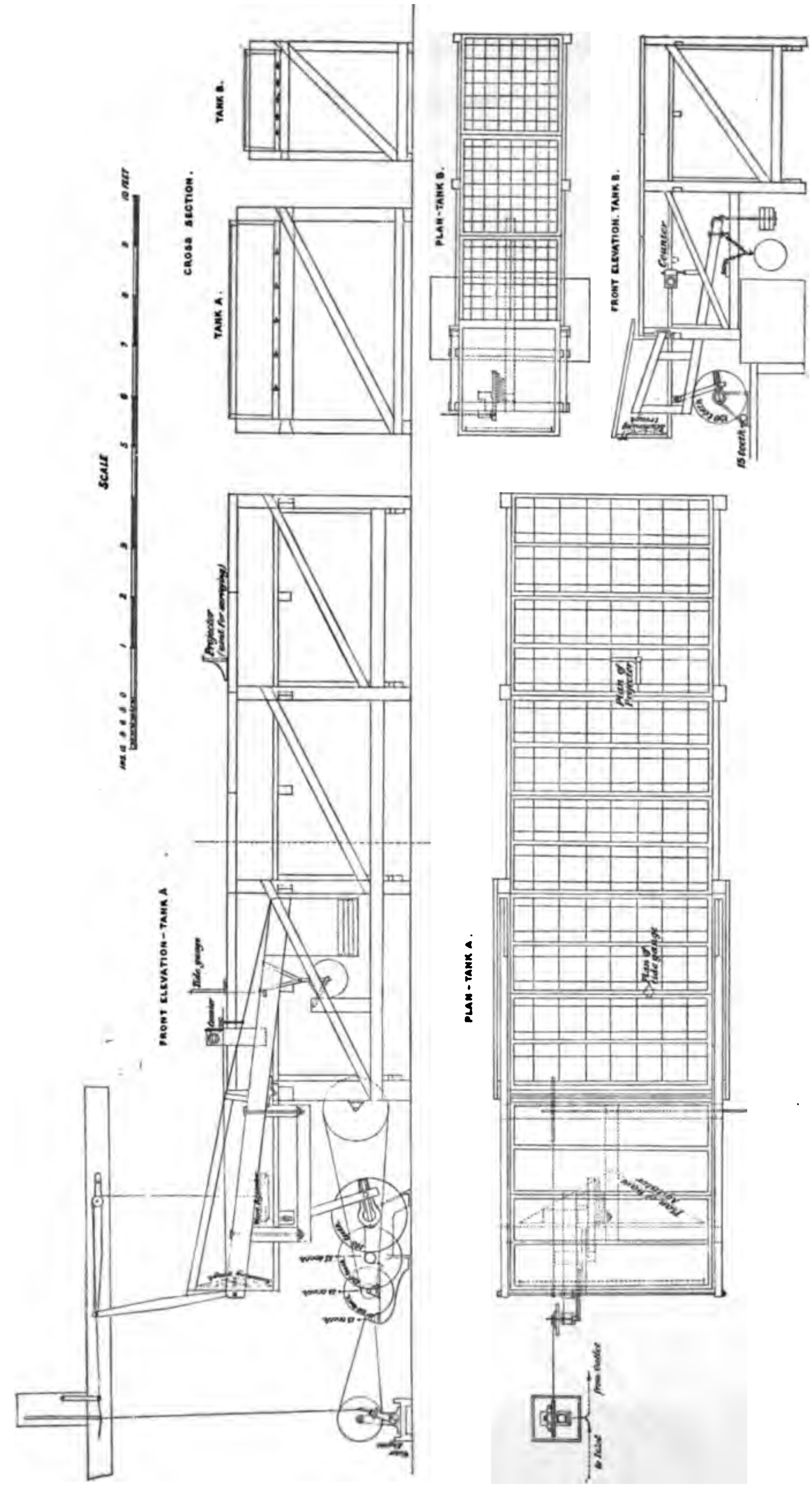

FRONT ELEVATION - TANK A
PLAN - TANK A
CROSS SECTION.
TANK A.
TANK B.
PLAN - TANK B.
FRONT ELEVATION. TANK B.
SCALE
10 FEET
Projector
Counter
Tide gauge
15 teeth

Fig. 1.

FIG. 1.

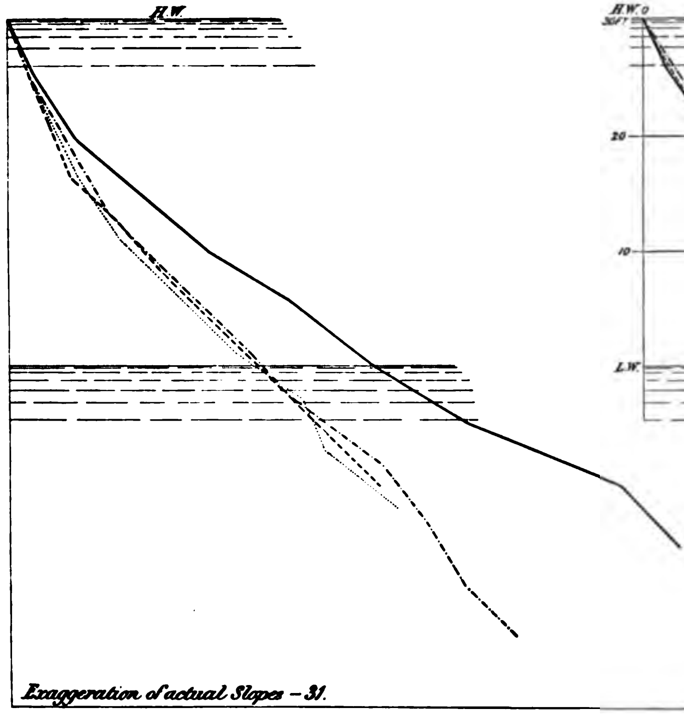


FIG. 2.

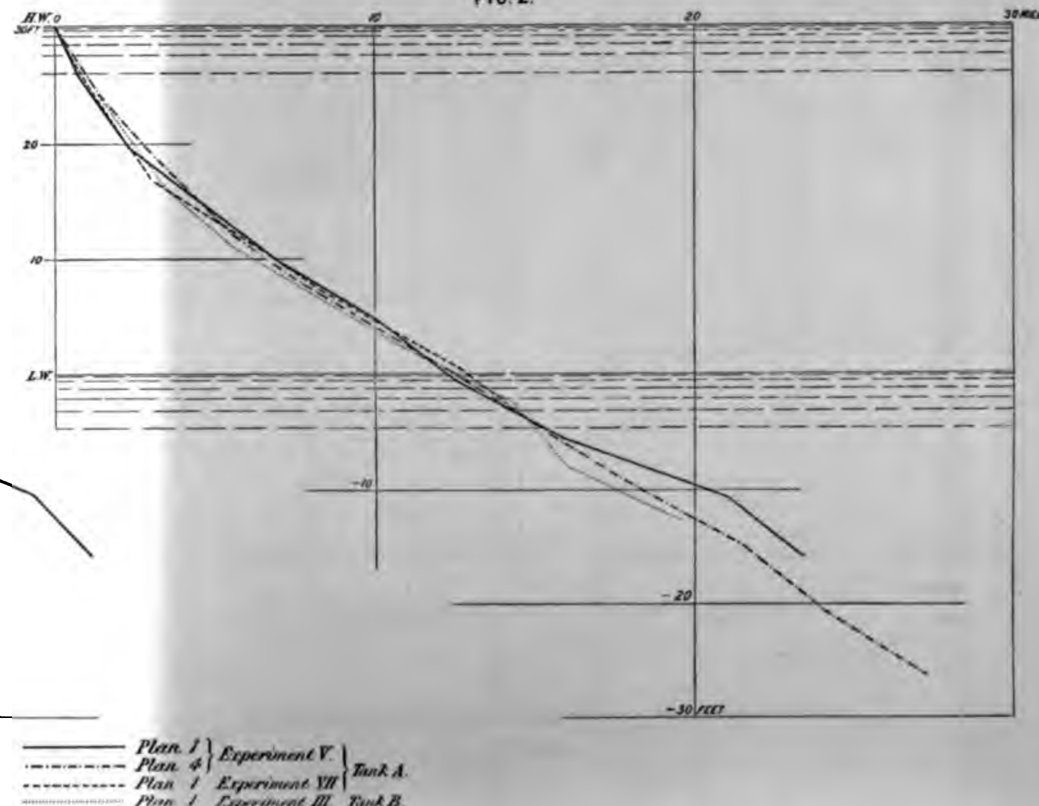


Fig. 2.

Tank A. Experiment V, Plan I, after 12697 tides at 53·4 Sec^s. & 3589 at 49·3 Sec^s.

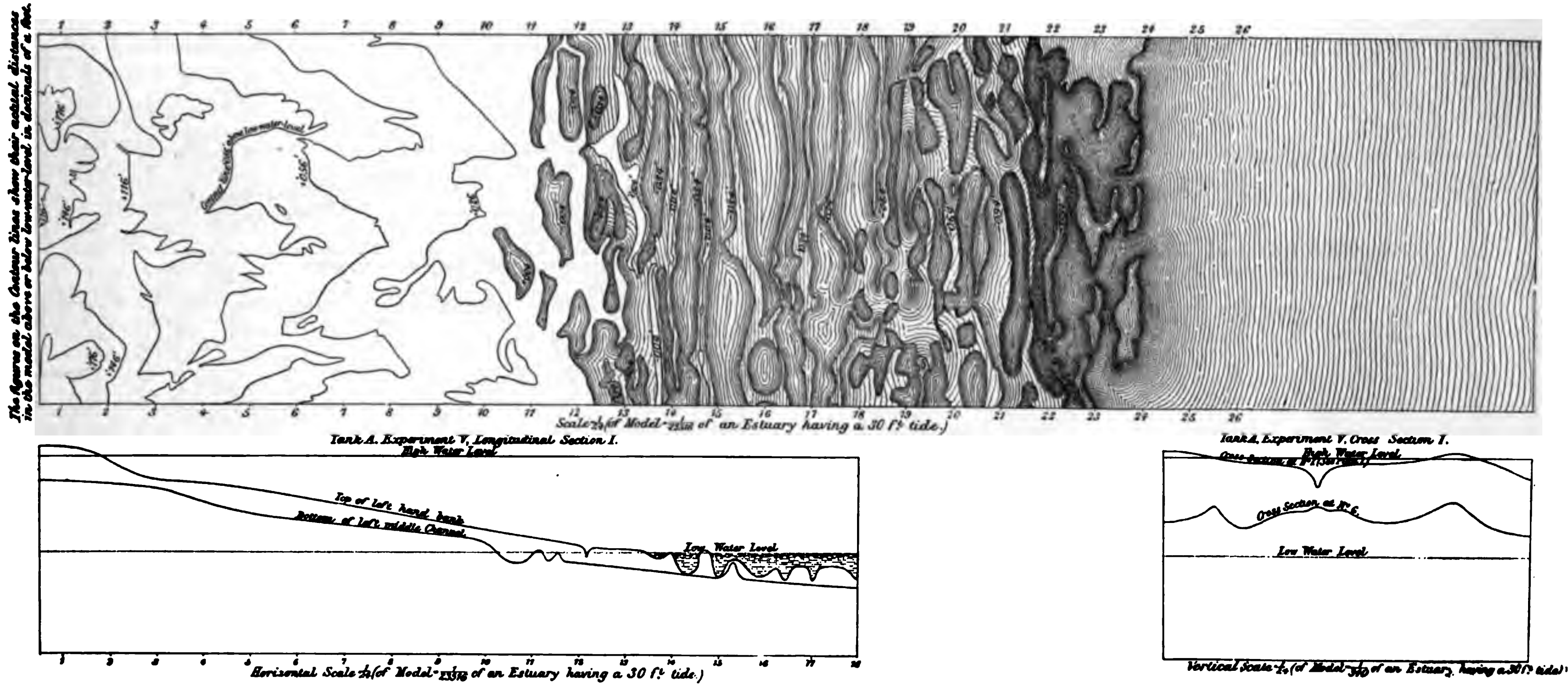


Fig. 3.

Tank A. Experiment V, Plan II, after 12697 tides at 53·4 Sec^s., 3589 at 49·3 Sec^s., & 7815 at 65·1 Sec^s.

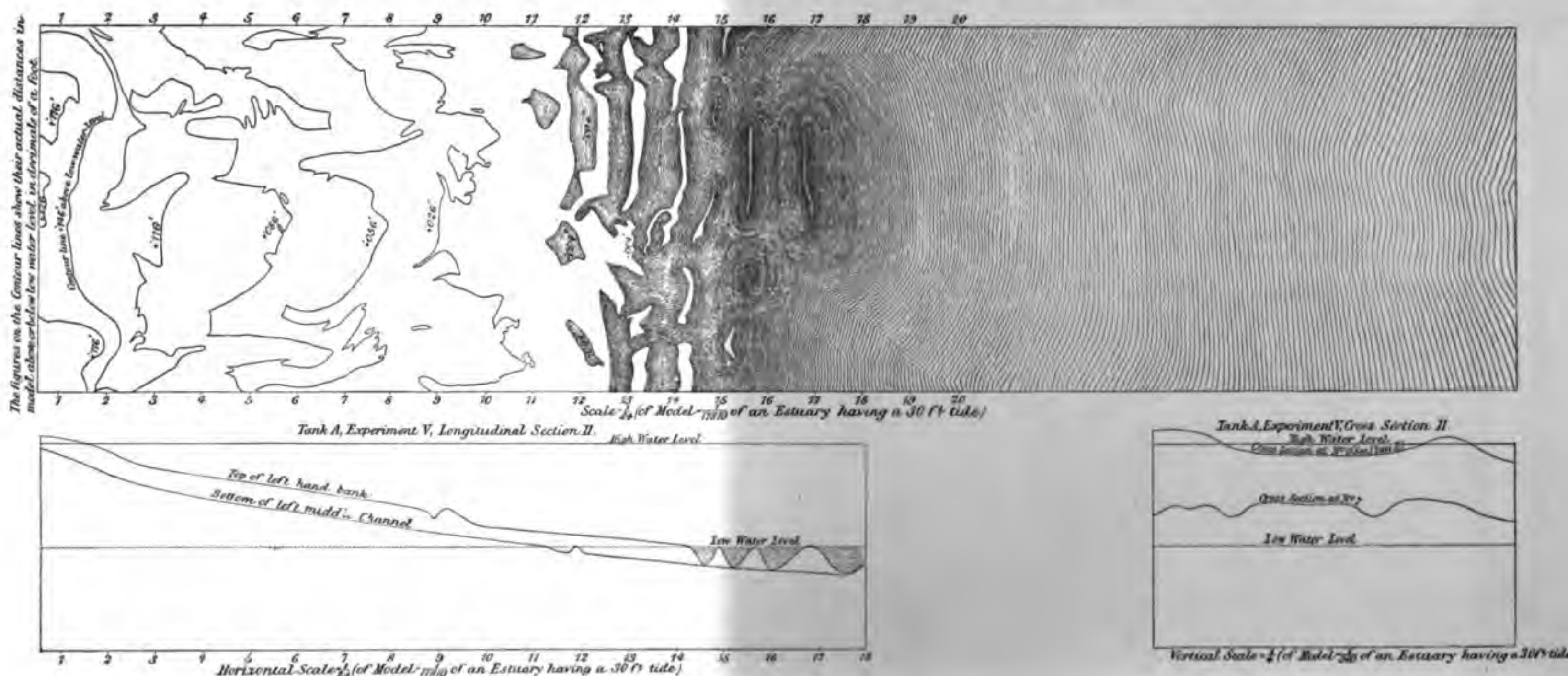


Fig. 4.

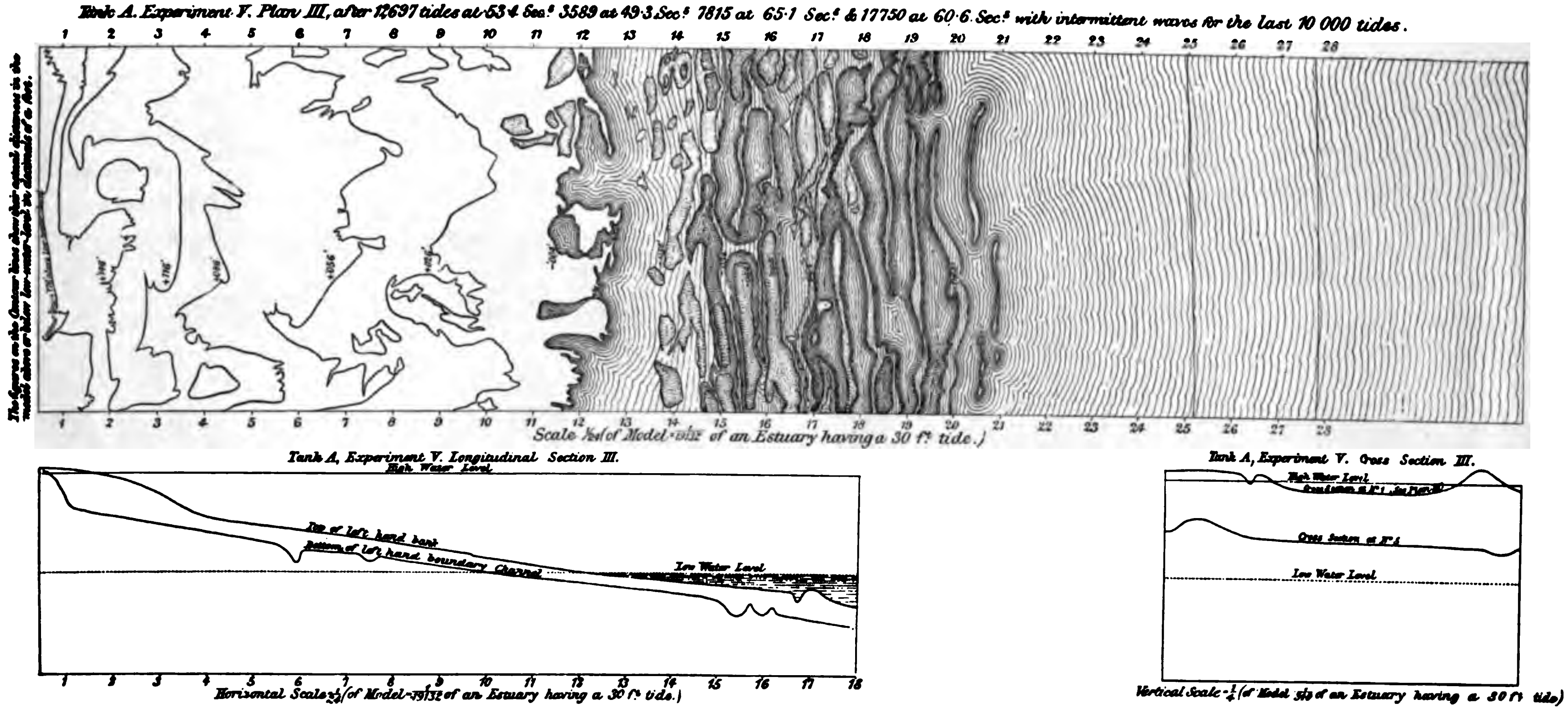

Tank A. Experiment V. Plan III, after 12697 tides at 53·4 Sec.s 3589 at 49·3 Sec.s 7815 at 65·1 Sec.s & 17750 at 60·6 Sec.s with intermittent waves for the last 10 000 tides.
Scale 1/80 (of Model ·) of an Estuary having a 30 ft. tide.)
Tank A, Experiment V. Longitudinal Section III.
High Water Level
Top of left hand bank
Bottom of left hand boundary Channel
Low Water Level
Horizontal Scale (of Model) of an Estuary having a 30 ft. tide.)
Tank A, Experiment V. Cross Section III.
High Water Level
Cross Section at No. 1
Low Water Level
Vertical Scale 1/4 (of Model) of an Estuary having a 30 ft. tide)

Fig. 5.

Tank A. Experiment V, Plan IV, after 12697 tides at 53·4 Sec^s., 3589 at 49·3 Sec^s., 7815 at 65·1 Sec^s., 17750 at 60·6 Sec^s., & 12705 at 43·2 Sec^s., with intermittent waves for the last 22705 tides.

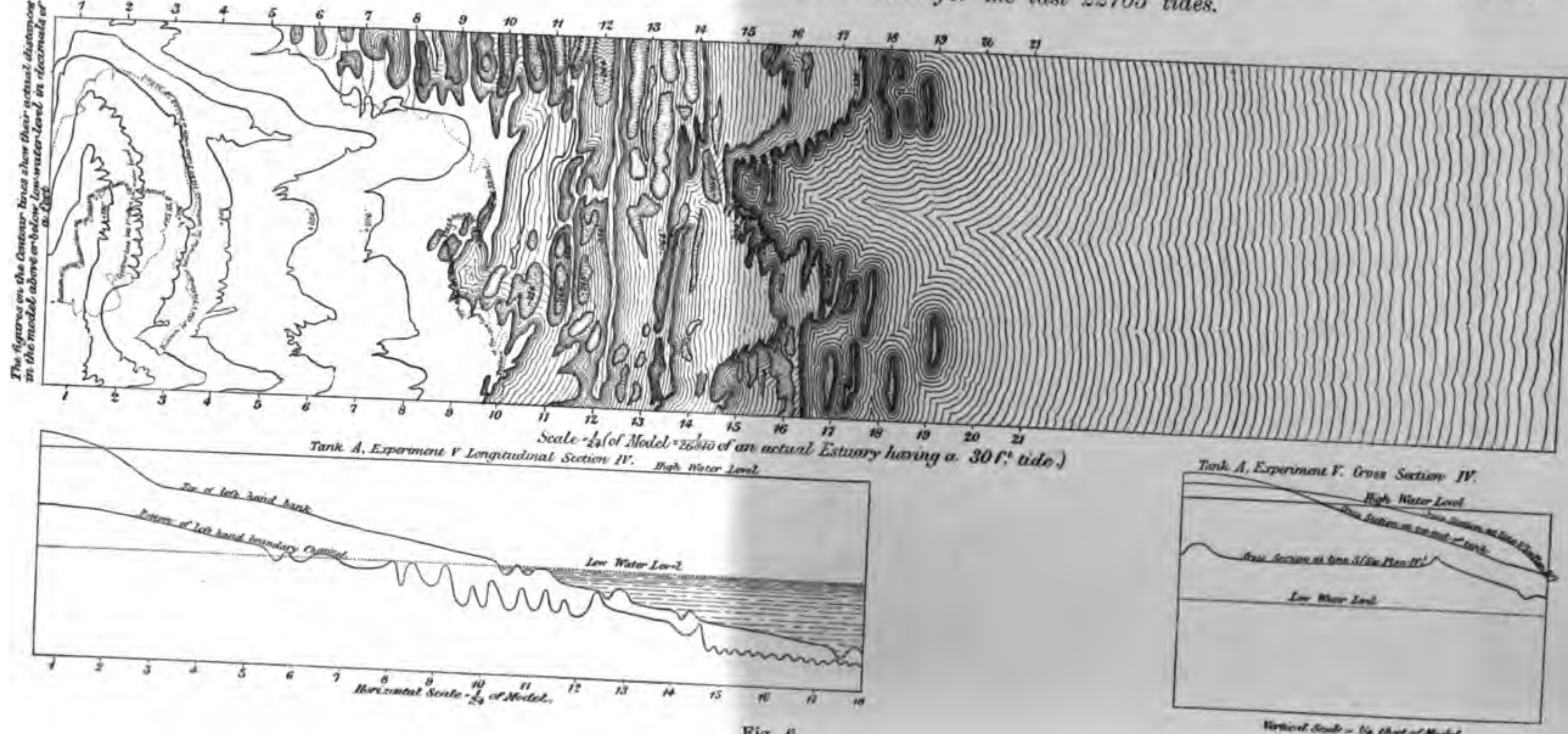


Fig. 6.

Tank B. Experiment II, Plan I, after 12058 tides at 36·8 Sec^s., & 4286 at 36 Sec^s.

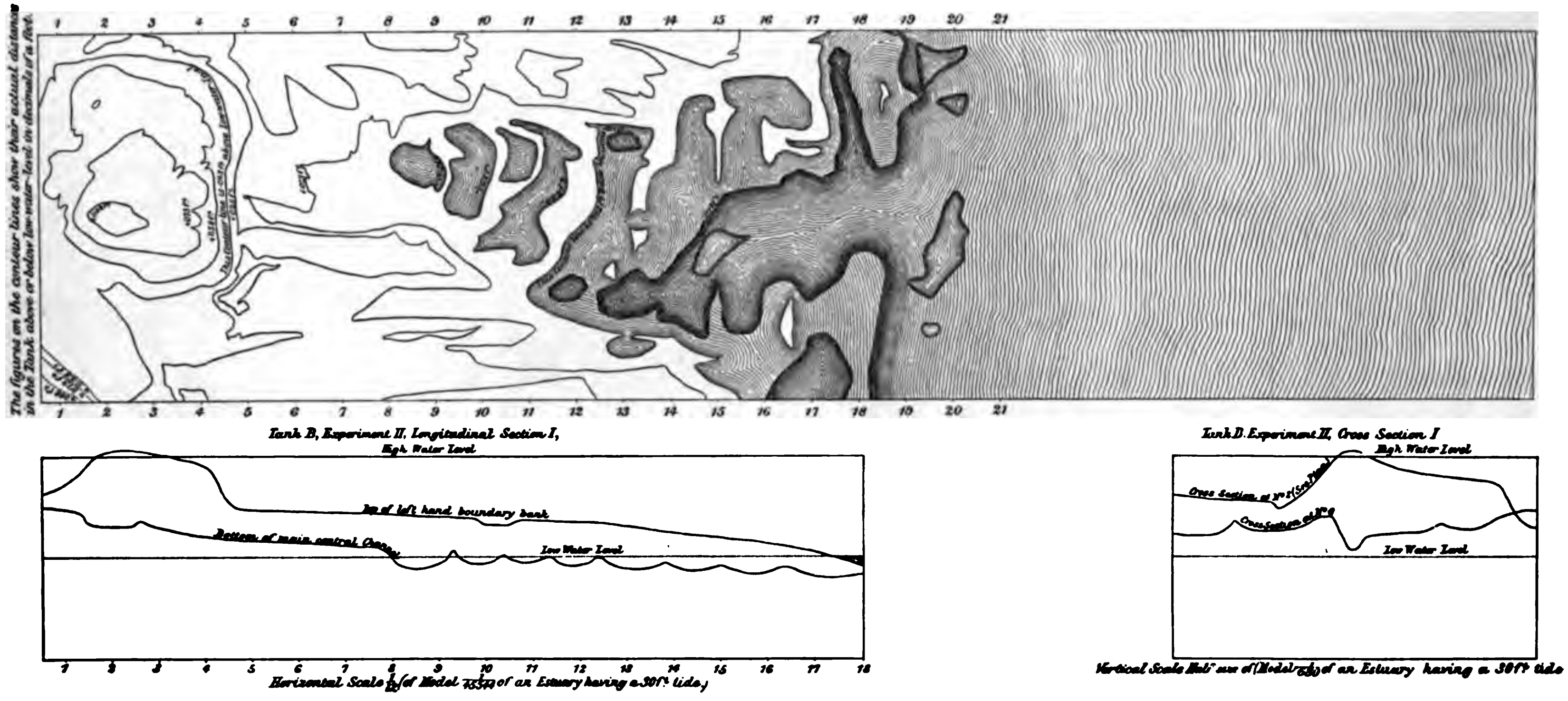


Fig. 7.

Tank A. Experiment VII, Plan I, after 16733 tides at 33·4 Sec^s.

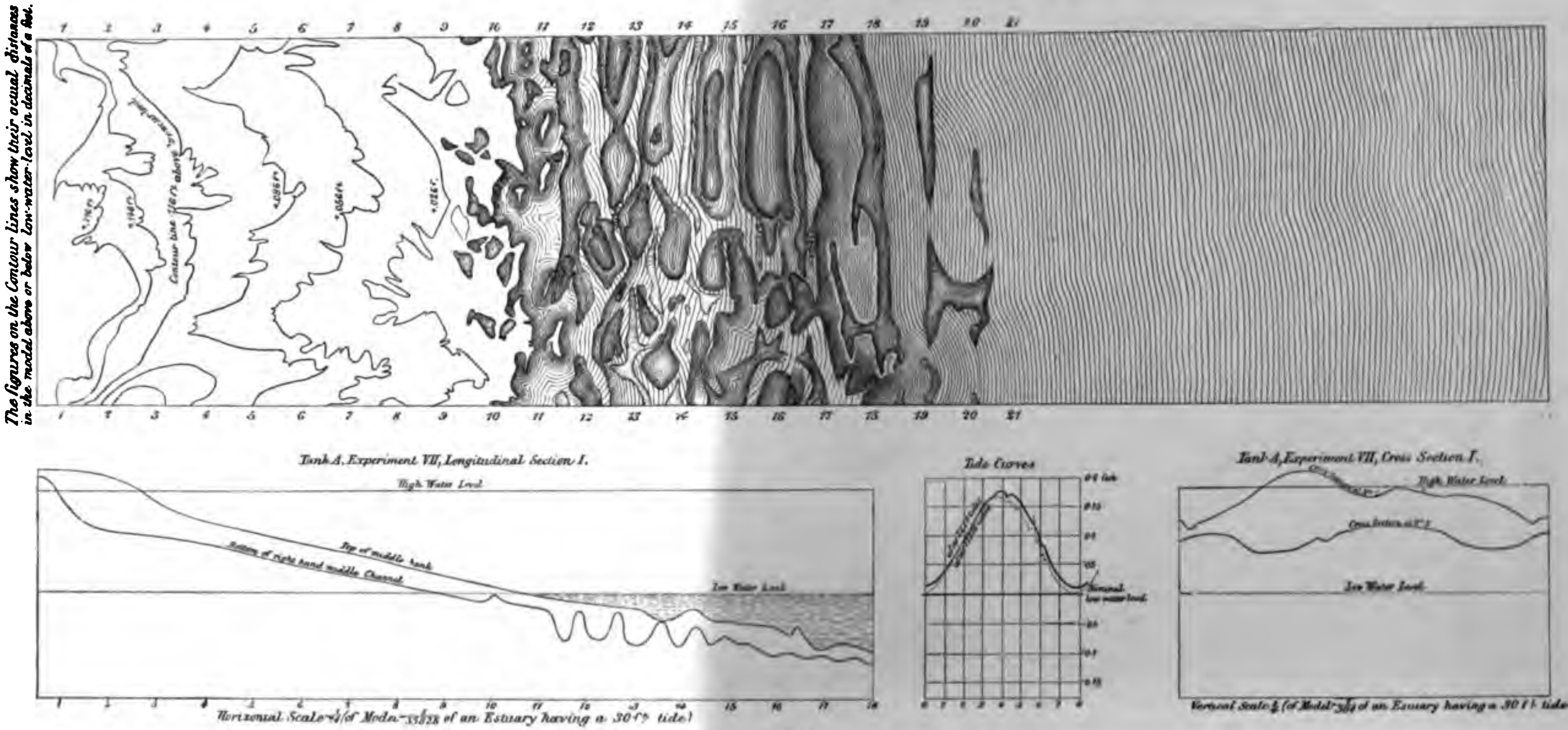


Fig. 8.

Tank B. Experiment III, Plan I, after 16570 tides at 23·53 Sec^s.

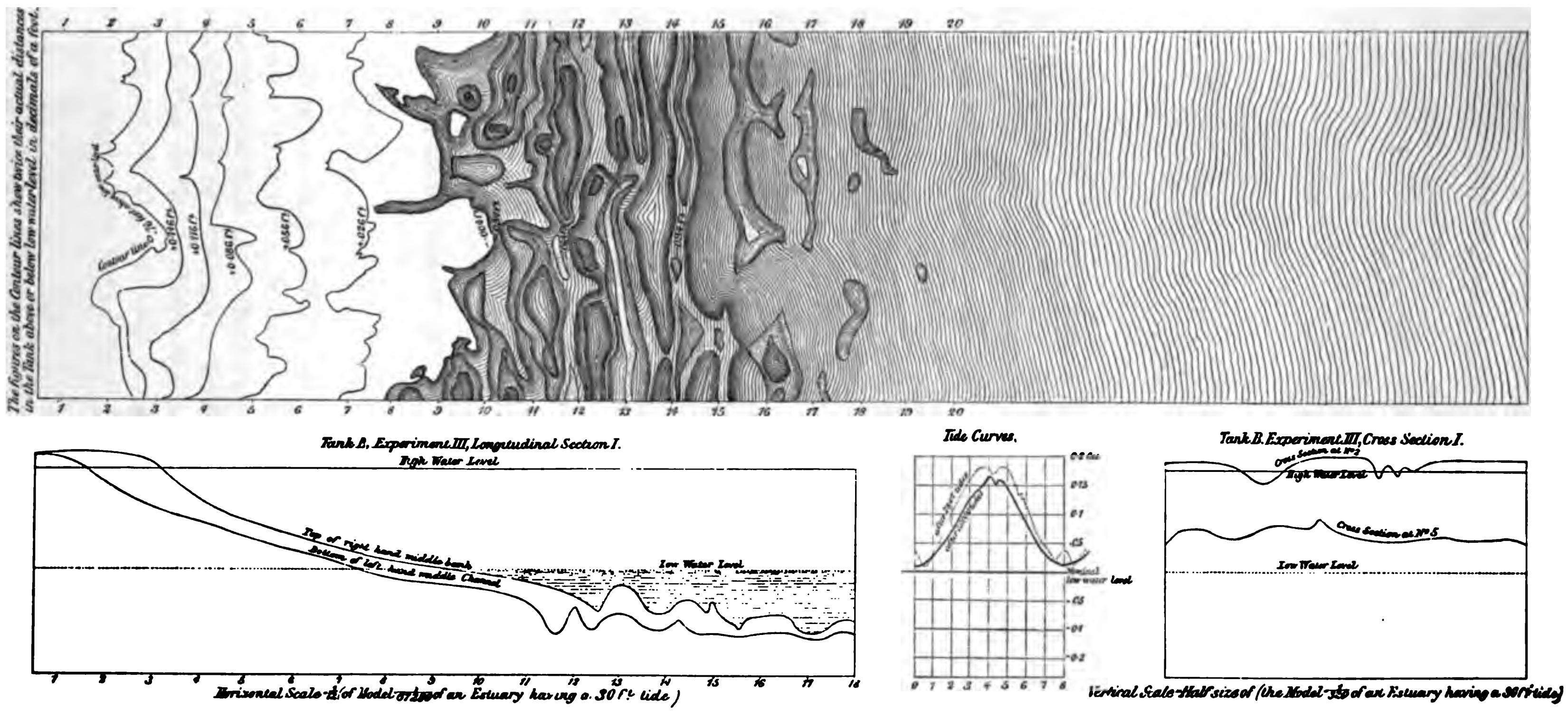


Fig. 9.

been already obtained in rectangular estuaries; and (3) to investigate the character and similarity of the results which may be obtained with V-shaped estuaries.

2. The two models, subject to such modifications as were required for the various experiments, have been continuously occupied in this investigation, running, driven by the water motor, at all times when they were not stopped for surveying or arranging a fresh experiment. They have thus run about five-sixths of the time day and night. In this way the large model has worked through in the twelve months 500,000 tides, corresponding to 700 years. These tides have been distributed over ten experiments in numbers from 32,000 to 100,000. The smaller model has run more tides than the larger, and these have been distributed over fourteen experiments.

3. The experiments have all been conducted on the same system as is described in last year's report (see p. 380).

Initially, with two exceptions, the sand has been laid with its surface as nearly as possible horizontal at the level of half-tide, extending from the head of the estuary to Section 18, and in the later experiments to Section 17. The vertical sand gauges, distributed along the middle line of the estuary, have been read and recorded once a day. Contour surveys have been made after the first 16,000 tides, and again after the first 32,000, and in the longer experiment further surveys have been made; in all, fifty complete surveys have been made, and forty-four plans, showing contours at vertical intervals corresponding to 6 feet on a 30-foot tide, are given in his report.

The general conditions of each experiment, together with the general results obtained, are given in Table I., p. 436, and a description of each experiment is given in § IV. p. 423.

The importance of a better means of recording the tide curves was mentioned in last year's report. Such means have been (see p. 422) obtained during this year, and automatic tide curves have been taken as nearly as practical at corresponding numbers of tides during the experiments, these curves being taken at several definite sections in each tank. Two series of these curves have been taken in the later experiments, one in which the paper is moved by a clock, the pencil being moved by a float; the other in which the paper is moved by the tide generator, by which means exactly similar motion for the paper is secured at all points of the estuary, so that differences in the phases of the tide at different parts of the estuary are brought out. These curves are shown on the plates.

Mr H. Bamford has continued to conduct the experiments, but on account of the very great amount of detailed work the entire time of a second assistant has been occupied. For this the services of Mr J. Heathcott, B.Sc., were obtained from October to February, when Mr Heathcott obtained an appointment in the office of the engineer to the L. & N.W.R. in Manchester. Mr Greenshields then applied for and obtained the post, and has continued the work with great patience and zeal.

§ II.—General Results and Conclusions.

4. *The Limits to Similarity in Rectangular Estuaries.*—In the experiments of last year it was found (1) that as regards

1. Rate of action as measured by the number of tides run;
2. Manner of action; and
3. The final condition of equilibrium

with tides of 0·176 foot and periods of 50 and 35 seconds the results were similar, according to the hydrokinetic law $\frac{p\sqrt{h}}{L}$ constant; (2) that, as regards rate and manner of action, the results obtained with tides of 0·094 foot and periods 23·7 seconds were similar to those with the tide of 0·176; but the experiment had not proceeded to the final condition of equilibrium.

It was also found that with tides of ·088 foot and periods 35·4 seconds, the results obtained differed in a marked manner from the others as regards rate and manner of action, so much so as to render the attainment of a final state of equilibrium impracticable.

These results seemed to indicate that for each rise of tide there exists some critical period such that for all smaller periods the results would be similar according to the simple hydrokinetic law, while for larger periods the results would be dissimilar in a greater or less degree to those obtained with periods smaller than the critical period. Whether or not the results obtained with periods greater than the critical periods would present a general similarity amongst themselves, or even similarity under particular relations among the conditions, were still open questions.

The experiments, as shown in Table I., Table II., made this year, emphatically confirm the conclusions (1) as to the existence for each rise of tide of a critical period at which the rate and manner of action begin to change, being similar for all smaller periods; (2) these experiments also confirm the general similarity of the final states of equilibrium as regards slopes for periods smaller than the critical period, as shown in Table II.

The experiments (Experiments IV. and VIII., B) this year, also show that with tides of 0·094 and 0·097 foot the periods 34·4 and 35·4 seconds are greater than the critical periods, although the results show a nearer approach to similarity, as regards manner and rate of action, than the results obtained last year in II. B, with the tide ·088 foot and period 35·4 seconds, while the final conditions of similarity were approximately reached.

With tides of 0·088 foot and periods 69·3 seconds the results in rate and manner of action are emphatically different from those with less than the critical period, and with tides of 0·042 foot and periods 50·5 seconds still greater differences are presented.

On the other hand, it is found (V. B) with tides 0·042 foot and periods 50·5 seconds that if the sand be given a condition corresponding with the condition of final equilibrium, as if the period were above the critical period according to the simple hydrokinetic law, this is a state of equilibrium; and, further, that it is not a state of *indifference* is shown, since on diminishing the period the sand readily shifted so as to bring it nearer the theoretical slope for the new period. This shows that the state of equilibrium follows the simple hydrokinetic law for periods greater as well as less than the critical period, which is thus shown to be critical only as regards rate and manner of action in reducing the sand from the initial level state to the final condition.

The experiments carefully considered suggest that there is some relation between the rise of tide and critical period. They do not, however, cover sufficient range to indicate what this relation is with any exactness. The critical period diminishes with the rise of tide, but much faster than the simple ratio.

5. *Causes of the Change in Manner and Rate of Action.* The change in the action, which sets in at the critical period, is the result of some action, of which no account is taken in the simple hydrokinetic law. A list of five such sources of possible divergence from the hydrokinetic law is included in last year's report (p. 396), and with a view to obtain an indication of some relation between the rise of tide and period (or vertical exaggeration, as compared with the standard tide of 30 feet, by the kinetic law), which relation would be a *criterion* of the limiting conditions under which the simple kinetic law may be taken as approximately accurate, these five discarded actions were carefully considered.

The fouling of the sand by the water, although it comes in as preventing further action, cannot take any part in imposing these limits, since it is at the immediate starting of the experiments that the action is observed to fail. For the same reason the limits cannot be in any way due to the *drainage from the banks*, as these banks have not appeared above water.

Again the limit cannot be due to the *size of the grains of sand* because it would then occur at particular velocities, whereas this is not the case.

The other actions are the *bottom resistances* and the *viscosity of the water, which causes a definite* change in the internal motion of the water as the velocity falls below a point which is inversely proportional to the dimensions of the channel.*

That this last source of divergence from the simple kinetic law must make itself felt at some stage appeared to be certain. But the critical velocity at which the motion of the water changes from the '*sinuous*' or *eddying* to the *direct* is inversely proportional to the depth, and by the kinetic law the homologous velocities in these experiments are proportional to the square roots of the depths only; hence this action would seem to place a limit, if it were a limit, to the least tide at which the kinetic law would hold independently of the period, and this is not the case. Observation of the action of the water above and below the critical periods, however, confirmed the view that the limit was in some way determined by this critical condition of the water. For when water is running in an open channel above the critical velocity, the eddies, of which it is full, create distortions in the evenness of the surface which distort the reflections, creating what is called swirl in the appearance of the surface. Now it was noticed and confirmed by careful observation, that in the cases where similarity failed, the swirl was absent at the commencement of the experiment, while it was easily apparent, particularly on the ebb, in the other experiments. Subsequently it appeared that the velocity of the water, particularly during the latter part of the ebb, which has great effect in the early stages, might be much affected by the bottom resistances, and hence not follow exactly the kinetic law.

6. *Theoretical Criterion of Similar Action.*—The velocities of the water running uniformly in an open channel, i being the slope of the surface and m the hydraulic mean depth, is given by

$$v = A\sqrt{im},$$

where A is constant.

If, then, i is proportional to e (the exaggeration of scale) and m proportional to h, since at the critical velocity v is inversely proportional to h, at this velocity h^3e has a constant value.

The function $h^3e = C$ is thus a criterion of the conditions under which similarity in the rate and manner of action of the water on the sand ceases.

7. *The Critical Values of the Criterion for Rectangular Tanks.*—Taking

* Reynolds on the Two Manners of Motion of Water, *Phil. Trans.*, 1883, pt. iii. (see page 51).

to represent the rise of tide in feet, and e to be the vertical exaggeration s compared with a 30-foot natural tide by the simple hydrokinetic law, ie values of this criterion have been calculated for each of the experiments nd are given in Table I.

Experiments I. and II., B, First Report, $C=0\cdot046$, showed marked sluggishness and local action; IV., B, $C=0\cdot058$ and VIII., B, $C=0\cdot064$ showed ss, but still a certain amount of sluggishness and local action*, while in II., B, $C=0\cdot083$, the rate of action was good and the action similar to the xperiments with values for C higher than $0\cdot087$*, whence it would seem iat the critical value of the criterion is about 0·087, and it may provisionally e assumed that $C=0\cdot09$ indicated the limits of the conditions of similar ction*.

8. *The value of the Criterion for* V-*shaped Estuaries.*—This critical alue of C deduced from the experiments in rectangular tanks appears to orrespond very well with the results of the experiments in the V-shaped stuaries. In the experiments Table I. with V-shaped estuaries in the nall tank, the value of C is in no case far from the critical value ·09 on ither side. In Experiment IX., B, however, the value of C at starting as only 0·046 as in I., B, and in consequence of the observed sluggishness nd local character of the action in the lower estuary, the rise of tide was icreased from 0·088 to 0·11, which remedied the action and raised the riterion to 0·101, and in Experiments X. and XII., B, and in I., D, the alues are between 0·095 and 0·084. In Experiments II., D, F, and F′, wing to the falling off in the tide in consequence of the addition of the iver, the criterion is as low as 0·073. In these experiments signs of sluggishness and local action in the lower estuary were observed at starting, nd the difference in the action of the upper estuary as compared with 'ank E in respect of closing up the tidal river may have been due to the w value of the criterion.

In the experiments in the large tanks the values of C are all well above he critical value: the nearest are the experiments in Tank E, $C=0\cdot17$, hich is only double the critical value, and the action was as quick and eneral as in the case where $C=0\cdot5$.

It may be noticed that the range through which the value of C as a riterion has been tested is small. Had the form of criterion been apprehended sooner this might have been somewhat extended, though considerable daptation of the apparatus would be required to carry it far.

* In both these experiments, IV. and VIII., B, the mean level of the tide was above the initial vel of the sand, which would naturally increase the value of the criterion.

9. If $C=0{\cdot}08$

With a tide	0·1 ft.	the greatest period is	32 secs.	and least exaggeration	80.
,,	0·12 ft.	,, ,,	60 secs.	,, ,,	47.
,,	0·14 ft.	,, ,,	102 secs.	,, ,,	30.
,,	0·2 ft.	,, ,,	6 mins. 9 secs.	,, ,,	10.
,,	0·43 ft.	,, ,,	1 h. 33 m. 48 s.	,, ,,	1.

From which the size of tanks and length of periods necessary to verify this law for exaggerations of less than thirty can be seen.

10. *The General Distribution of Sand in V-shaped Estuaries.*—The experiments all show that with sufficiently high values of the criterion, as in the rectangular tanks so in those of symmetrical V-shape, the sand arrives at a definite general state of equilibrium after a definite number of tides. This state in the rectangular tanks was a general slope which corresponded to a definite curve, twelve miles long as reduced by the kinetic law to a 30-foot tide, between the contours at high and low water in the generator. This slope was furrowed by 3 or 4 shallow channels at distances of some two miles, commencing very gradually at the top and dying out at some distance below low water. In the V-shaped estuaries the state of equilibrium differs from that in the rectangular tanks in a very systematic manner; it consists in a main low-water channel commencing at the end and extending all the way down the V out into the parallel portion of the tank. If this channel is in the middle it is the only channel, but if, as is as often as not the case, it takes one side of the estuary, then at the lower end there is on the other side a second channel starting at some distance down the estuary. The height of the banks above the bottom of the main low-water channel towards the lower end of the V is much greater than in the rectangular estuaries. No general method of comparing the general slope or distribution of the sand in the V-shaped estuaries has been suggested other than that of comparing the contoured plans and the longitudinal section taken down the highest banks and lowest channels, together with the cross sections which have been plotted on the plans. These are very similar for the similar tanks and corresponding periods. They show that the slope in the channels down to low water is nearly the same as in the rectangular tanks, the level of low water being reached at distances from the head of the estuary a little greater than in the rectangular tank, and a little greater in the long V than in the short. Below low water the slope in the channels is less than in the rectangular estuaries, which is, doubtless, a consequence of lateral spreading. The slope of the banks is much less than in the rectangular tanks, and these extend from two to three times as far from the top of the estuary, according to the angle of the V.

The range of observations on V-shaped estuaries has necessarily been limited, and time has not sufficed to duly consider all the results obtained, but the following conclusions may be drawn:

(1) In similar shaped V-estuaries configurations similar according to the simple hydrokinetic law are obtained irrespective of scale, provided the criterion of similarity has a value greater than its critical value. (2) That the general character is that of a main channel and high banks. (3) That the estuaries are longer in a degree depending on the fineness of the V than rectangular estuaries with corresponding tides, while the low-water contour reaches to nearly the same distance from the top of the estuary.

11. *In the experiments with a long (fifty miles) tidal river increasing in width downwards slowly until it discharges into the top of the V-shaped estuary* the character of the estuary is entirely changed. The time occupied by the tide getting up the river and returning causes this water to run down the estuary while the tide is low, and necessitates a certain depth of water at low water, which causes the channel to be much deeper at the head of the estuary. In its effect on the lower estuary the experiments with the tidal river are decisive, but as regards the action of silting up the river further investigation is required, both to establish the similarity in the models and to ascertain the ultimate state of equilibrium.

It may, however, be noticed that the general conditions of the experiments in Tank E do not differ greatly from the conditions of some actual estuary, as, for instance, the Seine. This estuary is some thirty miles long before it contracts to a tidal river which extends fifty miles further up. In the model the tidal river reduced to a 30-foot tide is forty-nine miles long and the V extends down twenty-eight miles further, while the results in the model show about the same depth of water in the channel down the estuary as existed in the Seine before the training walls were put in.

12. *The Effects of Land Water.*—These come out clearly in the experiments, which show that the stream of land water running down the sand, although always carrying sand down, does not tend to deepen its channel, since at every point it brings as much sand as it carries away. If it comes into the estuary pure, it carries sand from the point of its introduction and deposits it when it gets to deep water, somewhat deepening the estuary at the top and raising it below, which effect is limited by the influence the diminished slope has to cause the flood to bring up more sand than the ebb carries down. The principal effect of the land water is that running in narrow channels at low water, which are continually cutting on their concave sides, it keeps cutting down the banks, preventing the occurrence of hard high banks and fixed channels. When the quantities of land water are small as compared with the tidal capacity of the tank, its direct action on the *régime*

of the estuary is small. But that it may have an indirect action of great importance in connection with a tidal river is clearly shown. In the upper and contracted end of a tidal river the land water may well be sufficient to keep it open to the tide, whereas otherwise it would silt up. This was clearly the effect in the experiments E, 1 and 2, and by keeping the narrow river open the full tidal effect of this was secured on the sand at the top of the estuary, causing a great increase of depth. The effects of large quantities of land water, such as occur in floods, have not yet been investigated.

13. *Deposit of the Land Water in the Tidal River.*—One incident connected with the land water in the tidal river is worth recording, although not directly connected with the purpose of the investigation.

The land water, one quart a minute, was brought from the town's mains in lead pipes. It is very soft, bright water, and was introduced at the top of the estuary. This went on for about three weeks. At the commencement the sand was all pure white, and remained so throughout the experiment except in the tidal river. At the top of the river a dark deposit, which washes backwards and forwards with the tide, began to show itself after commencing the experiment, gradually increasing in quantity and extending in distance. At the end of the experiment the sand was quite invisible from a black deposit at the head of the river and for 5 or 6 feet down; this, then, gradually shaded off to a distance of 12 feet. Nor was it only a deposit, for the water was turbid at the top of the river and gradually purified downwards.

On the other hand, in the precisely similar experiment, without land water the sand remained white and the water clear right up to the top of the river. This seems to suggest that these experiments might be useful to those interested in river pollution.

14. *The International Congress on Inland Navigation.*—During the Fourth International Congress on Inland Navigation, held in Manchester at the end of July, the members were invited to see the experiments then in progress, the subject being one which was occupying the attention of the Congress. Advantage of the invitation was taken by many engineers, and especially by the French engineers. M. Mengin, engineer in chief for the Seine, stated in a paper* read at the Congress that in consequence of the paper (read by the author before Section G at Manchester) the engineers interested had advised the Government to stop the improvement works on the Seine until a model having a horizontal scale of 1 in 3000 was constructed, and the effect of the various improvements proposed investigated

* International Congress on Inland Navigation, 1890.

a the model, the model being then nearly ready, but the experiments had ot commenced. M. Mengin paid several visits to the laboratory and carefully xamined the apparatus and experiments, for which all facilities were placed t his disposal.

15. *Recommendations for further Experiments.*—Although the immediate bjects proposed for investigation this year have been fairly accomplished, here remain several general points on which further information is very nportant, besides the further verification of the criterion of similarity and he determination of the final conditions of equilibrium with tidal rivers, lready mentioned. It seems very desirable to determine the effect of tides a the generators diverging from the simple harmonic tides so far used, imple harmonic tides being the exception at the mouths of actual estuaries. t would also be desirable before concluding these experiments that they hould include the comparative effects of tides varying from spring to neap.

§ III.—Modifications of the Apparatus.

16. *General Working of the Apparatus.*—The apparatus has worked erfectly in all respects except that of the driving cord connecting the 'ater motor with the gearing. For this cord hemp was first used, as it 'as liable to be wet. This hemp cord wore out with inconvenient rapidity. continuous cord made of soft indiarubber was then tried, and, after several ttempts, has been made to answer well. The only other failure was the mall pinion, which was fairly worn out, and had to be replaced.

17. *Extensions.*—For carrying out the experiments on the V-shaped stuaries the original tanks had to be increased in length. To do this it 'as necessary to remove temporarily part of the glass partition dividing he engine room of the laboratory, in which the tanks are placed, from the esting room. This being done, the tanks were then extended, as shown Fig. 1, page 439), the first extension being an addition of a trough 6 feet ong and 2 feet wide to Tank A, and a similar extension of half the size to 'ank B, the new tanks being thence called C and D.

18. *Extensions for Tidal Rivers.*—The second extension consisted of a rough 19 feet long and a foot wide to the end of C, the new tank being ence called E. The corresponding extension to D was not at first made the same way, because to do so would require the removal not only of a anel of the glass partition, but also of a fixed bench, which was a much ore serious matter, or else the extension would have closed up an important assage. The extension was therefore made, as shown in Figs. 46 and 47,

page 479, which admitted of the tidal river being the corresponding length to that in E, but required a bend of 180°, which was effected by two sharp corners. This tank was thence called F′. This was the best that could be done during the time the students were in the laboratory. It was not certain that the corners would produce any sensible effect, whereas if the results obtained in F′ were not similar to those in E no time would have been lost, since the straight extension could not be made till the end of June. As the results in F′ were not similar to those in E in a way which might be explained by the bends, as soon as possible the straight extension was made similar to E, and the tank called F.

All these tanks were constructed in the same manner as the original tanks, and covered with glass at the same level as A and B, under which glass survey lines, conforming to those on A and B, were set out.

19. *The Numbering of the Cross Section.*—The extension of the tanks raised the question as to how the new cross sections should be numbered: the numbering of A and B ran from the ends of the tanks, and it seemed best to run the numbers in C and D from the ends of these tanks, continuing this new numbering to the generators. On the other hand, as the long, narrow extensions in E and F were more in the nature of a tidal river than an estuary, the numbers in these were carried backwards 1, &c., from the ends of C and D, in which the cross sections preserved the same numbers as before.

20. *Appliances for Land Water.*—The introduction of land water, besides the extension of the pipes for its introduction, required certain arrangements for its regular supply in definite quantities. The water was to be taken from the town's mains. And in first laying down the pipes, it had been anticipated that it would be sufficient to regulate the supply by cocks against the pressure in the mains. Fresh water, regulated in this way, had been from the first supplied in small quantities into the generators, to ensure the level being kept properly. The experience thus gained showed that it was impossible to obtain even approximate regularity in this way, as the nearly closed cocks always got choked even within twenty-four hours.

To meet this it was arranged to supply the water through thin-lipped circular orifices under a small but constant head of water, which head can be regulated to the quantity required. The head of water in the tank from which the orifices discharge is regulated by a ball cock, which only differs from an ordinary ball cock in that the ball is not fastened directly on to the arm of the cock, but is suspended from it by a rod so arranged that the distance of the ball below the arm can be adjusted at pleasure. This arrangement has answered well. The cylinder in which the ball cock works

; made of sheet copper, with a water gauge in the form of a vertical glass ube, with a scale behind to show the height of water above the orifices, 'hich are made in the bottoms of two lateral projections from the sides of he cylinder. One of these orifices feeds the large, and the other the small ank. The streams from the orifices descend freely in the air for about inches, and are then caught in funnels on the tops of lead pipes leading o the respective tanks. The cylinder is fixed against a wall about 8 feet bove the floor, and conveniently near the tanks. Any obstruction in the ipes conveying the water to the tanks would be at once shown by the verflow of the funnel. The orifices are made with areas in proportion to he quantities to be supplied to their respective tanks. Then the supply ock connecting the ball cock with the main is fully opened, and the ball is djusted till the quantity supplied to one of the tanks is correct. The other s then measured; if this is not found correct, one of the holes is slightly nlarged until the proportions are correct.

This having once been done for an experiment, no further regulation is equired except to test the quantities and wipe the edges of the orifice. When the tanks are stopped for surveying, the water is shut off from the nain and simply turned on again on restarting.

21. *The Tide Gauges.*—In the experiments made last year a tide gauge vas used. This gauge consisted of a small tin saucer with a central depression n its bottom, in which a vertical wire rested, restraining any lateral motion n the float, the wire being guided vertically by a frame made to stand on he level surface of the class covers, while the wire passed down between wo of the covers opened for the purpose, the frame carrying a vertical cale. This gauge was used, both to adjust the levels of the water and to btain tide curves, by observing the heights of the tide at definite times, and hen plotting the curves with the heights of the tide as ordinates and the imes as abscissæ.

For the earlier experiments this year the same gauge was used for both urposes, and it has been used all through for the purpose of adjusting the evels of the water, automatic arrangements being used for drawing the tide urves.

In devising these automatic arrangements several difficulties presented hemselves, besides those inherent in all chronographic apparatus. Anything n the nature of standing apparatus was inadmissible, as it would interfere vith the working and adjusting of the tanks. The apparatus must be such s could be put up and taken down with facility, and hence could not admit f complicated arrangements. A pencil worked direct by a float with a lrum turning about a vertical axis by a clock, all to stand on the level lass surface, appeared the most desirable arrangement. In the first instance, a clock driving a detached vertical cylinder with a cord was kindly lent by Dr Stirling from the Physiological Laboratory of Owens College, and an arrangement of float and stand was constructed by Mr Bamford. The loan of this clock was temporary, and experience gained with it led to the purchase of an ordinary Morse clock from Latimer, Clark, & Co. at comparatively small cost. A pulley was fitted so that the clock would drive the borrowed cylinder. This clock did its work quite as well as the more costly instrument. Its rate of action varied considerably with the resistance of the apparatus to be driven, so much so that the curves taken at different times from the same experiment could not be compared by superposition. Still the action of the clock during the individual observations was sufficiently regular to give a fairly true tide curve, and it became obvious that it would be impossible to obtain any independent clock-driven apparatus that would give absolutely constant speeds such as would admit of the comparison of the curves taken from different parts of the estuary by direct superposition. To obtain such comparison it would be necessary to move the paper by the gearing which moved the generator.

22. *Compound Harmonic Tide Curves.*—On considering how best this might be done, it appeared that if the paper had a horizontal motion corresponding to the rise and fall of the generator while the pencil had a vertical motion corresponding to the rise and fall of the tide at any point in the tank, then, if the tide were in the same phase as the generator, the curve would be a straight line or an ellipse of infinite eccentricity, with a slope ($\tan\theta$) equal to the rise of tide divided by the horizontal motion imparted to the paper, while any deviation of phase would be shown by the character of the ellipse or closed curve described by the pencil, and that to obtain the *time-tidal* curve from such curves would be easy by projecting on to a circle, while for the purpose of comparison, and bringing out any difference of phase or deviation from the harmonic curves, such compound harmonic curves would be much more definite than the harmonic curves. This plan was therefore adopted with the happiest results, for, although it may take some study to become familiar with the curves, the obvious differences in these curves taken at different parts of the tanks, and at the same part at different stages of the progress towards a state of equilibrium are clearly brought out. The method also shows the similarity of the curves taken in the two tanks, or in different experiments at the corresponding places and corresponding numbers of tides run, as well as in the final states of equilibrium. The tide curves (Fig. 48, page 481) bring out emphatically the inter-dependence of the character of the tide on the arrangement of the sand, and the coincidence of a state of equilibrium of the sand with a particular tide curve at each part of the estuary.

In these experiments the balance of the tanks has been adjusted so as to make the time intervals of rise and fall of the generator equal, *i.e.* to make the motion of the generator harmonic, so that these compound harmonic curves are at all parts of the tank comparable with a simple harmonic motion. But it is important to notice that they are not essentially so, being merely comparable with the motion of the generator, so that if the generator were given a compound harmonic motion, such as that of the tide in the mouths of most estuaries, these curves would have a different dynamic significance. These curves would still be valuable as showing the state of progress and final similarity of the tidal motion at the same parts of the estuaries, but to bring out their dynamical significance it would be necessary to substitute a simple harmonic motion with the same period as that of the generator.

§ IV.—Description of the Experiments on the Movement of Sand in a Tideway from September 9, 1889, to September 1, 1890*.

23. *Continuation of Experiments VII., Tank A, and III., B,* (see Figs. 4, 5, 6, pages 441,...) *September* 7 *to October* 11.—These experiments were in progress at the time of the Newcastle Meeting of the British Association, and had so far advanced that tracings of the first surveys were exhibited and included in the First Report. So far as they went, they took an important place in the conclusions arrived at in that report, showing that with a vertical exaggeration of 100, the results obtained in the small tank (B) with rectangular estuaries, without land water, as to rate and general distribution of the sand, were closely similar to those obtained in A, and that the mean slopes, reduced to a 30-foot tide, in these experiments agreed with those obtained in A, with vertical exaggerations of 64. It was desirable to continue these experiments to see how far a state of equilibrium had been arrived at. This was accomplished by the assistance of Mr Foster, who kindly looked after the running of the tanks till the return of the author and Mr Bamford in October, and thus enabled a month, which would otherwise have been wasted, to be utilised, in obtaining an experience of the effect of about 100,000 tides after apparent equilibrium had been obtained in each tank. Daily records of the counters were taken, and, although there were several stops, the intervals of running gave the periods very constant.

The plans show but little alteration, except that the sand, particularly in B, had shifted upwards and accumulated somewhat at the head of the estuary, leaving the slope the same; a circumstance which would be accounted for by a difference in the level of the water, and which is also

* In the published report of these experiments it is not thought desirable to give the daily records of progress in the notebook.

indicated by the mean slope reduced to a 30-foot tide shown in Figures 2 and 3, page 440. The agreement of the slopes here shown as compared with the mean slope in the case of Experiment V., A, which has been introduced in this diagram for the sake of comparison, is quite as great as could be expected, considering the difficulties of the experiments, and affords very valuable evidence of the permanence of these slopes when once a condition of equilibrium has been attained.

In respect of the ripple the two tanks presented a very different appearance, which is clearly shown in the plans and sections. While the ripple in A was comparatively small and shallow, in B it was larger and deeper than anything previously noticed; that this was a symptom of the condition of B being on the verge of dissimilarity seemed probable, and to test this the period of B was increased from 23·85 to 26·5 seconds, and it was allowed to run on 16,000 more tides and again surveyed. Plan 3, page 443, shows the result; the ripple has increased in breadth though rather diminished in depth.

24. *Experiments to find the Limits to Similarity. Experiment IV., B, Fig. 7, page 444, October 22 to November 27.*—In this the rise of tide was 0·094 foot, and the vertical exaggeration as compared with a 30-foot tide 71. In Experiments I. and II., B, with a rise of tide 0·088 and a vertical exaggeration 68, described in the First Report, it had been found that the rate and manner of distribution of the sand did not correspond with that in the corresponding experiment in the larger tank, indicating that with an exaggeration 68 the tide of ·088 was somewhat below the limit of similarity. The determination of these limits being a primary object of the investigation, it appeared desirable to repeat these experiments with a slightly higher tide. In IV., B, the character of the action presented the same peculiarities as previously observed, but in a smaller degree, and the final state, as shown in the plans and in the curve of slopes (Figs. 2 and 3), is a much nearer approach to the general law, the conclusion being that in IV., B, the conditions were still below the limit, but nearer than in I. and II., B.

Experiment VIII., A, October 22 to November 14.—This was an experiment to determine the manner of action with the same horizontal scale as the first part of Experiment V., A, but half the rise of tide. Experiments I. and II., B, with a rise of tide of ·088 foot and a period of 36 seconds, being a vertical exaggeration of 68, had indicated that with this rise of tide a change in the manner of action had already set in, but it was none the less desirable to see what would be the character of the action and the final state of equilibrium well below this limit.

The rise of tide in VIII., A, was 0·088 foot and the mean level 0·138 foot from the bottom, and the period 70 seconds, the sand being placed level at a uniform depth of 1½ inch to Section 18 as in the previous experiments. The vertical exaggeration would thus be only 34.

The manner of action of the water on the sand was in this case essentially different from that in any previous experiments even in I. and II., B, although it presented characteristics which had been indicated in those experiments. Instead of the sand being in the first instance rippled over the whole surface a middle depression was formed, extending some way up the estuary, the bottom and sides of which were rippled; the rest of the sand soon became set and yellow. After 16,000 tides a survey was made and the experiment continued to 24,000, when another partial survey was made, showing very small alterations, and those nearly confined to the rippled channels. It was, in fact, clear that the apparent equilibrium was owing to the sand having become set, and that to proceed till real equilibrium was established would take an almost indefinite time.

As the setting of the sand, owing to the slow action of the water, appeared to play such an obstructive part, it seemed possible that better results could be obtained if the sand could be kept alive with waves. Accordingly the experiment was stopped, to be repeated with waves.

Experiment IX., Tank A, Plans 1, 2, 3, *Figs.* 8, 9, 10 (*with Intermittent Waves*), *November* 16 *to January* 4.—The conditions were the same as in Experiment VIII., with the addition of the waves.

This experiment presented the same characteristics as those observed in VIII., A. The rate of action did not fall off so rapidly or completely as in VIII., but was mainly confined to the channels; and, although the experiment was continued to 57,000 tides, the condition of equilibrium was far from being arrived at, owing to the setting of the sand. After the last survey a small stream of land water (one pint per minute) was admitted at the top of the estuary, without any perceivable effect for 1000 tides, whereupon the experiment was stopped.

Experiment V., B, Plan 1, *Fig.* 11, *p.* 448, *November* 21 *to December* 2.—This was the corresponding experiment in B to Experiment VIII. in A, the rise of tide being one-half inch (·042 foot), and the period 50 seconds, exaggeration 32. The characteristics were yet more definitely marked, rippling being entirely absent, and the action being entirely confined to the space between Sections 14 and 18.

Experiment VI., B, December 5 *to December* 9.—In this experiment the conditions were exactly the same as in Experiment V., B, except that the sand, instead of being laid level, was laid with a slope of 1 in 124, the slope corresponding to the theoretical condition of equilibrium as in the previous experiment. After 6757 tides with a mean period of 60·1 seconds the sand was not moved anywhere in the slightest degree.

Experiment VII., B, Plans 1 *and* 2, *Figs.* 12 *and* 13, *December* 9 *to January* 3.—This was a continuation of Experiment VI., with the tidal period diminished in the ratio 1 to $\sqrt{2}$ from 50 to 35·35.

The effect of changing the period would be to increase the vertical exaggeration, so that the slope of 1 in 124 would not be the theoretical mean slope of equilibrium as previously determined, which would be 1 in 87, so that any sensitiveness to the condition of equilibrium would be shown by the shifting up of the sand.

This commenced at once and continued until the mean slope was about 1 in 100 above Section 13.

The absolute quiescence of the sand in Experiment VI., B, when laid with the mean slope of equilibrium corresponding to the period, together with the increase of the slope with the increase of period in Experiment VII., B, indicates that, although, as shown in Experiment V., the limiting conditions under which the water could redistribute the sand from the level condition had been long passed, the conditions of equilibrium remained the same; or, in other words, that for a half-inch tide, with a period of 50 seconds —*i.e.*, an exaggeration of 32—with the sand originally distributed according to the theoretical slope of equilibrium, the sand will be in equilibrium, while if the sand be laid with a smaller slope the water will shift it, tending to institute the slope of equilibrium.

25. *Rectangular Estuaries with Land Water. Experiments X., A, and VIII., B, Figs.* 14, 15, 16, *and* 17, *January* 7 *to March* 10.—The conditions in Tank A were the same as in Experiment V., Plan 1. The sand lay 0·25 foot deep, height of mean tide 0·256, rise 0·176, tidal period 50·2 seconds. A tin saucer was placed on the sand under Section 1 in the middle of the estuary, and a stream of water (one quart per minute, about 1/170 of the tidal capacity of the estuary per tide) run into the pan.

During the early distribution of the sand the land water produced no apparent effect, but as the sand approached a condition of equilibrium, the effect of the fresh water in keeping a channel full of water at low tide, from the source all down the estuary, was very marked. The effect of this river in distributing the sand at the top of the estuary was also marked. The channel did not remain in one place; it gradually shifted from the middle towards one or other of the sides, cutting away high sandbanks until it followed along the end of the tank into the corner, and then flowed back diagonally into the middle. Then, after some 10,000 tides, a fresh channel would open out suddenly towards the middle of the estuary, and then proceed in the same gradual manner perhaps to the other side. This happened more than once during the progress of the experiment, which was carried to 85,000 tides. The different positions of the channels are apparent in the plans 1, 2, and 3 (Figs. 14 to 19). The comparison of these plans, and the accompanying sections with Plan 1, Experiment V., in the last report (Fig. 3, p. 403), shows but slight general effect of the land water—so slight, indeed, that it might pass almost unnoticed. This shows that the land water does not alter the greatest height of the banks or the lowest depth of the channels.

It will be noticed, however, in the plans, that the land water has lowered the general level of the sand in the middle of the estuary at the top, and raised it towards low water. This effect comes out in the mean reduced slopes shown in Figs. 2 and 3, p. 440. From these it appears that the effect of the land water, by continually ploughing up the banks at the top of the estuary, has been to disturb the previous state of equilibrium, lowering the sand near the top, and raising it further down the estuary.

In Experiment VIII., B, the conditions at starting were the same as those in IV., B, and one quart of land water in 2·8 minutes was admitted in the same manner as in X., A, the period being 35·4 seconds. The quantity of land water per tide was one-fourth the quantity in A, while the capacities of the estuaries are as 1 to 8, or the percentage of land water in B was 1·8 that of the tidal capacity at starting. After running 600 tides the rise of tide was increased from 0·094 to 0·097 foot without any alteration in the period. The experiment was then continued to 91,184 tides (Fig. 19).

The apparent effects of the land water observed were exactly the same in character as in A, but were decidedly greater on account of the larger quantity. The curves agree fairly with those in A.

26. *Experiments in short* V-*shaped Estuaries with and without Land Water.*—In the tanks A and B inner vertical partitions were introduced so as to form the upper end of the tank A into a symmetrical V, of length 6 feet and greatest breadth 4 feet; while that of tank B was formed in a similar manner into a V of length 3 feet and breadth 2 feet. The lengths of the tanks were thus unaltered, the tidal capacity being reduced to three-quarters of what it was before.

The sand was arranged in a similar manner to that previously adopted, except that the initial depth of the sand was 4 inches (0·33 foot in A) instead of 3 inches, and the scummers raised so as to maintain the water higher in a corresponding degree.

Experiments XI., A, and X., B, Figs. 20 *to* 23, *March* 18 *to April* 29.—In tank A the rise of tide was 176 and the period 47·20. The experiments were first started without land water. The observed character of the action was much the same as with the rectangular estuaries, being more intense towards the top of the V, and quieter at and below the broad end.

The first attempt in Tank B showed that, owing to the diminished capacity of the estuaries, the sand would not come down even so well as in corresponding experiments with rectangular estuaries. This led to the abandonment of Experiment IX., B, and starting X., with a rise of tide 0·110, without, however, altering the level of the sand. The experiments were continued in both tanks without land water until about 40,000 tides had been run, and Plans 1 and 2 had been taken. These plans show the similarity of the effects in the two tanks. They also show decidedly the character of the distribution of the sand in the V-shaped estuary. It will be seen that the extreme positions of the contours up the estuary are much the same as in the rectangular estuaries, while the extreme positions down the estuaries are very much increased. The low-water contours extend from Section 11 to Section 19, while in Experiment V., A, Plan 1, it extends from Section 11 to Section 13. The low-water channels are nearly the same depth at corresponding points all down the estuary in both experiments, while in the V estuaries the banks extend 6 to 7 miles (reduced to a 30-foot tide) further down.

After Experiments XI., A, and X., B, had proceeded to about 40,000 tides, corresponding quantities of land water were introduced at the tops of the estuaries, one quart in one minute in A, about 1/140 of the tidal capacity; in B one quart in 5·68 minutes, or about 1/140 of the tidal capacity. The tanks were then run on for 12,000 tides, and surveys for the plans 3 made (Figs. 24 and 25). The general effect of this land water, as shown in these experiments, is, as before, to lower the sand at the tops of the estuaries and slightly to raise it at the bottom. They were not, however, continued long enough to show a state of equilibrium. As in the rectangular estuaries, the detailed effects of the land water were much more observable than those shown in the surveys. The land water continually ploughed up the sand at the top of the estuary and kept the banks down, but owing to the narrowness of the estuary the general effects of this were not so striking as in the rectangular estuaries.

Experiments XII., A, and XII., B, with Land Water, Figs. 26 *to* 29, *April* 29 *to May* 19.—These were under conditions precisely similar to XI., A, and X., B; XI., B, with land water, was started, but owing to an accident it was restarted as XII., B.

Both experiments were run about 16,000 tides and then surveyed, and then run on about 16,000 more tides and surveyed again.

The plans are all very similar, and show but little difference from the plans 3 with land water in the previous experiments.

27. *Experiments in long V-shaped Estuaries without and with Land Water in Tanks C and D.*—Tank C was formed by extending A by adding a rectangular trough to the top, and so as to admit of partitions forming a V extending from Section 23 (12 A), and D was formed by extending B in a similar manner. The lengths of the tanks were thus extended 6 feet and 3 feet greater than A and B, while the capacities were the same as the original capacity of A and B.

The sand in C (A extended) was laid 4 inches deep from the top of the V to Section 28·5 C (17·5 A).

The sand in D (B extended) was laid $2\frac{1}{4}$ inches deep from the top of the V to Section 28·5 D (17·5 B).

Experiments I., C and D, Figs. 30 *to* 33, *May* 24 *to June* 16, *without Land Water.*—In C the tide was 0·162 foot, and the scummer was placed so that the mean tide when running was 0·008 foot above the initial level of the sand; this was not observed at the time, being a consequence of the land water raising the level of low water by the necessity of getting over the weir.

In D the tide was 0·105 foot and the mean tide was ·010 foot below the initial level of the sand. Thus reduced to a 30-foot tide, the initial depth of the sand was 5 feet higher in C than in B. The experiments were run for about 16,030 tides and surveyed, then restarted, when the level of water in C fell owing to a leak in the scummer.

This lowered the sand at the lower end of the estuary, and a partial survey was made, and then the experiment continued until both tanks had exceeded 30,000 tides. The results, as shown in the plans, are very much alike, considering the very considerable differences in the initial quantities of sand. Owing to the much higher level of the sand in D, the top of the V was much more silted up in the early part of the experiment, and the sandbanks were higher towards the bottom of the estuary. Otherwise both tanks show the same characteristics.

The highest point of the contour low water in the generator is still at Section 15, while the highest point of the contour at high water in the generator is at Section 4, so that the distance between the highest points of these sections was still about 11 miles, while the banks at low water extended down to Section 26.

Experiments II., Tanks C and D, with Land Water, Figs. 34 *to* 37, *June* 17 *to July* 8.—The conditions in these experiments were the same as in Experiments I., Tanks C and D, except that the scummer in D was altered, until the mean tide level was only ·003 foot above the initial height of the sand, and in Tank A ·002 foot above, while the rise of tide in A was slightly greater and that in B slightly less.

Surveys were taken at about 16,000 and 32,000 tides respectively; they are very similar, and the effects of the land water are, as before, to slightly raise the lower sand and lower the upper. At low water there was still water in the channels right up to the top of the estuary, and at high water there was what would correspond in a 30-foot tide with 10 or 12 feet of water at the top in the low-water channels.

28. *Experiments in long V-shaped Estuaries with straight tidal Rivers extending up from the top of the V with and without Water in Tanks E, F′, and F.*—Tank E was formed by opening out the partition boards in Tank C at the end of the V to a distance of 4 inches. That portion of the V below Section 12 remained as in Tank C, the position of the partition boards not being altered. At a section, 12·5, a small angle was formed, so that while the boards above the section remained straight their ends stood apart 4 inches instead of closing up to form a V. Tank C was extended by a trough 19 feet long, in which partition walls were constructed continuing the partitions in the lower portion up to a section, 38, above the zero in Tank C; these were straight, vertical boards, the distance between them contracting from 4 inches at the lower end to 1 inch at the end of the river.

Tank F′ was formed in a similar manner, except that the upper extension was bent through two sharp right angles so as to return along the side of the tank; and subsequently tank F was formed exactly similar to Tank E with half the dimensions.

Experiment with Land Water, I. and II., Tanks E and F, Figs. 38 *to* 47, *July* 11 *to July* 31.—In Tank E the sand was laid to a depth of 4 inches, the same as in C, from the upper end of the river, Section 38 down to Section 28. The rise of tide was 0·140 foot, and the mean level of the tide about ·016 foot above the level of the sand. The period 49 secs. and water 1 quart a minute, or 1/200 the tidal capacity per tide, was introduced at the upper end of the river.

In Tank F′ the sand was laid similar to that in Tank E, the rise of tide 0·1 foot, and the mean tide 0·006 foot above the level of the sand. The period being 30·04, land water, 1/200 the capacity of the estuary, was introduced at the top of the river.

In starting these experiments the effect of the tidal river was very marked. After the first tide in Tank E, some depth of water remained in the river, and a long way down the estuary, at low water, and the tide came up with a bore increasing in height all the way to the top of the river, and then returned with a bore to the lower end of the river. The bore, as before, soon died out over the greater part of the estuary, as the sand at the bottom became lower. And the bore gradually died out in the top of the V, until, as the number of tides approached 16,000, the bore only began to show at about Section 4, and ran up the river very much diminished from what it was originally.

Owing to the indraught and outflow of the river, the velocity of the water and its action on the sand was greater at the top of the V and the mouth of the river than at any part of the estuary, while for some way up the river, and all the way down the estuary, there was a large volume of water running at low water. The top of the river was ninety miles (reduced to a 30-foot tide) from the bottom of the estuary, and the tide did not commence to fall at the top of the river until after low water at the mouth, so that nearly all the tidal water in the river ran over the estuary during the low water. The delay in the return of the water from the river obviously played a most important part in the effects produced.

At the bottom of the estuary the sand came down much as usual, but it did not rise at the head of the estuary. For the first 10,000 tides the sand was all covered at low water and rippled with active ripples up to the end of the river, and it seemed as if no banks were going to appear. The sections of the sand appeared as nearly as possible horizontal. The level having lowered from the bottom of the estuary up to Section 15, from Section 15 to Section 3 it was somewhat raised, then from 3 upwards to 7 it was lowered, and thence up to the top of the river it was raised in a gradual slope. At about 12,000 tides two small banks appeared at low water, one on each side of the estuary at Section 13. Everything was perfectly symmetrical so far, but from this time the bank on the right of the estuary extended downwards, while that on the left extended upwards and a depression or channel formed between them extending across the estuary in a diagonal manner. This was the condition when at 16,000 tides the first survey was made.

As the running continued these banks continued to rise, that on the right downwards, that on the left upwards, until a distinct channel was formed from the mouth of the river down to Section 20, as shown in the second survey at 32,000 tides.

The level of the sand at the mouth of the river altered very little, diminishing during the first 10,000 tides, and then reassuming its original height, but the sand passed upwards through the mouth, and gradually raised the level in the river above, until there was only about 0·02 foot in the shallowest places at low water (corresponding to 5 feet on a 30-foot tide); this level was first reached at the top of the river and then gradually extended down to Section 19, which point it had reached at 32,000 tides, when the second survey was taken. In this condition the bore still reached the end of the river, raising the water 0·02 foot (5 feet on the 30-foot tide). Above Section 19 all motion of the sand had ceased, but below this the sand was still moving up when the experiment stopped. The bore still formed at the mouth, but very much diminished, and was very slowly diminishing. The final condition of the estuary shows the contour at low water in the generators extending up to Section 9, and the contour at high water in the generator to Section 11.

In tank F′, with the sharp turns in the river, the action of the sand at the bottom of the tank was at first sluggish, as in Experiment IV. In the top of the estuary and river the appearance of things for the first 10,000 tides was much the same as in Tank E, except that the ripple of the sand did not extend more than half-way up the river, and deep holes were formed at the bends, banks being formed between them. The bore, however, ran up to the end of the river until some time after the first survey was taken, and the tide still rose very slightly when the second survey was made, though the river was barred by a bank between the bends, by which the flood just passed in small channels at the sides. The sand had risen in the top of the estuary until it virtually closed the mouth of the tidal river, and the condition of the estuary resembled that obtained in Tank D. This virtually ended the experiment, but opportunity was taken to try the effect of a larger quantity of land water, which was increased to one quart in two minutes—*i.e.* nearly three times—and the experiment continued for 20,000 more tides without any material effect.

In Tank F the action at the lower end of the tank was again sluggish. At the top of the estuary and in the river the conditions of the sand were as near as possible similar to those in Tank E, but, as it came out, the mean level of the water, relative to the level of the sand, was some 5 feet (reduced to a 30-foot tide) lower in F than in E.

The appearances for the first 16,000 tides were the same as far as was observed; the ripple now extended up to the top of the river, and no banks formed at the mouth. Nevertheless, before the second survey was taken, the tide ceased to rise above the mouth of the river, proving that the previous failure to realise the same state in the small tank as in the larger, had not been entirely due to the bends in the river. The question remained whether it might not be owing to the higher level of the sand relative to the mean level of the tide.

This question brings into prominence a fact observed during all the experiments, but which had not, previous to the experiments on E and F, assumed a position of importance. This is the *gradual diminution of the rise of tide owing to the lowering of the sand.*

29. The rise of the tide depends not only upon the rise of the generator, but also upon the tidal capacity of the tank. This capacity is the product of the area of the surface at high water multiplied by the rise of tide, less the volume of sand and water above low water in the generator. Now in starting the experiments with the sand at the level of mean tide, not only is there much more sand above the level of low water in the generator than when the final condition of equilibrium is obtained, but the quantity of water retained on the top of the level sand is considerable, so that the tide rises considerably higher in the generator at starting than when the condition of equilibrium is obtained, which excess of rise gradually diminishes as the sand comes down at the lower end of the estuary.

Although the foot of the sand comes down pretty rapidly at the commencement of the experiment, owing to the surface being rippled, the water runs off slowly, and it is not till the sand at the end of the estuary has been raised, and a slope formed, that the water runs down freely at low water, so that during the early part of the experiment not only is the rise of tide at the head of the estuary high, but also the low tide and the mean level of the tide. The result is that the mean level of the water at the head of the estuary is higher during the early part of the experiment. These changes in the tide at different parts of the estuary and at different stages of the tide are well shown by the automatic tide curves, page 481. As the sand is rising at the top of the estuary, the result of the high water is to raise the first banks above the level to which the tide finally rises.

As these banks come out and the ripple is washed off, leaving smooth surfaces and channels, from which the water runs, and clean dry banks, the mean level as well as the rise of tide falls, leaving the tops of the bank, which were at first covered, high and dry.

These effects were much greater in Experiments C and D than in A and B, and still more marked in E, F, and F′. In E, F, F′, the plans 1 and 2, taken at 16,000 and 33,000 tides respectively, show the difference in the level of the sand at the mouths of the respective rivers. In Tank E the rise of tide at the mouth of the river was observed to be 0·02 higher at 16,000 than at 30,000 tides, and in Tanks F and F′ at 16,000 tides there was a bore which ran up to the top of the river, while at 33,000 tides the sand at the mouth was not covered at high water.

It thus seems that the condition of things which follows from starting with the sand level, and a constant height of low water, is to institute a distribution of sand at the top of the estuary, corresponding to a state of equilibrium with a higher tide than that which ultimately prevails; and the greater the initial height of the sand relative to the mean level of the water the greater will be this effect. That this action tends to explain the closing of the mouths of the rivers in Tanks F′ and F and not in E is clear. But it is not clear that this is the sole explanation; the conditions in F′ and F were not far removed from the limits of similarity obtained in the rectangular tanks, and it is not clear that these limits may not be somewhat different in the long estuaries with tidal rivers. This is a matter which requires further experimental examination, for which there has not been time.

30. *Experiment II. in E and F, Figs.* 42 *to* 45, *without Land Water, August* 5 *to September* 1.—These experiments have been made under the same conditions as in I. E and F, except for the land water. The general appearance of the progress of the experiments was nearly the same, and Plan 1 shows little difference. But as the experiment in E proceeded, it became clear that the river was going to fill up gradually from the end. The bore no longer reaches the end at 16,000 tides, while it had ceased to exist and the tide had ceased to rise at Section 11 in the river at 32,000 tides, the end of the estuary also having filled up, the action in F being nearly the same. Thus we have evidence similarly shown by both estuaries that, although the fresh water produces little effect on the condition of equilibrium of a broad estuary, the existence of a long tidal river above the estuary does produce a great effect on the level of the low-water channels in the upper portions of the estuary, and that land water, even in such small quantities, is effective to keep open a long tidal river emptying into a sandy estuary or bay*.

* For continuation, see p. 482.

TABLE I.—GENERAL CONDITIO[NS]

Shape of the Estuary	Per-centage of Land Water	References: Experiment	References: Tank	References: Plan	References: Figure	Period in seconds	Horizontal scales: 1 in.	Horizontal scales: Inches to a mile	Vertical scale 1 in.	Rise of tide in feet
Rectangular	0·0	VII	A	2	4	33·5	17,600	3·58	177	0·170
	,,	III	B	2	5	23·8	33,600	1·88	327	0·094
	,,	,,	,,	3	6	23·8	33,600	1·88	327	0·094
	,,	IV	,,	1	7	34·4	23,300	2·71	327	0·094
	,,	IX	A	1	8	69·3	10,500	6·02	333	0·090
	,,	V	B	1	11	50·5	23,600	2·68	720	0·042
	,,	IX	A	2	9	69·3	12,400	5·08	379	0·080
	,,	,,	,,	3	10	67·3	12,600	5·02	366	0·082
	,,	VII	B	1	12	34·0	39,200	1·57	986	0·030
	,,	,,	,,	2	13	34·0	39,200	1·57	986	0·030
	0·6	X	A	1	14	50·2	11,500	5·49	171	0·176
	1·2	VIII	B	1	16	35·4	22,000	2·87	309	0·097
	0·6	X	A	2	15	48·6	11,900	5·30	171	0·176
	1·2	VIII	B	2	17	34·5	22,600	2·8	309	0·097
	0·6	X	A	3	18	48·6	11,900	5·30	171	0·176
	1·2	VIII	B	3	19	34·5	22,600	2·8	309	0·097
Short V-shaped	0·0	XI	A	1	20	47·5	12,400	5·10	177	0·170
	,,	X	B	1	22	35·4	20,700	3·05	273	0·110
	,,	XI	A	2	21	47·2	12,670	5·01	181	0·166
	,,	X	B	2	23	35·4	20,700	3·05	273	0·110
	0·7	XI	A	3	24	47·2	12,400	5·08	177	0·170
	0·7	X	B	3	25	34·0	21,800	2·90	280	0·107
	0·7	XII	A	1	26	48·2	12,300	5·15	179	0·168
	0·7	XII	B	1	28	34·2	21,700	2·91	280	0·107
	0·7	XII	A	2	27	47·0	12,700	5·00	182	0·165
	0·7	XII	B	2	29	34·2	21,900	2·88	286	0·105
Long V-shaped	—	I	C	1	30	49·8	12,100	5·22	185	0·162
	—	I	D	1	32	35·9	20,900	3·03	285	0·105
	—	I	C	2	31	46·2	13,200	4·78	190	0·158
	—	I	D	2	33	34·4	21,800	2·90	286	0·105
	0·6	II	C	1	34	48·4	12,500	5·04	188	0·160
	0·6	II	D	1	36	34·6	22,200	2·85	300	0·100
	0·6	II	C	2	35	48·4	12,500	5·04	188	0·160
	0·6	II	D	2	37	34·6	22,200	2·85	300	0·100
Long with Tidal River	0·5	I	E	1	38	48·9	13,100	4·82	208	0·143
	0·5	I	F	1	39	30·0	25,800	2·45	313	0·096
	0·5	I	E	2	40	47·8	13,400	4·70	208	0·143
	0·5	I	F	2	41	30·0	24,700	2·56	313	0·096
	0·0	II	E	1	42	47·9	13,500	4·67	214	0·140
	,,	II	F	1	43	31·5	25,400	2·49	327	0·091
	,,	II	E	2	44	47·9	13,600	4·64	217	0·138
	,,	II	F	2	45	30·3	26,200	2·41	321	0·093
	0·5	I	F′	1	46	30·1	25,500	2·48	300	0·100
	0·5	I	F′	2	47	30·1	25,700	2·46	305	0·098

AND RESULTS OF THE EXPERIMENTS.

Criterion of similarity $C=h^5e$	Height of initial sand in feet	Height of mean tide in feet	Number of tides from the start	Action of the water on the sand in forming the bed at the lower end of the estuary		
				Manner	Rate	Final state
0·490	0·25	0·265	93,839	General	Normal	—
0·083	0·125	0·140	99,388	General	Normal	Normal
0·083	0·125	0·140	130,176	—	—	Large ripple
0·058	0·125	0·130	16,344	Nearly normal	Nearly normal	Nearly normal
0·023	0·125	0·1325	13,078	Very partial	Very slow	—
0·002	0·65	0·065	17,919	—	Zero	—
0·016	0·125	0·142	36,776	—	—	—
0·019	0·125	0·141	78,986	—	—	Not reached
0·001	Slope 1	0·065	17,424	—	Zero	—
0·001	in 124	„	39,727	—	—	Nearly normal
0·252	0·25	0·256	19,437	Normal	Normal	—
0·064	0·125	0·148	18,332	Nearly normal	Nearly normal	—
0·362	0·25	0·256	42,820	—	Normal	Normal
0·066	0·125	0·148	68,861	—	—	—
0·362	0·25	0·256	76,273	See description	—	See description
0·066	0·125	0·148	91,184	See description	—	See description
0·346	0·333	0·337	17,206	Normal	Normal	—
0·101	0·166	0·179	17,879	—	Normal	—
0·320	0·333	0·348	39,809	—	—	Normal
0·101	0·166	0·169	40,268	—	—	Normal
0·343	0·333	0·348	60,243	Normal	Normal	Normal
0·095	0·166	0·169	57,024	Normal	Normal	Normal
0·327	0·333	0·340	16,538	Normal	Normal	—
0·095	0·166	0·168	15,981	Normal	Normal	—
0·315	0·333	0·343	31,991	Normal	Normal	Normal
0·081	0·166	0·175	35,129	Normal	Normal	Normal
0·278	0·333	0·341	16,943	Normal	Normal	—
0·084	0·187	0·179	16,383	Nearly normal	Nearly normal	—
0·275	0·333	0·345	30,584	Normal	Normal	Normal
0·088	0·187	0·179	35,344	Nearly normal	Nearly normal	Nearly normal
0·274	0·333	0·344	16,908	Normal	Normal	—
0·074	0·187	0·190	18,128	Nearly normal	Nearly normal	—
0·274	0·333	0·335	31,127	Normal	Normal	Normal
0·074	0·187	0·190	31,928	Nearly normal	Nearly normal	Nearly normal
0·185	0·333	0·350	16,368	Normal	Normal	—
0·073	0·187	0·191	16,577	Partial	Sluggish	—
0·189	0·333	0·337	32,635	—	Normal	Normal
0·073	0·187	0·191	32,880	—	—	Ripple large
0·174	0·333	0·349	15,871	Normal	Normal	—
0·060	0·187	0·193	17,184	Partial	Sluggish	—
0·163	0·333	0·349	32,501	—	—	Normal
0·066	0·187	0·192	29,947	—	—	Ripple large
0·085	0·187	0·187	16,577	Partial	Sluggish	—
0·080	0·187	0·187	32,677	—	—	Ripple large

Measured Heights of Contours shown on the Plan	TANK A					
	Experiment V., Plan 4			Experiment VII., Plan 2		
	Height (reduced to a 30-foot Tide) of Contours from L.W.	Mean Horizontal Distance of Contours from the Contour at 30 feet above L.W.	Horizontal Distances reduced to a 30-foot Tide	Height (reduced to a 30-foot Tide) of Contours from L.W.	Mean Horizontal Distance of Contours from the Contour at 30 feet above L.W.	Horizontal Distances reduced to a 30-foot Tide
Feet	Feet	Unit 6 inches	Miles	Feet	Unit 6 inches	Miles
1	—	−0·975	−1·65	—	−1·792	−3·003
0·176	30·00	0·00	0·00	30·000	0·000	0·000
0·146	24·39	0·79	1·355	24·546	0·647	1·133
0·116	18·68	1·86	3·20	19·092	1·254	2·171
0·086	13·00	2·96	5·07	14·638	2·356	4·085
0·056	7·46	4·64	7·95	9·184	3·724	6·447
0·026	1·87	6·63	11·38	3·730	5·428	9·397
−0·004	− 3·74	8·43	14·50	− 1·724	7·467	12·930
−0·034	− 9·35	10·30	17·80	− 7·178	9·283	16·070
−0·064	−15·00	12·17	21·60	−12·632	11·780	20·400
−0·094	−20·80	13·60	23·40	−18·086	14·003	24·235
−0·124	−26·20	15·88	27·30	—	—	—
	Experiment X., Plan 1			Experiment X., Plan 2		
Feet	Feet	Unit 6 inches	Miles	Feet	Unit 6 inches	Miles
1	—	−0·690	−0·774	—	−1·302	−1·167
0·176	30·000	0·000	0·000	30·000	0·000	0·000
0·146	24·886	0·741	0·810	24·886	0·665	0·752
0·116	19·772	2·147	2·347	19·772	1·900	2·149
0·086	14·658	4·256	4·652	14·658	3·648	4·124
0·056	9·544	6·916	7·560	9·544	6·631	7·507
0·026	4·430	9·880	10·800	4·430	9·101	10·290
−0·004	−0·684	11·533	12·606	−0·684	11·227	12·594
−0·034	−5·798	13·737	15·013	—	—	—
	TANK B					
	Experiment III., Plan 2			Experiment IV., Plan 1		
Feet	Feet	Unit 3 inches	Miles	Feet	Unit 3 inches	Miles
1	—	−3·540	−5·643	—	−0·994	−1·124
0·094	30·000	0·000	0·000	30·000	0·000	0·000
0·079	25·213	0·760	1·240	25·213	0·665	0·760
0·064	20·426	1·330	2·163	20·426	1·558	1·773
0·049	15·639	2·052	3·340	15·639	2·185	2·487
0·034	10·852	3·249	5·290	10·852	4·142	4·714
0·019	6·065	4·332	7·044	6·065	6·859	7·806
0·004	1·278	6·061	9·854	1·278	9·766	11·120
−0·011	− 3·509	7·828	12·727	−3·509	12·046	13·710
−0·026	− 8·296	9·291	15·110	−8·296	—	—
−0·031	−13·083	11·341	18·430	—	—	—
	Experiment VIII., Plan 1			Experiment VIII., Plan 2		
Feet	Feet	Unit 3 inches	Miles	Feet	Unit 3 inches	Miles
1	—	−0·595	−0·621	—	−1·925	−2·062
0·097	30·000	0·000	0·000	30·000	0·000	0·000
0·082	25·360	0·608	0·634	25·360	0·988	1·060
0·067	20·720	2·090	2·181	20·720	1·672	1·792
0·052	16·080	3·268	3·410	16·080	2·983	3·197
0·037	11·440	5·224	5·472	11·440	5·168	5·538
0·022	6·800	8·987	9·378	6·800	8·398	9·000
0·007	2·160	11·400	11·896	2·160	11·285	12·100
−0·008	−2·480	13·148	13·720	−2·480	13·108	14·050
−0·023	—	—	—	−7·120	14·535	15·570

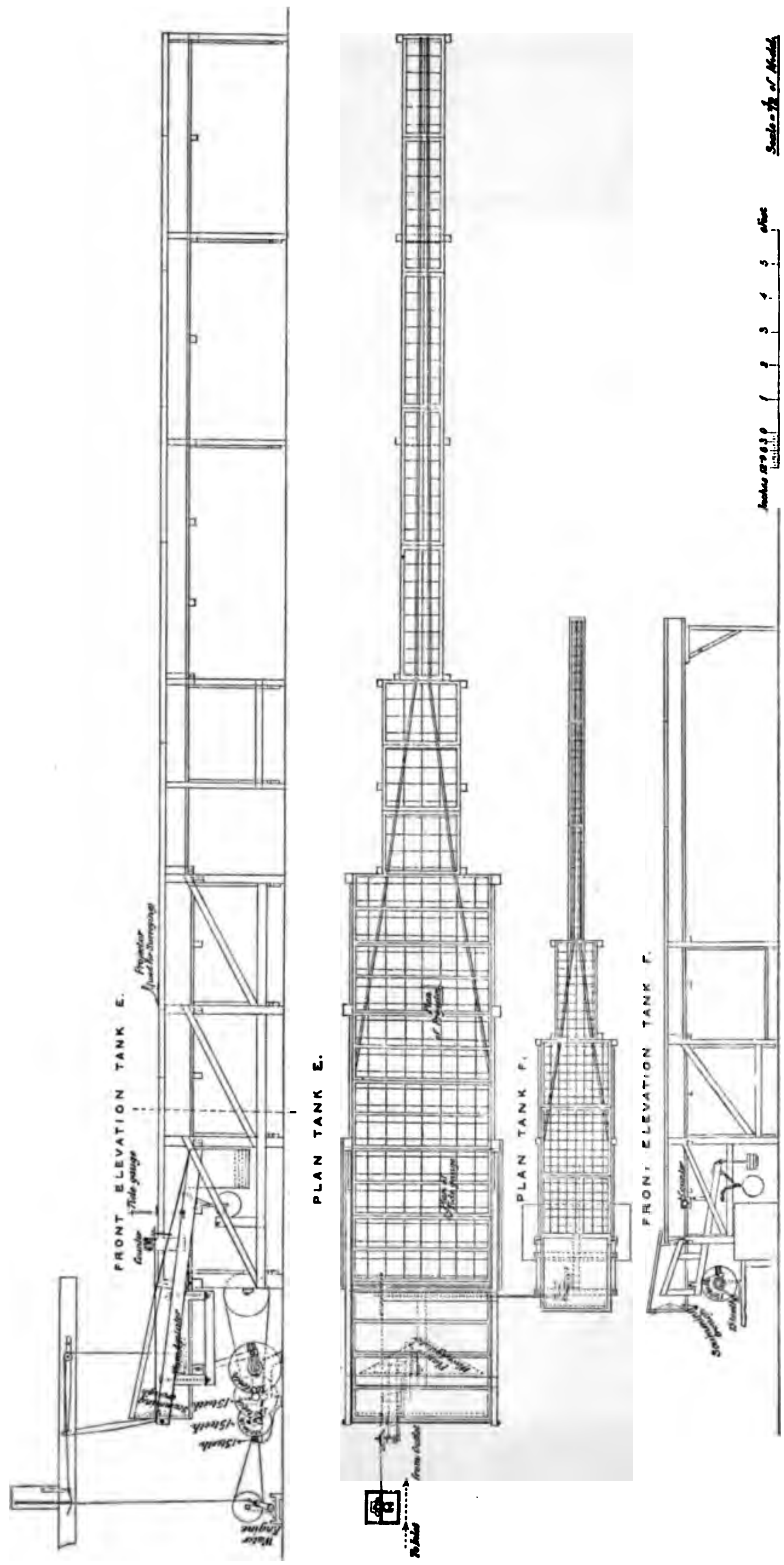

FRONT ELEVATION TANK E.
PLAN TANK E.
PLAN TANK F.
FRONT ELEVATION TANK F.

Fig. 1.

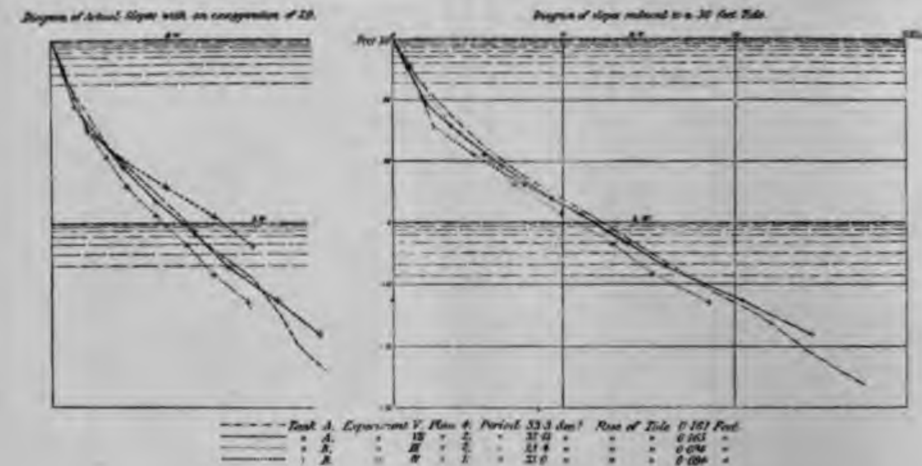


Diagram of Actual Slopes with an exaggeration of 29.
Diagram of Slopes reduced to a 30 feet Tide.
Tank A. Experiment V. Plan 4. Period 33·3 Secs Rise of Tide 0·161 Feet.

Fig. 2.

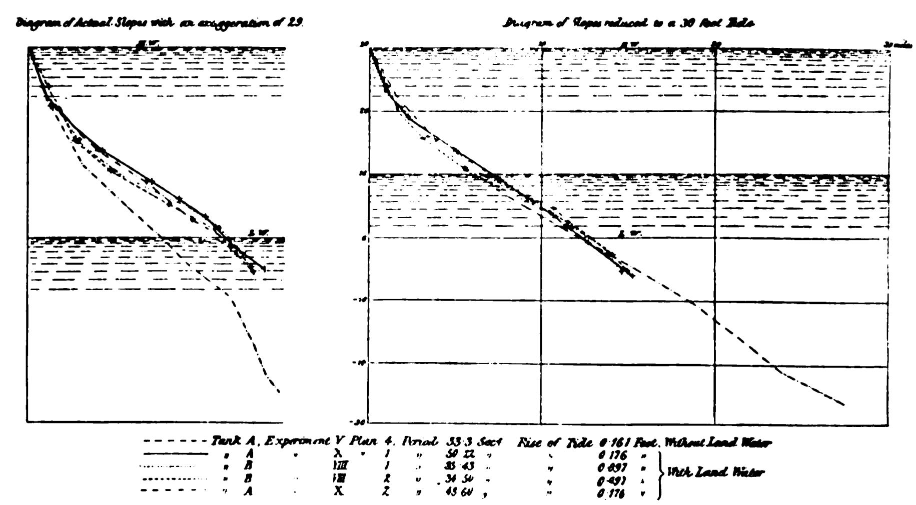


Diagram of Actual Slopes with an exaggeration of 29.
Diagram of Slopes reduced to a 30 feet Tide.
Tank A. Experiment V Plan 4. Period 33·3 Secs Rise of Tide 0·161 Feet. Without Land Water
A X 1 50·22 0·176
B VIII 1 35·43 0·097
B VIII 2 34·50 0·491
A X 2 45·60 0·176
With Land Water

Fig. 3.

On a 30 ft. tide, distance between the sections represents about 1·6 miles.

SERIES I WITHOUT LAND WATER.

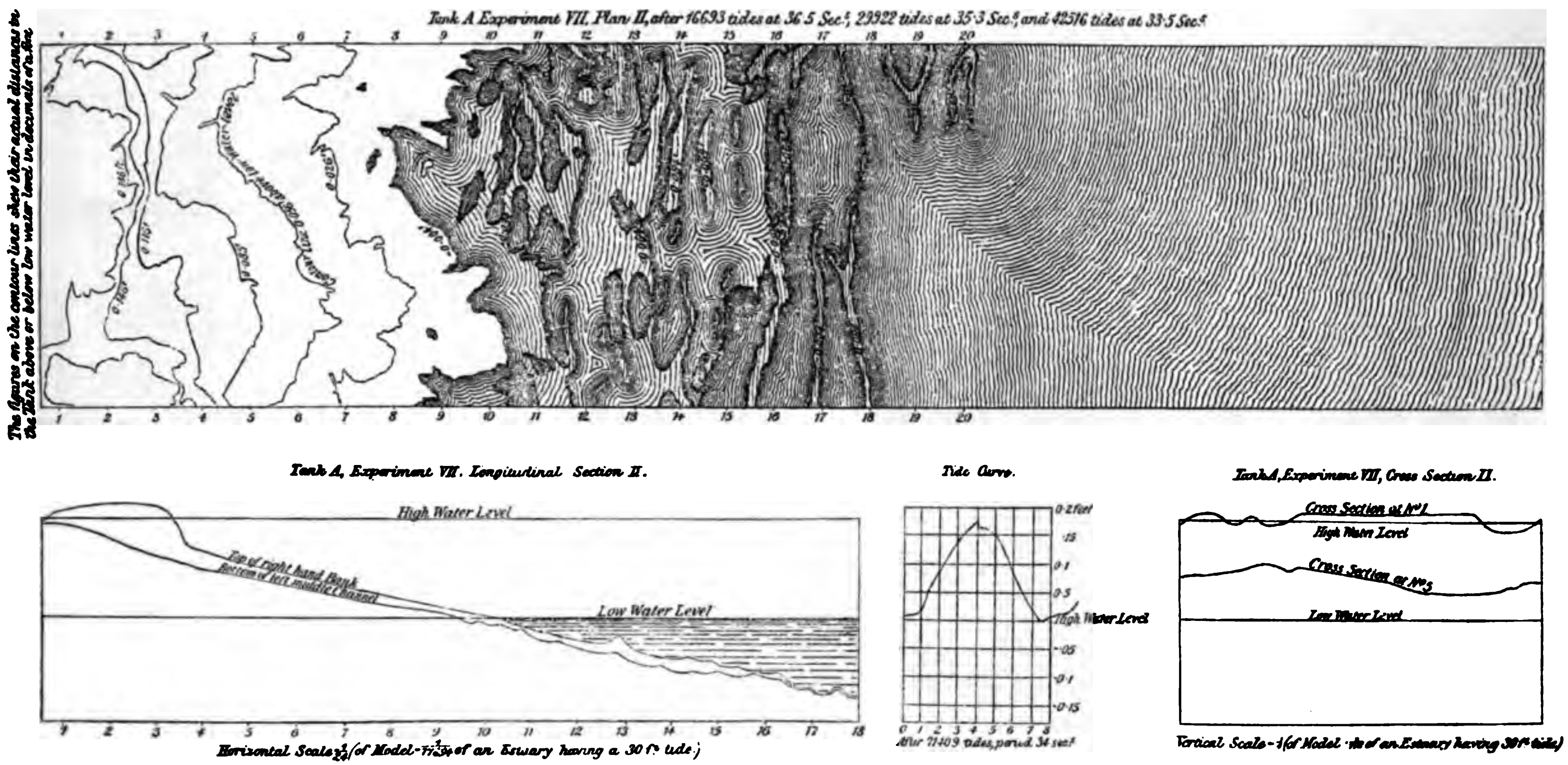


Fig. 4.

On a 30 ft. tide, distance between the sections represents about 1·6 miles.

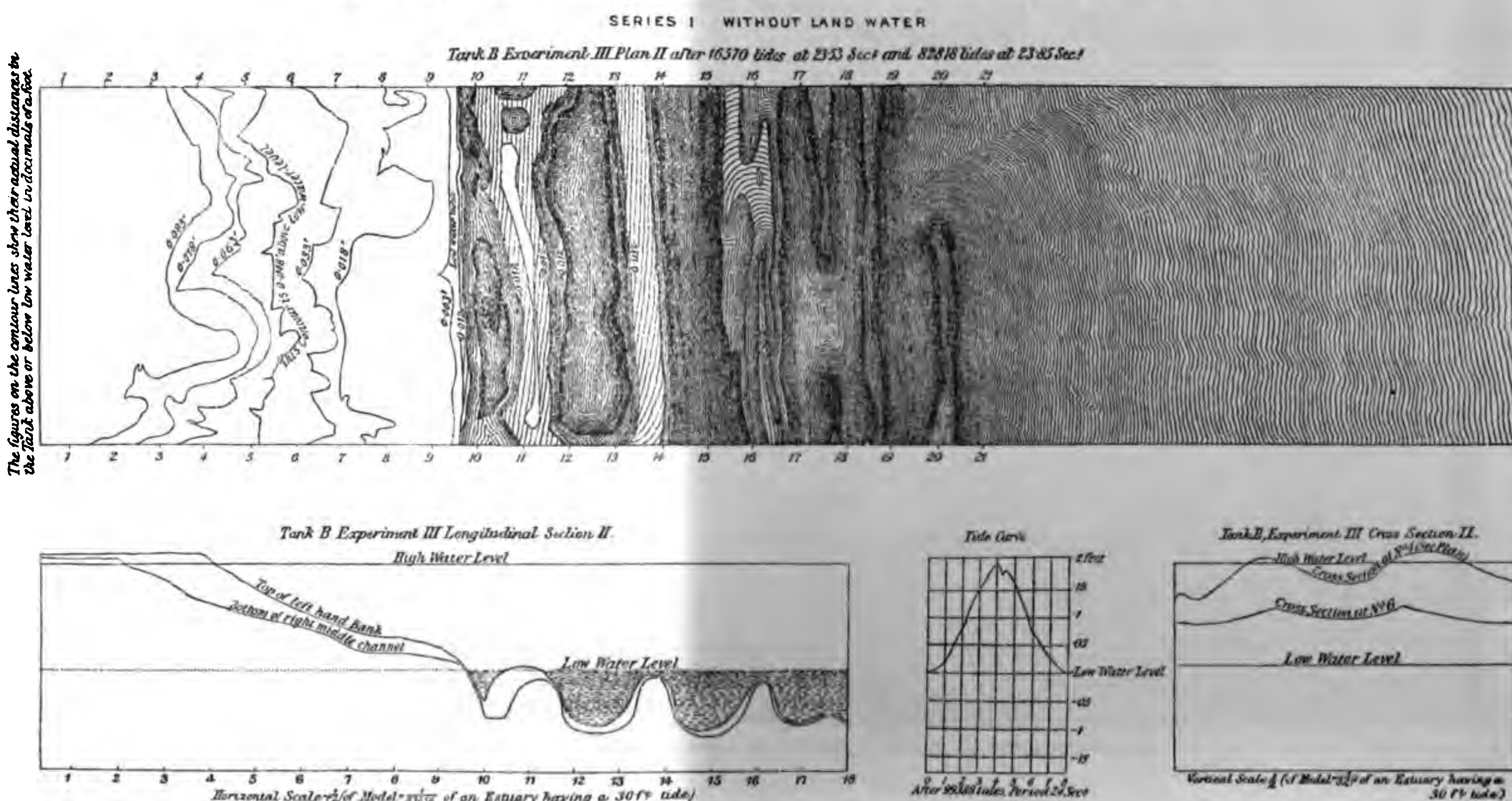
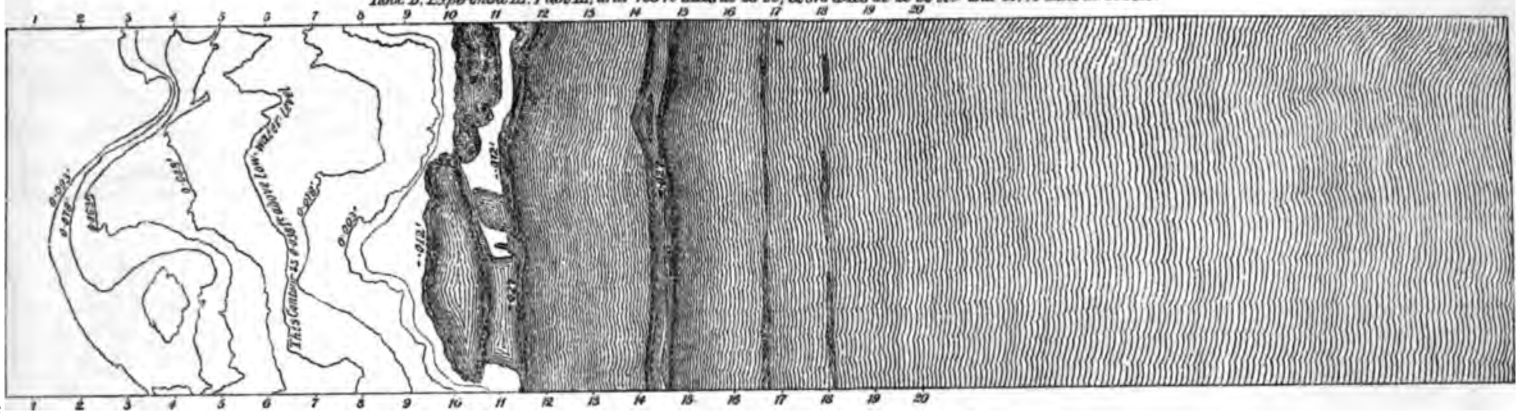


Fig. 5.

On a 30 ft. tide, distance between the sections represents about 1·6 miles.

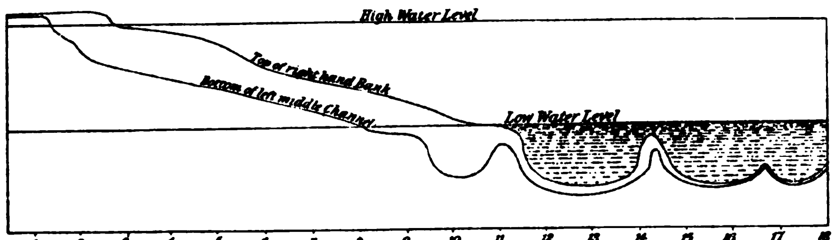
SERIES I WITHOUT LAND WATER.

Tank B, Experiment III. Plan III, after 16570 tides at 23·53, 82818 tides at 23·83 sec^s and 30176 tides at 26·2 sec^s



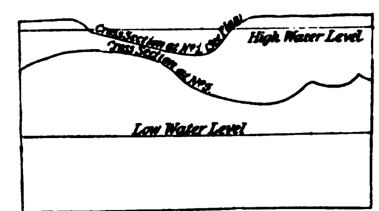
The figures on the contour lines shew their actual distances in the Tank above or below low water level in decimals of a foot.

Tank B, Experiment III, Longitudinal Section III,



Horizontal Scale 1/12 (of Model · 1/36312 of an Estuary having a 30 ft. tide.

Tank B, Experiment III, Cross Section III



Vertical Scale 1/2 (of Model · 1/327 of an Estuary having a 30 ft. tide)

Fig. 6.

On a 30 ft. tide, distance between the sections represents about 1·11 miles.

SERIES I. WITHOUT LAND WATER.

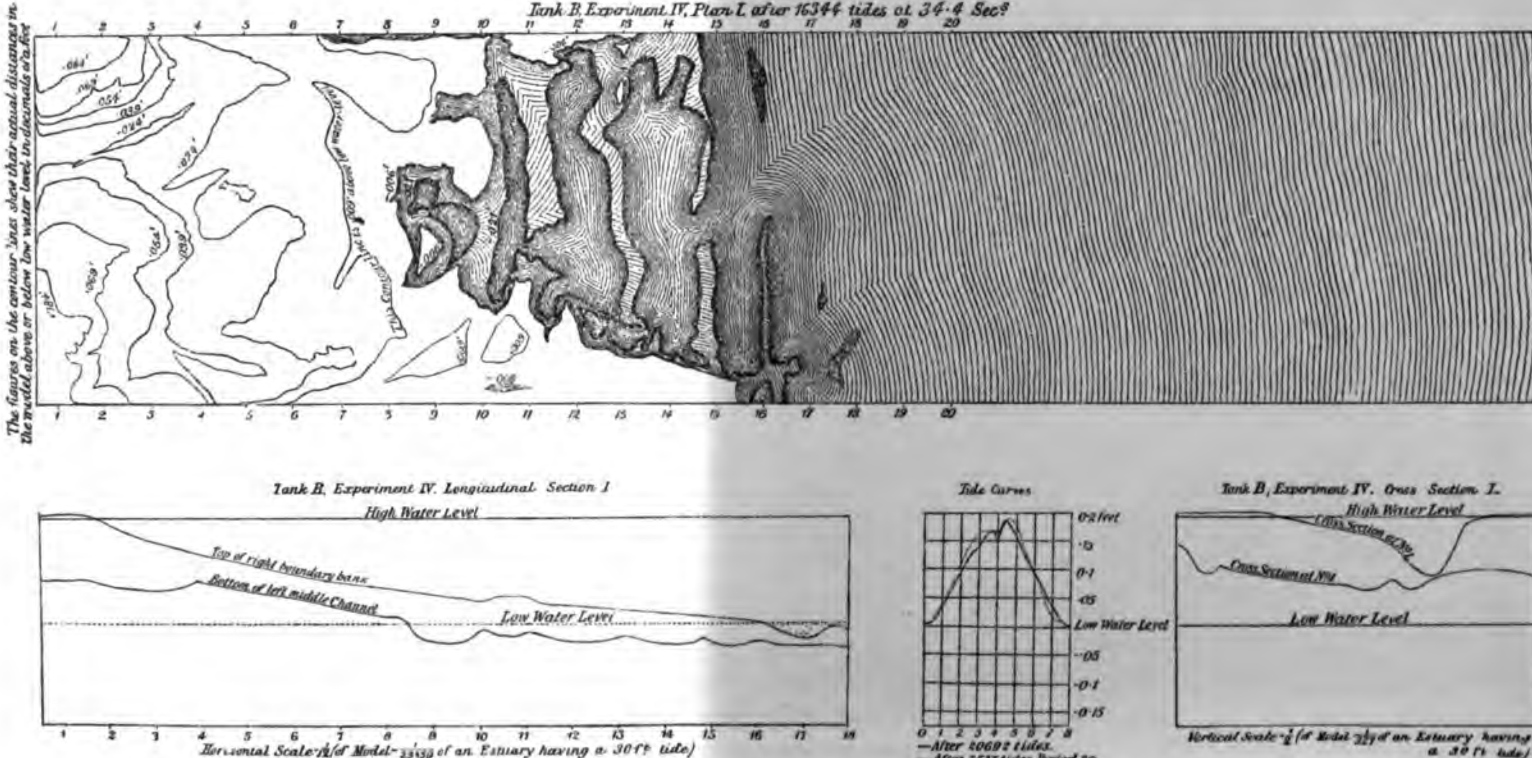


Fig. 7.

On a 30 ft. tide, distance between the sections represents about 1·1 miles.

SERIES I WITHOUT LAND WATER.

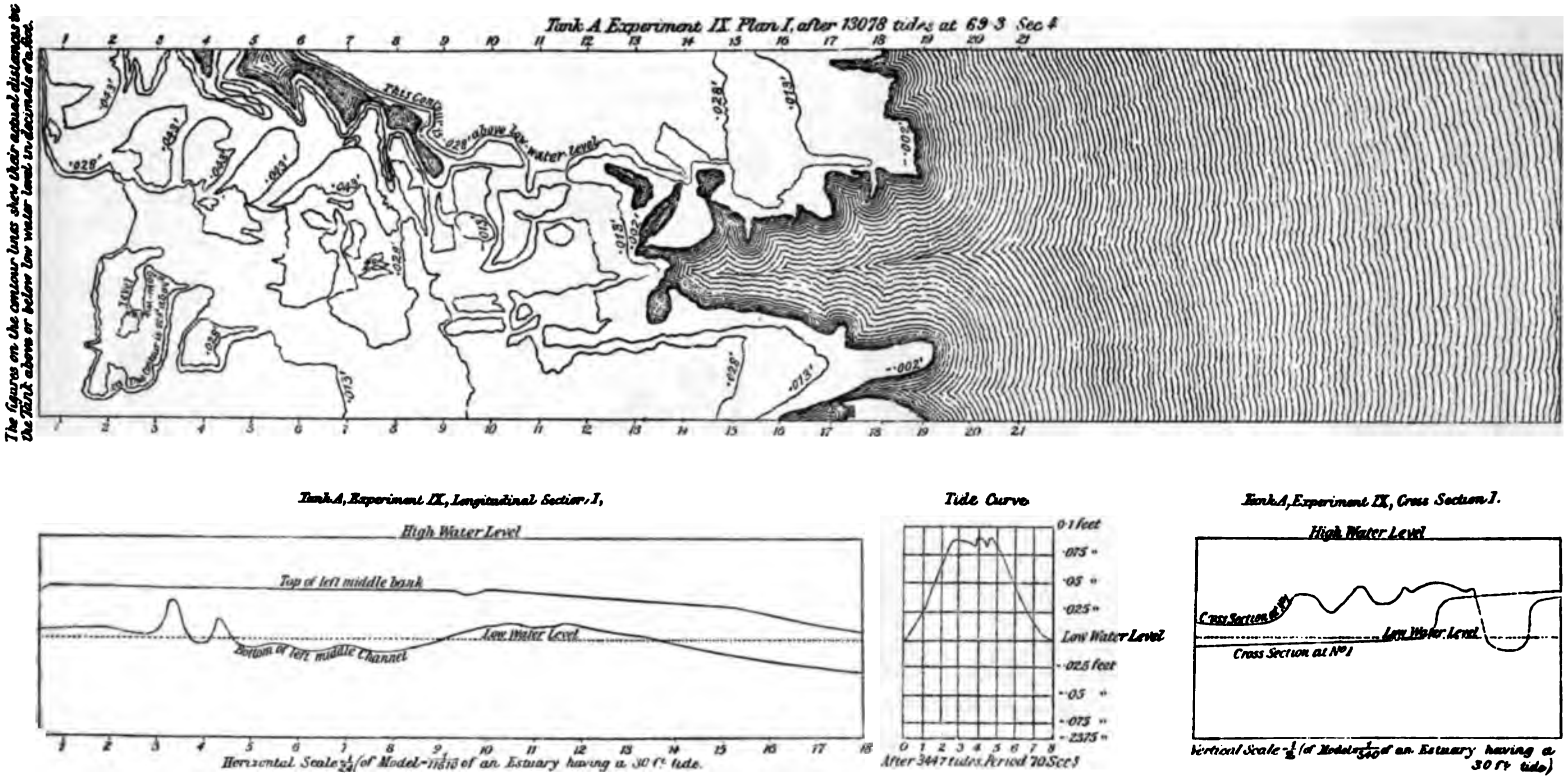


Fig. 8.

On a 30 ft. tide, distance between the sections represents about 1·1 miles.

SERIES I WITHOUT LAND WATER.

Tank B Experiment V. Plan I. after 17919 tides at 50·52 Secs.

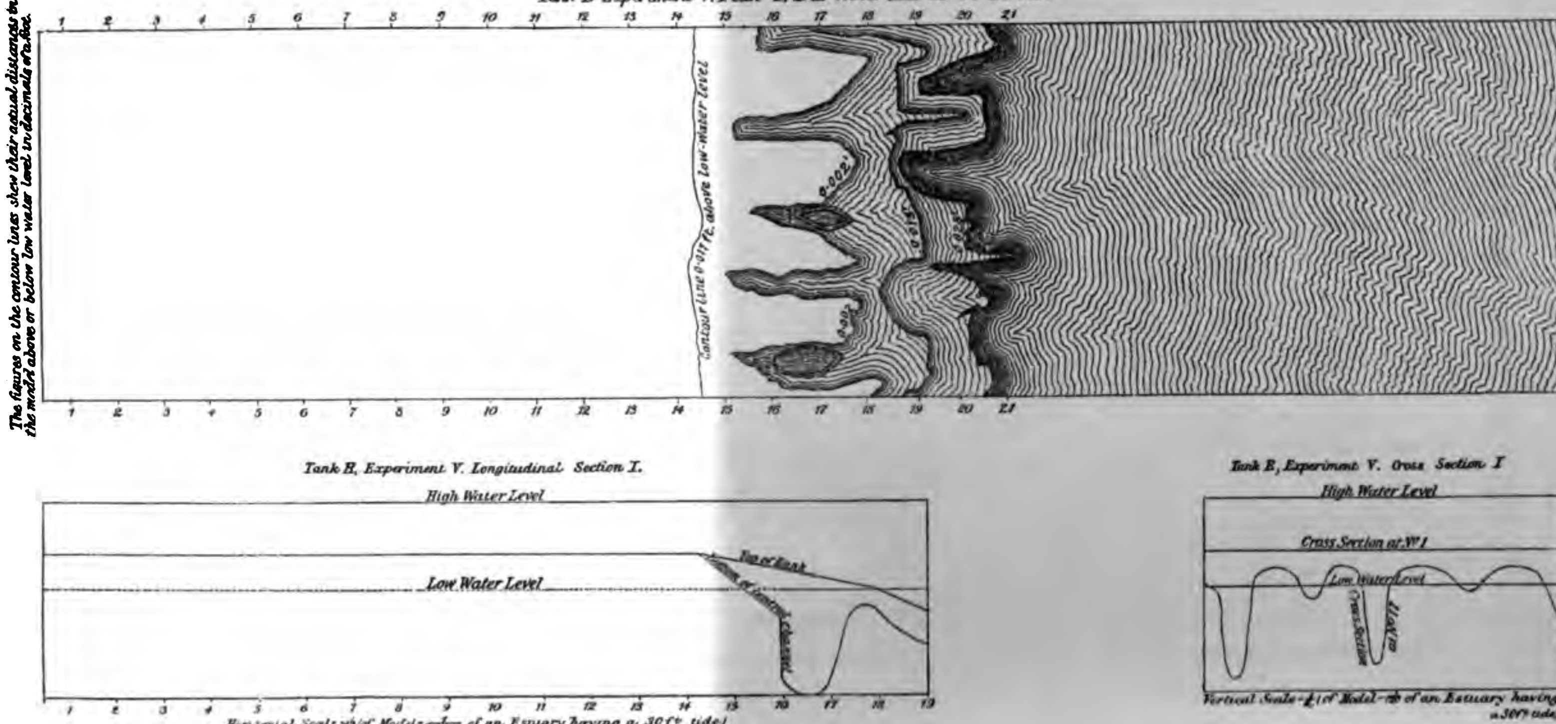
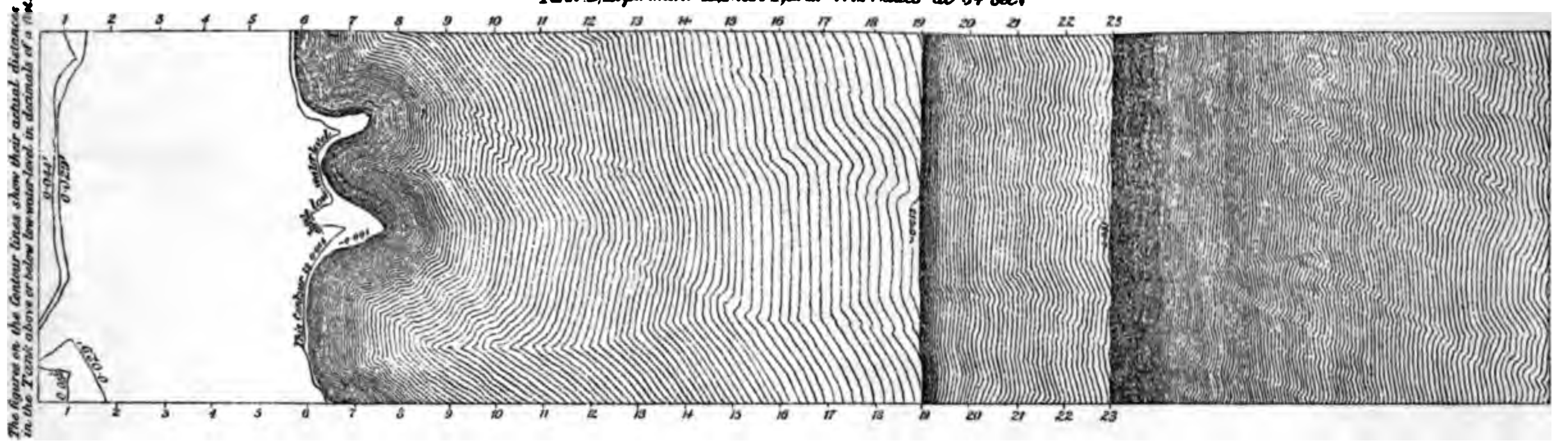


Fig. 11.

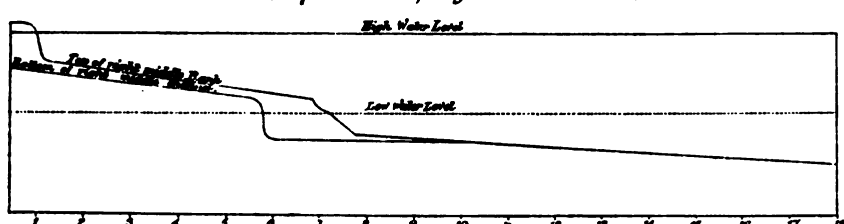
On a 30 ft. tide, distance between the sections represents 1·91 miles.

SERIES I WITHOUT LAND WATER.

Tank B, Experiment VII, Plan I, after 17424 tides at 34 Secs

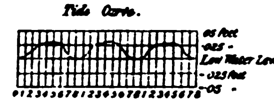


Tank B, Experiment VII, Longitudinal Section I.



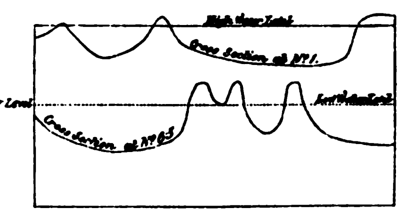
Horizontal Scale · 1/… (of Model · … of an Estuary having 30 ft tide)

Tide Curve.



After 47331 tides period 35 Secs

Tank B, Experiment VII, Cross Section I.



Vertical Scale · Full Size of Model = 1/116 of an Estuary having 30 ft tide

Fig. 12.

On a 30 ft. tide, distance between the sections represents 1·91 miles.

SERIES I. WITHOUT LAND WATER.

Tank B, Experiment VII, Plan II, after 17424 tides at 34 Secs and 39727 tides at 36·3 secs

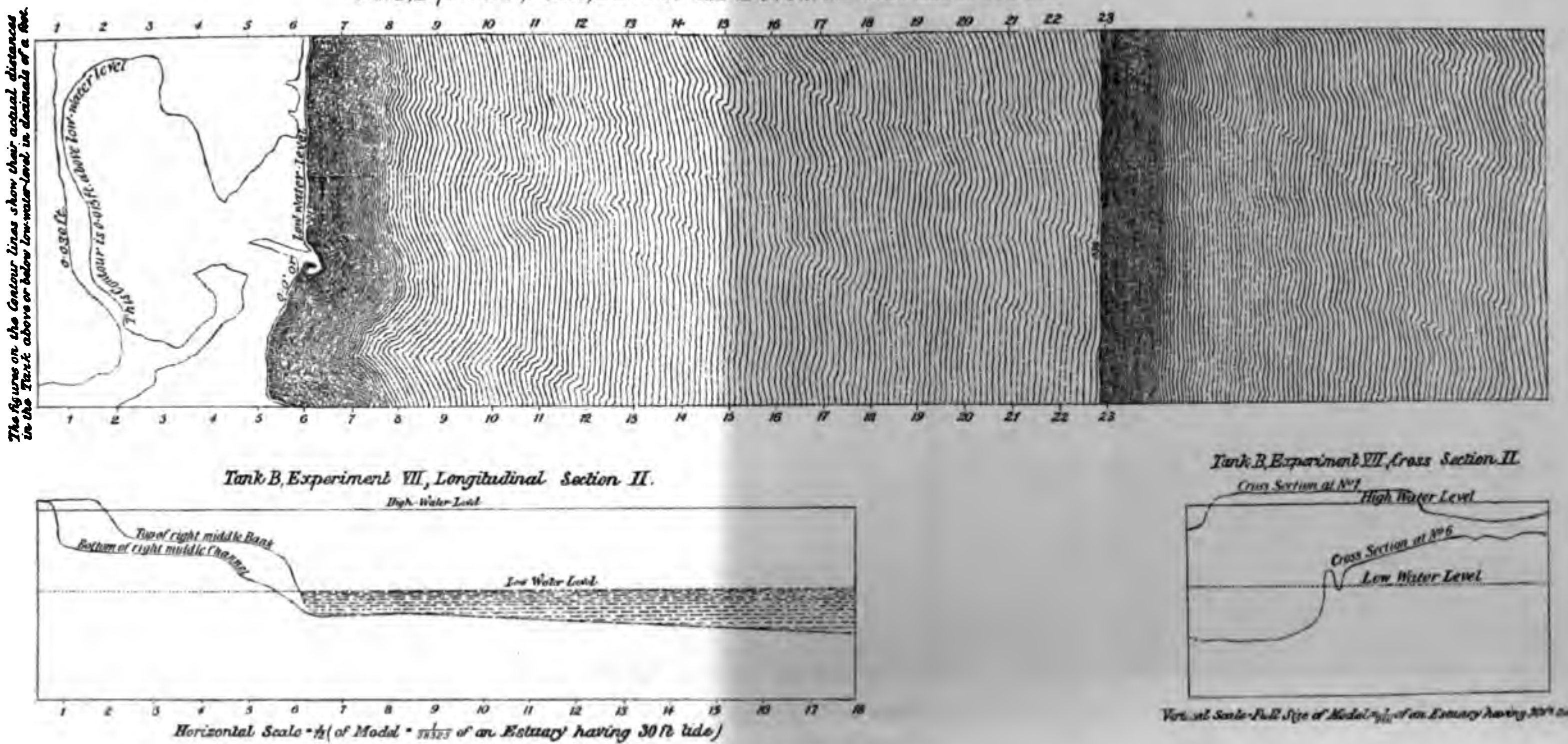


Fig. 13.

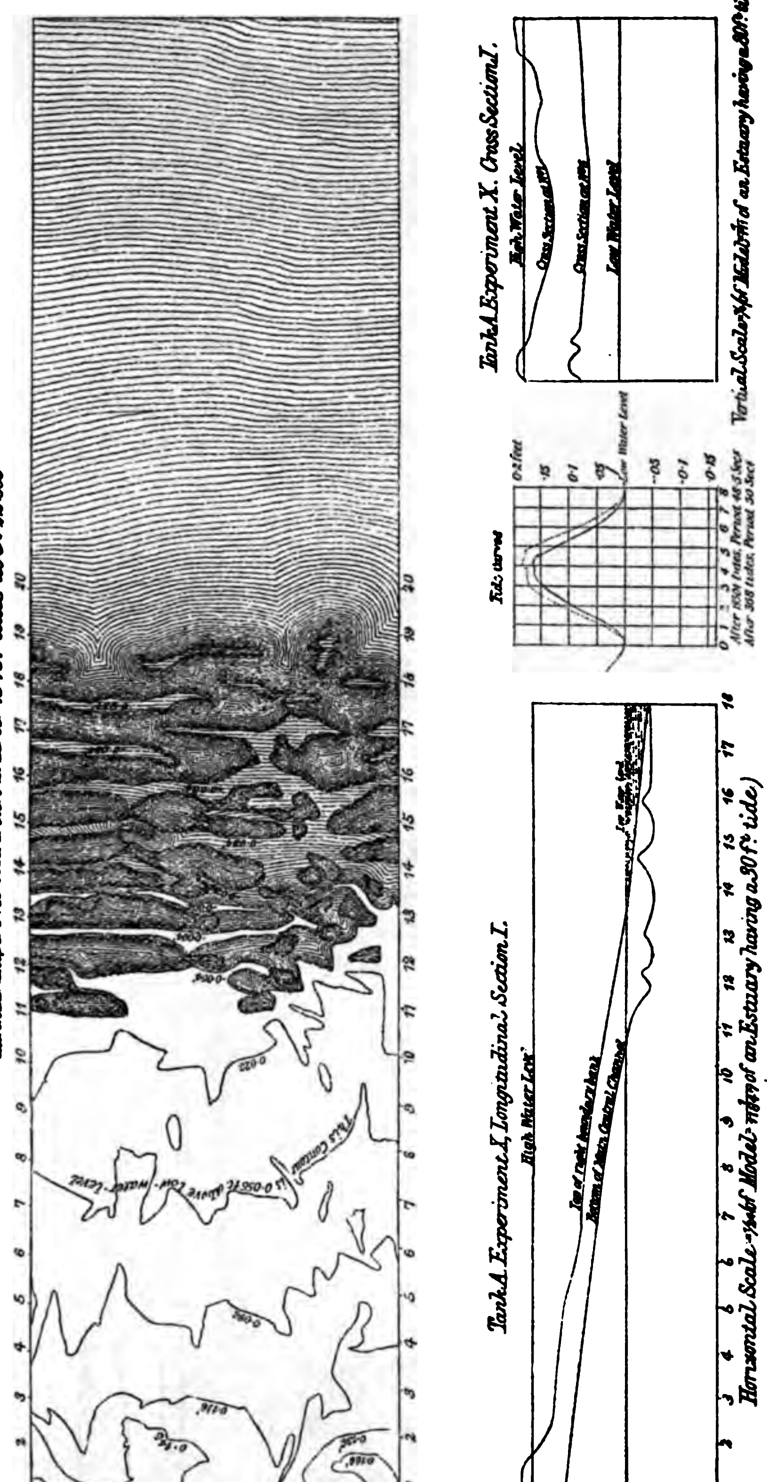

SERIES I. WITH LAND WATER
Tank A. Experiment X. Plan I. After 19437 tides at 30·22 Secs
Tank A. Experiment X. Longitudinal Section I.
High Water Level
Tank A. Experiment X. Cross Section I.
High Water Level
Low Water Level
Tide curves

Fig. 14.

On a 30 ft. tide, distance between the sections represents about 1·1 miles.

SERIES I WITH LAND WATER

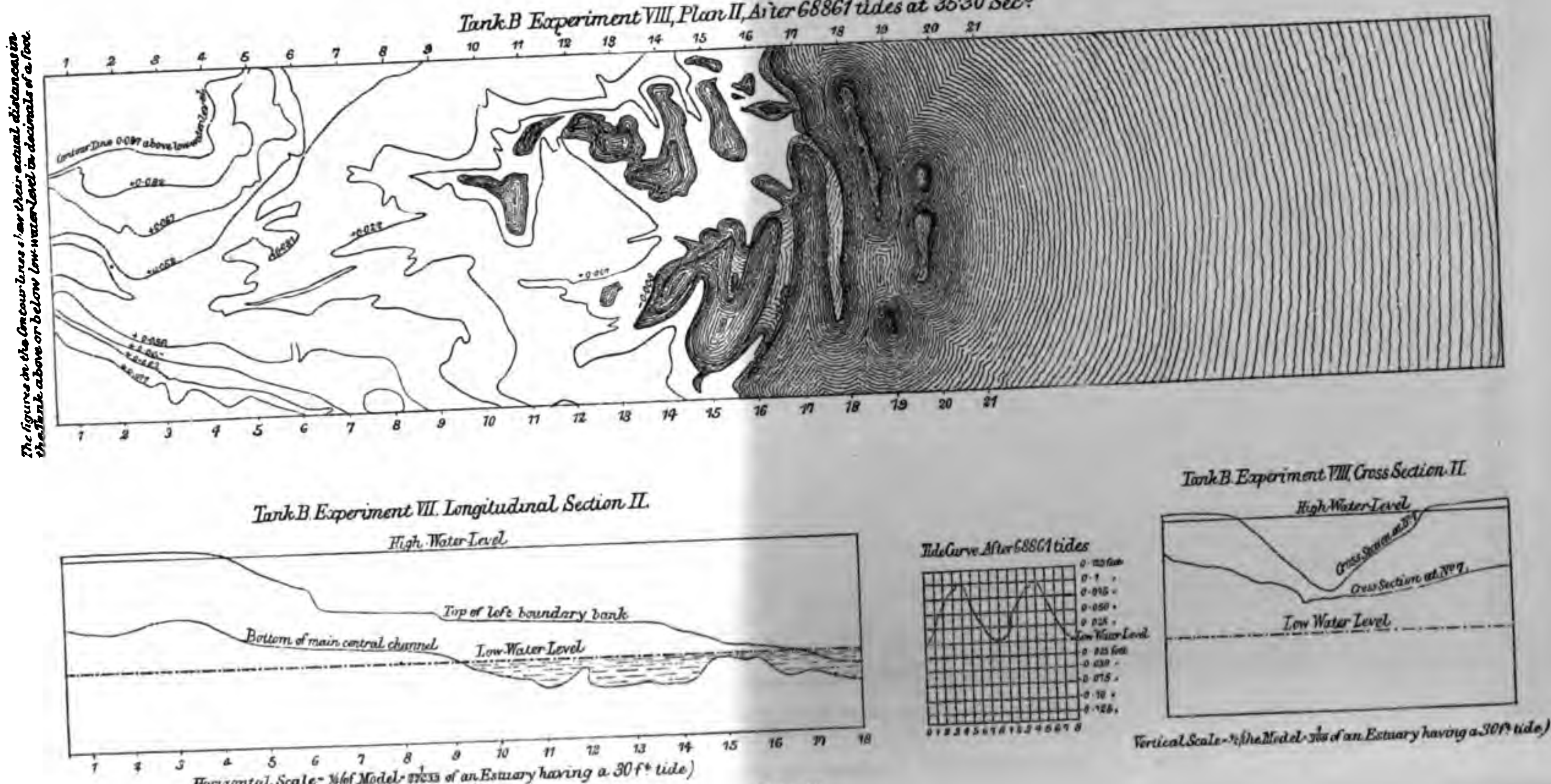


Fig. 17.

On a 30 *ft.* tide, distance between the sections represents about 1·1 miles.

SERIES I WITH LAND WATER.

Tank A. Experiment X. Plan III. After 71574 tides at 49·8 seconds.

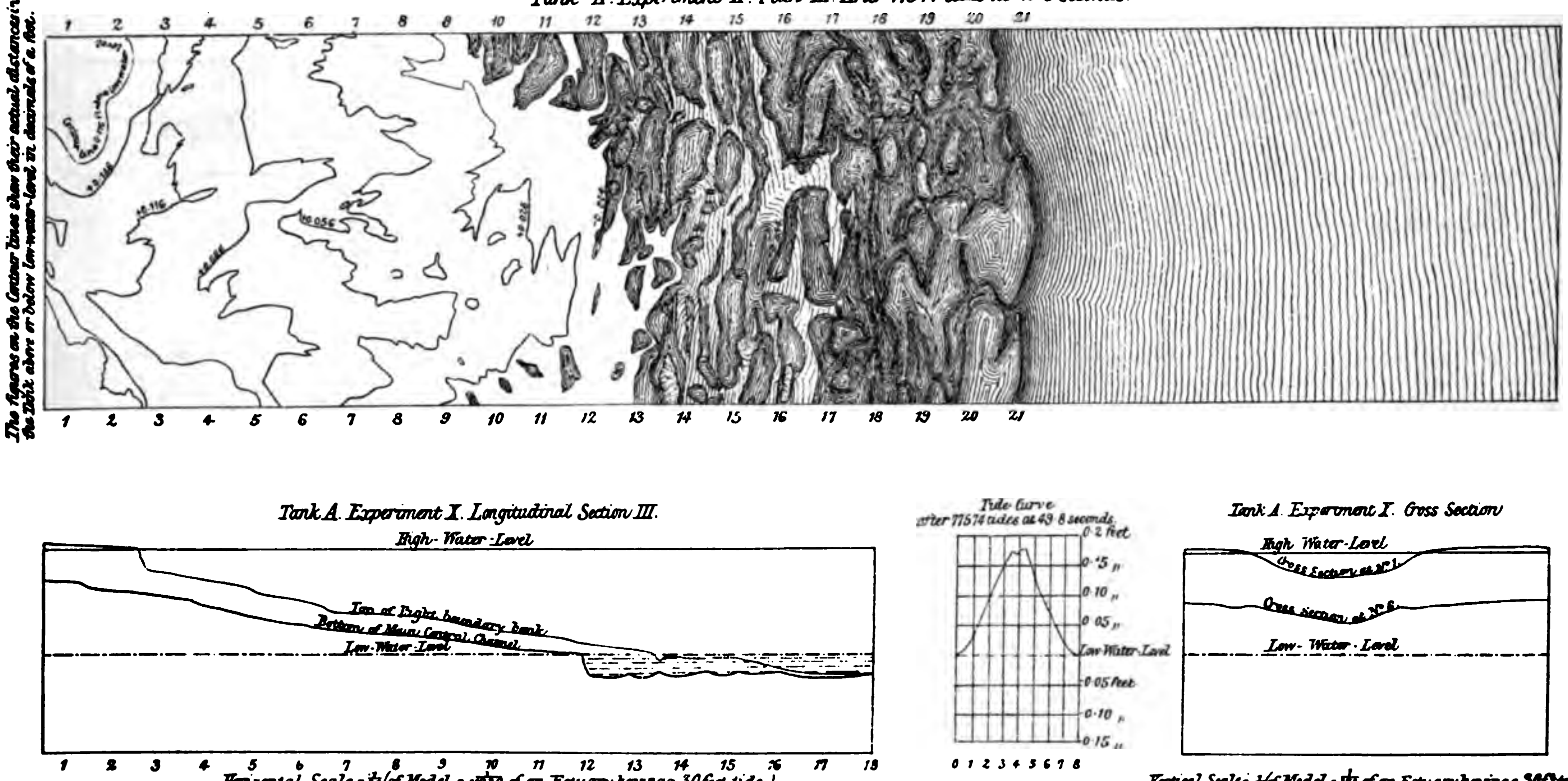


Fig. 18.

On a 30 *ft.* tide, distance between the sections represents about 1·1 miles.

SERIES I WITH LAND WATER.

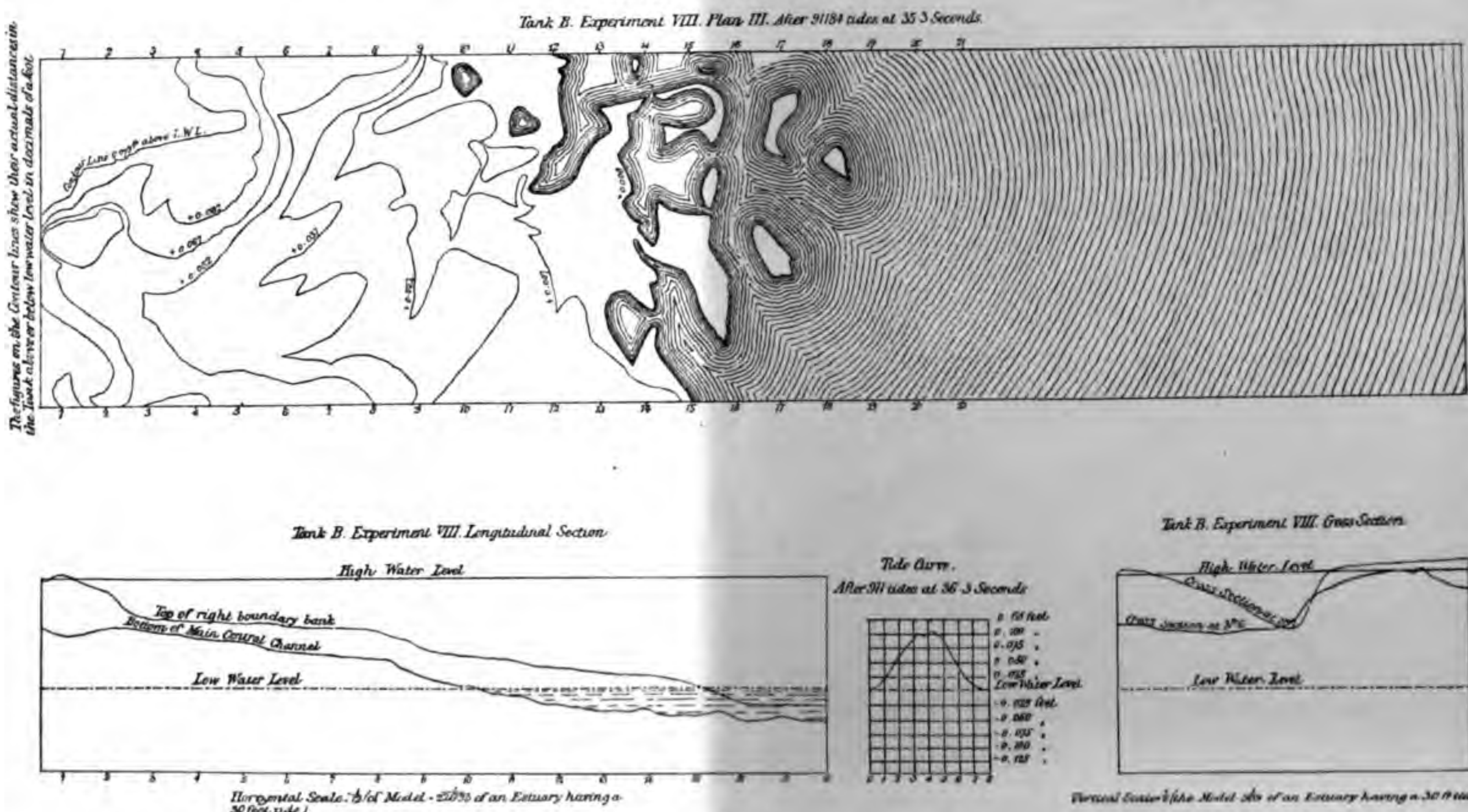


Fig. 19.

SERIES II. WITHOUT LAND WATER.

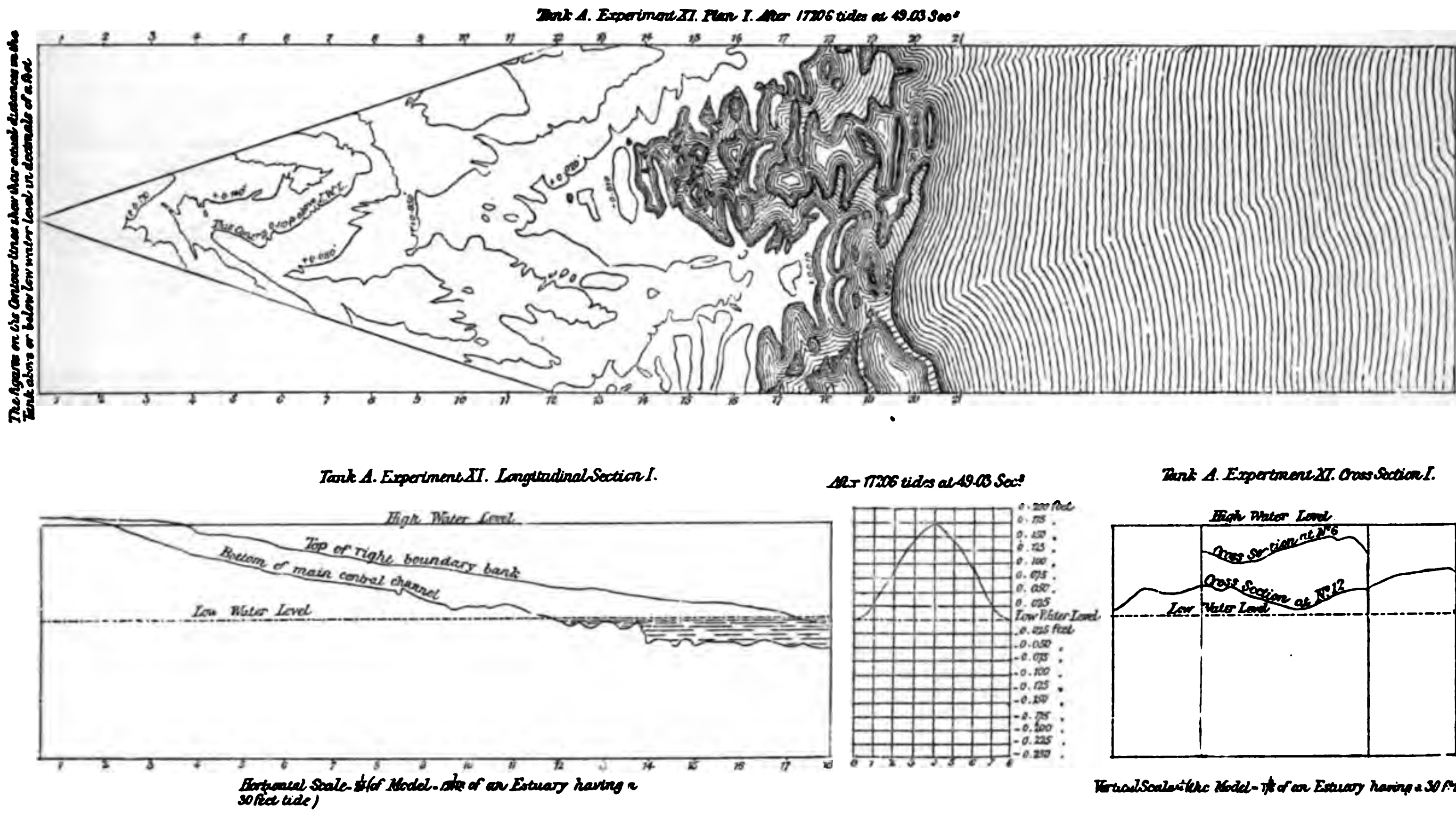


Fig. 20.

On a 30 ft. tide, distance between the sections represents about 1·16 miles.

SERIES II. WITHOUT LAND WATER.

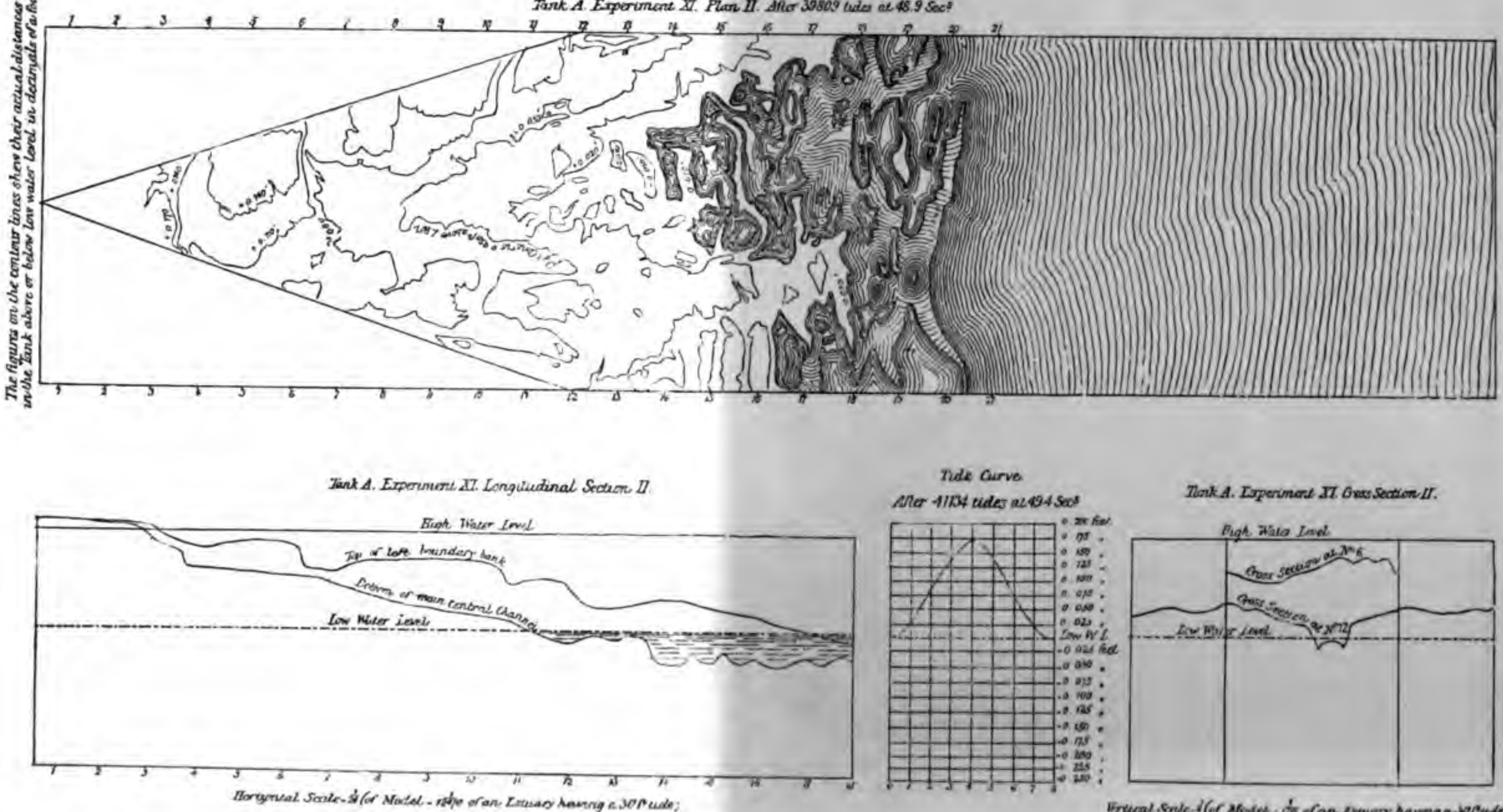


Fig. 21.

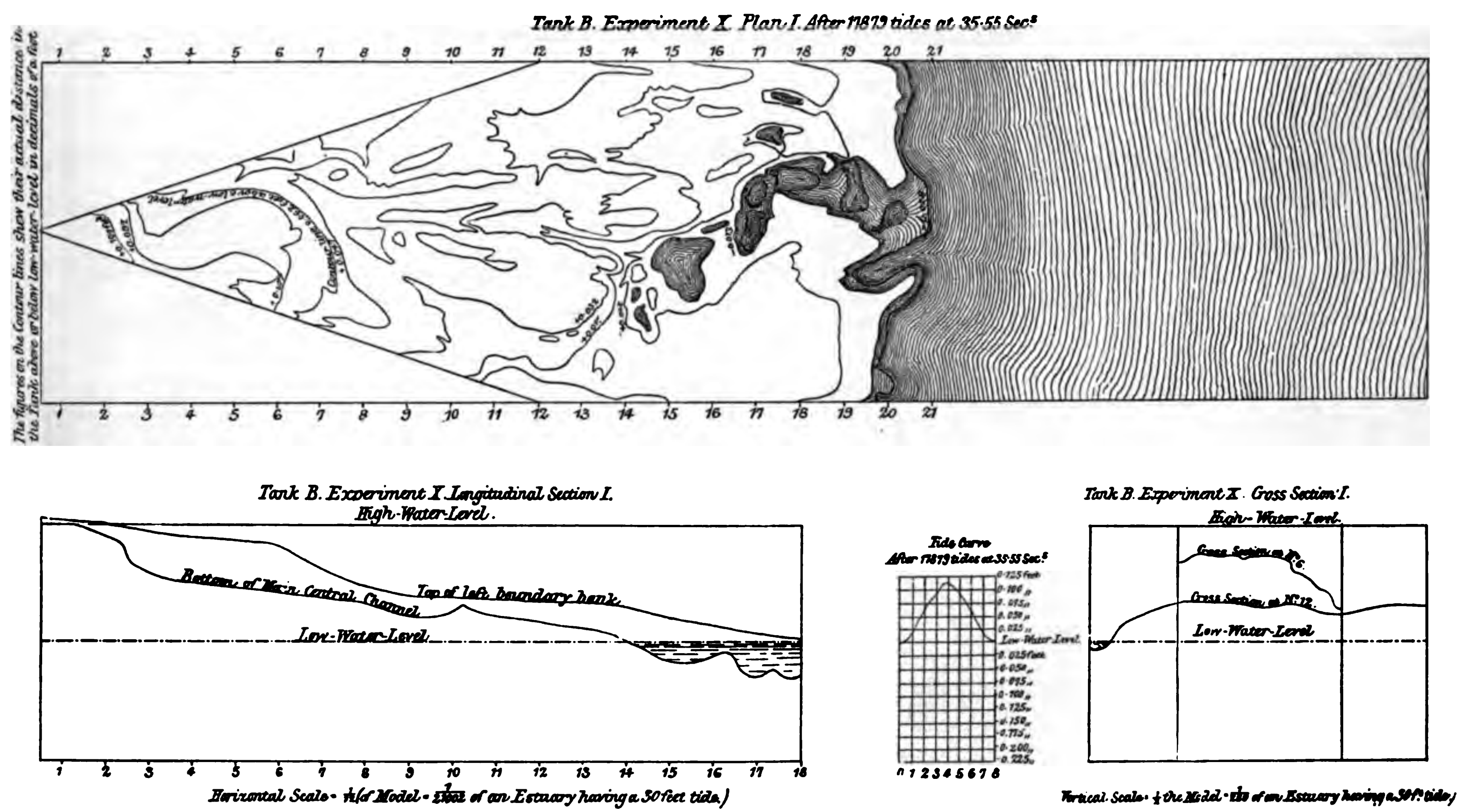

Tank B. Experiment X. Plan I. After 11879 tides at 35·55 Secs
The figures on the Contour lines shew their actual distance in the Tank above or below low-water-level in decimals of a foot.
Tank B. Experiment X. Longitudinal Section I.
High-Water-Level.
Bottom of Main Central Channel.
Top of left boundary bank.
Low-Water-Level.
Horizontal Scale
Tide Curve
After 11879 tides at 35·55 Secs
Low-Water-Level.
Tank B. Experiment X. Cross Section I.
High-Water-Level.
Cross Section on No. 6.
Cross Section on No. 12.
Low-Water-Level.
Vertical Scale

Fig. 22.

On a 30 ft. tide, distance between the sections ...

SERIES II. WITHOUT LAND WATER.

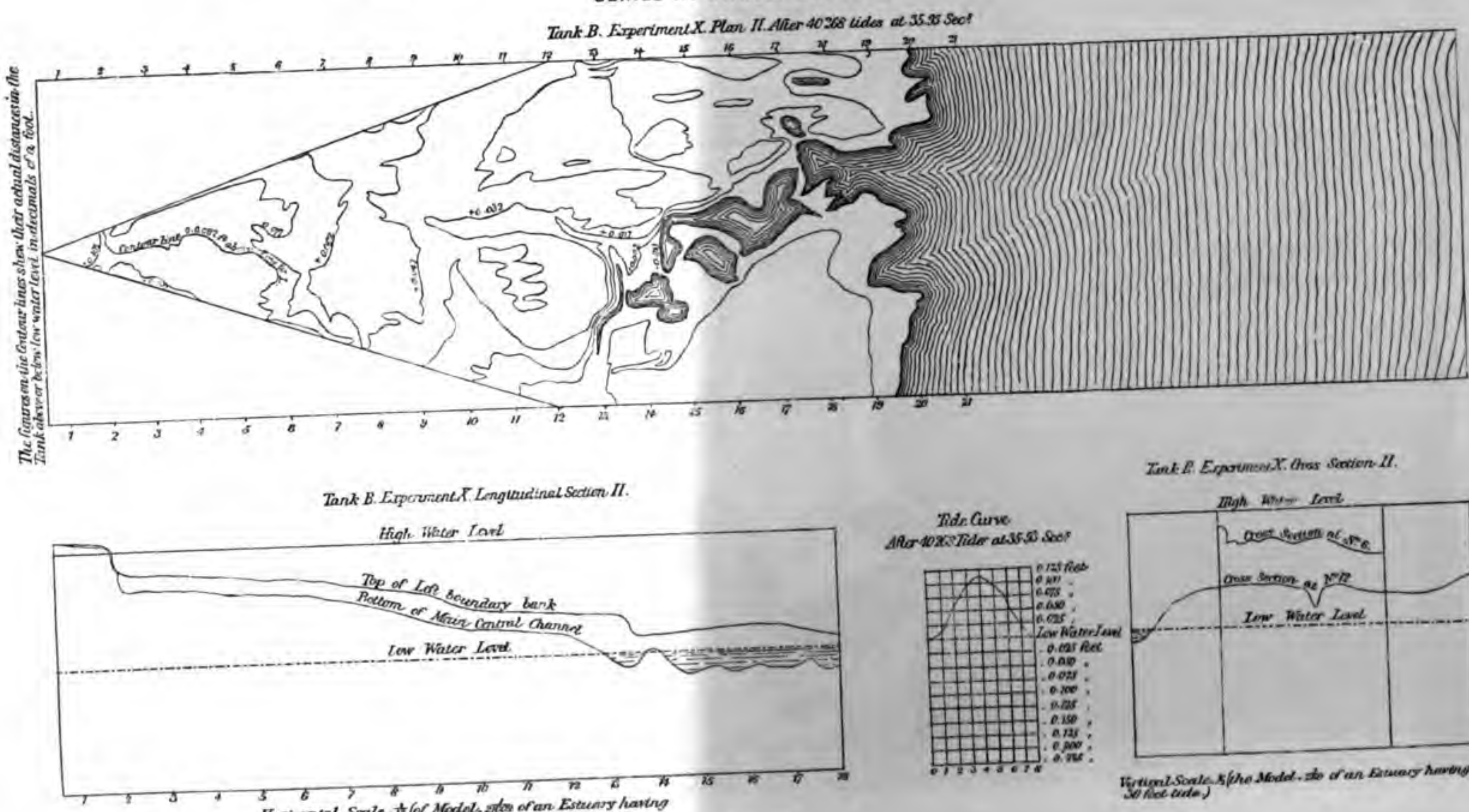


Fig. 23.

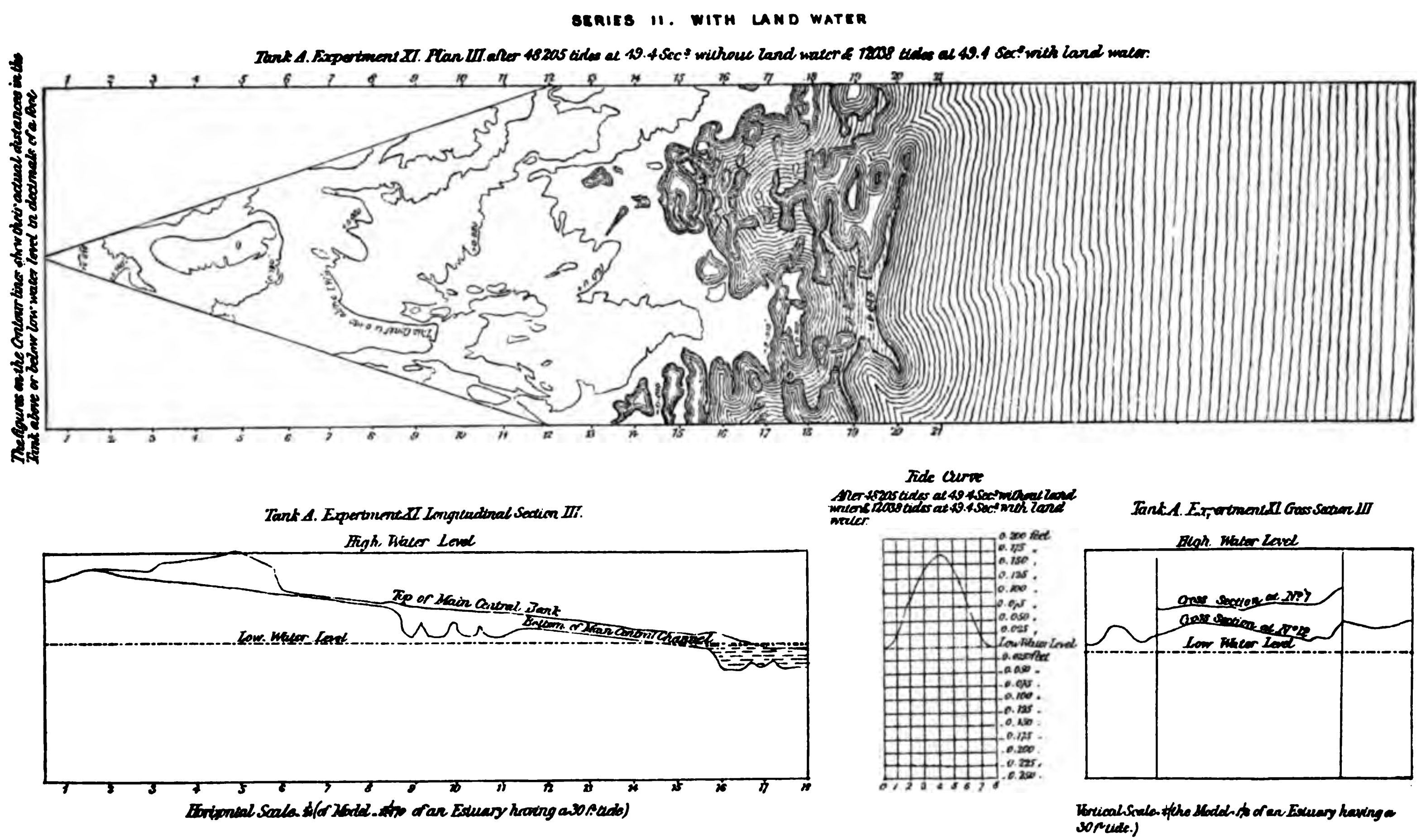

SERIES II. WITH LAND WATER
Tank A. Experiment XI. Plan III. after 48205 tides at 49·4 Sec.s without land water & 12038 tides at 49.4 Sec.s with land water.
The figures on the Contour lines show their actual distances in the Tank above or below low water level in decimals of a foot.
Tank A. Experiment XI Longitudinal Section III.
High. Water Level
Top of Main Central Tank
Bottom of Main Central Channel
Low. Water Level
Horizontal Scale. 1/6 (of Model. 1/4770 of an Estuary having a 30 ft tide)
Tide Curve
After 48205 tides at 49·4 Sec.s without land water & 12038 tides at 49·4 Sec.s with land water.
0.200 feet
Low Water Level
Tank A. Experiment XI Cross Section III
High. Water Level
Cross Section at No 7
Cross Section at No 12
Low Water Level
Vertical Scale. 1/4 (the Model 1/60 of an Estuary having a 30 ft tide.)

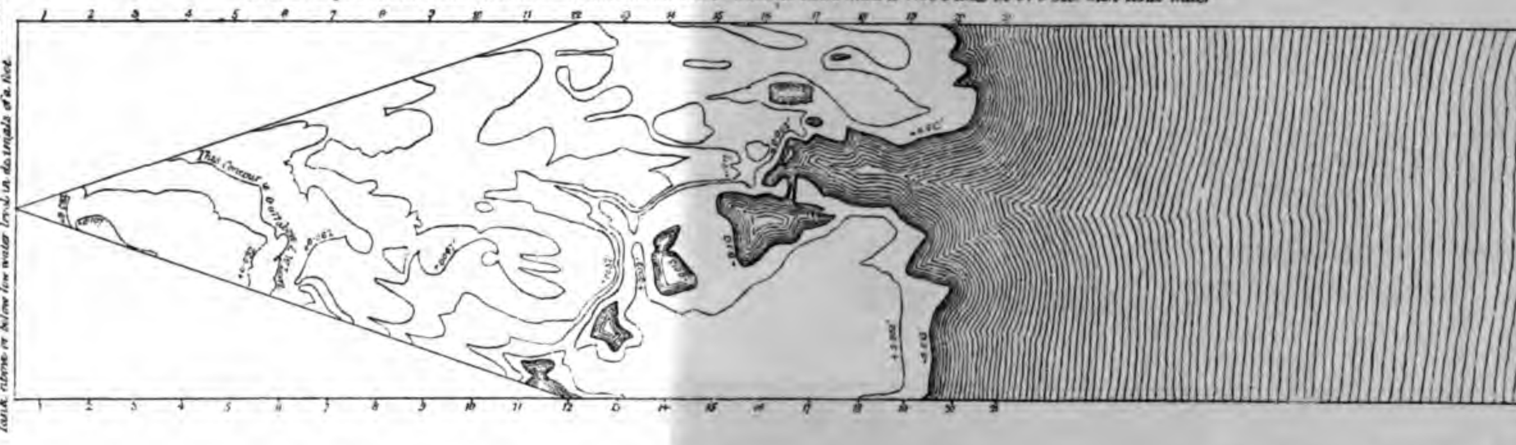
Fig. 24.

On a 30 *ft.* tide, distance between the contours represents about 1·1 miles.

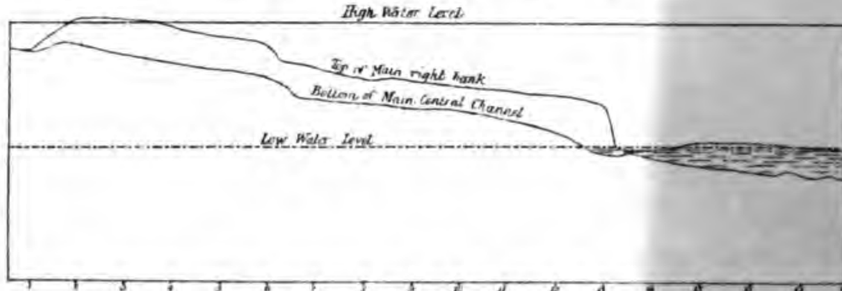
SERIES II. WITH LAND WATER.

Tank B. Experiment X. Plan III. After 45332 tides at 35·9 Sec.s without land water & 11692 tides at 34·8 Sec.s with land water

The figures on the Contour Lines show their actual distance in the Tank above or below low water levels in decimals of a foot.



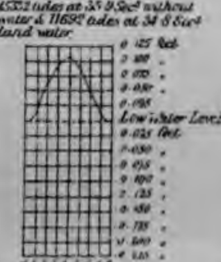
Tank B. Experiment X. Longitudinal Section III



Horizontal Scale ... of Model ... of an Estuary having a 30 ft tide)

Tide Curve

After 45332 tides at 35·9 Sec.s without land water & 11692 tides at 34·8 Sec.s with land water



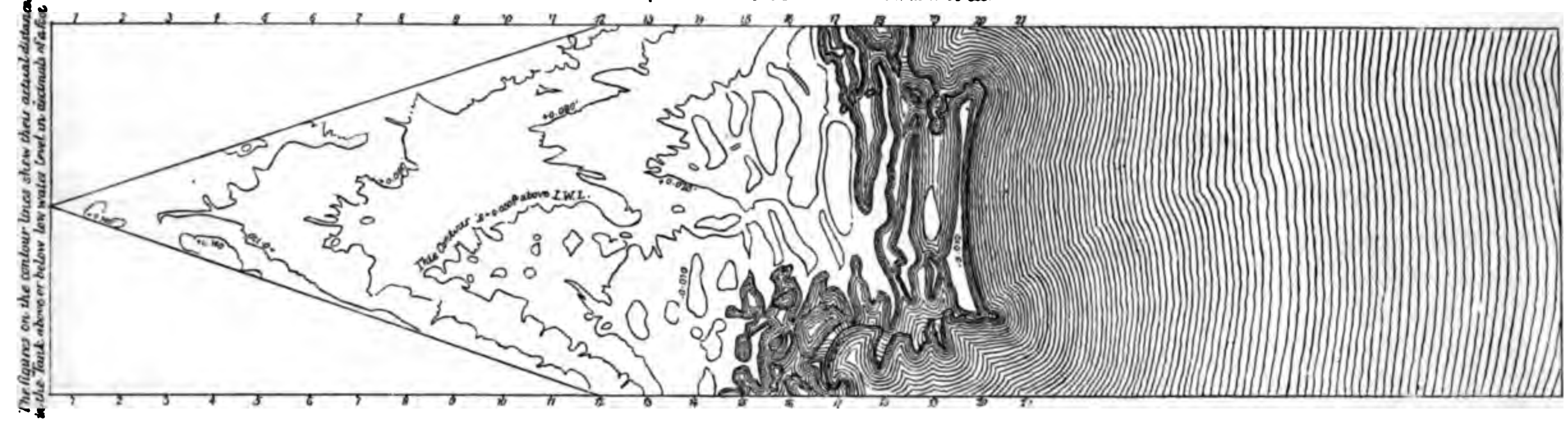
Tank B. Experiment X. Cross Section III

Vertical Scale ... (the Model ... of an Estuary having a 30 ft tide)

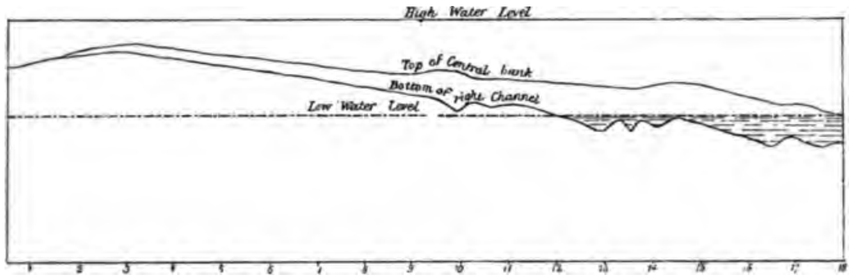
Fig. 25.

SERIES II. WITH LAND WATER.

Tank A. Experiment XII. Plan I. After 16538 tides at 49·08 Sec.



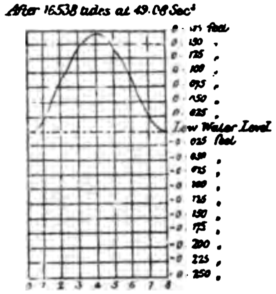
Tank A. Experiment XII. Longitudinal Section I.



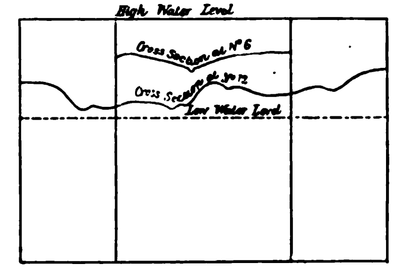
Horizontal Scale · (of Model · of an Estuary having a 30 feet tide)

Tide Curve.

After 16538 tides at 49·08 Sec.



Tank A. Experiment XII. Cross Section.



Vertical Scale · (of Model · of an Estuary having a 30 ft tide)

Fig. 26.

On a 30 ft. tide, distance between the sections represents about 1·2 miles.

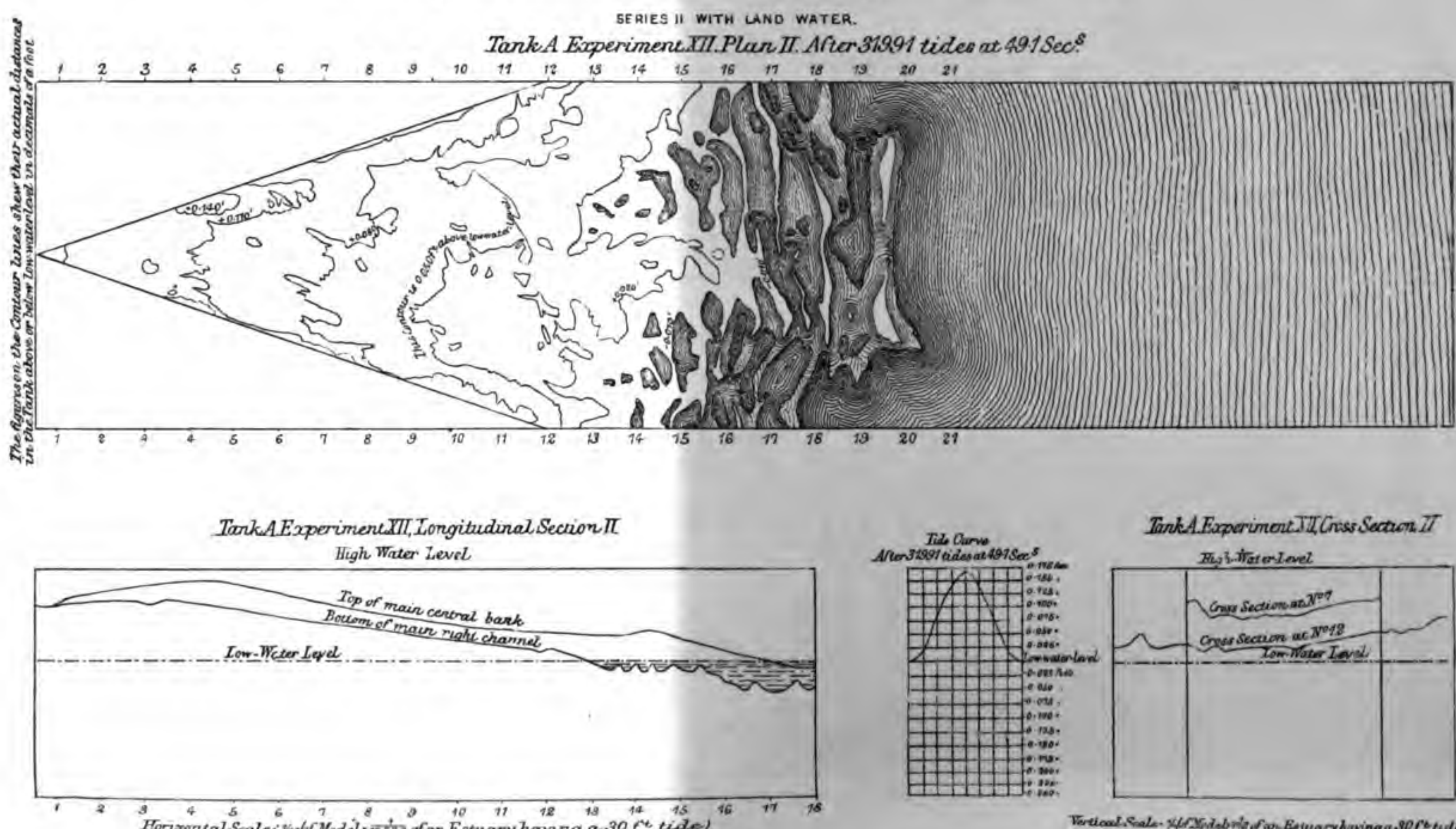


Fig. 37.

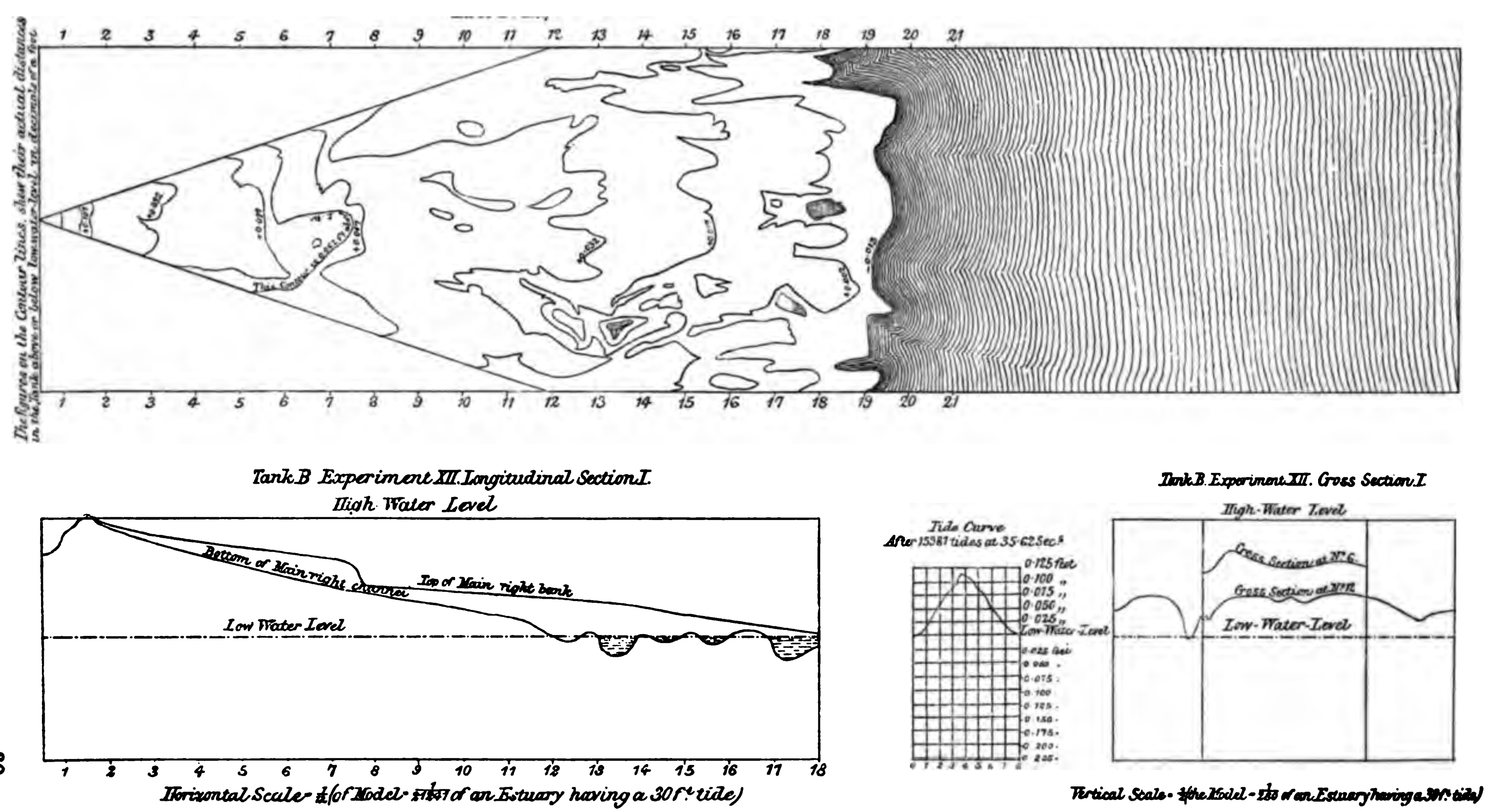
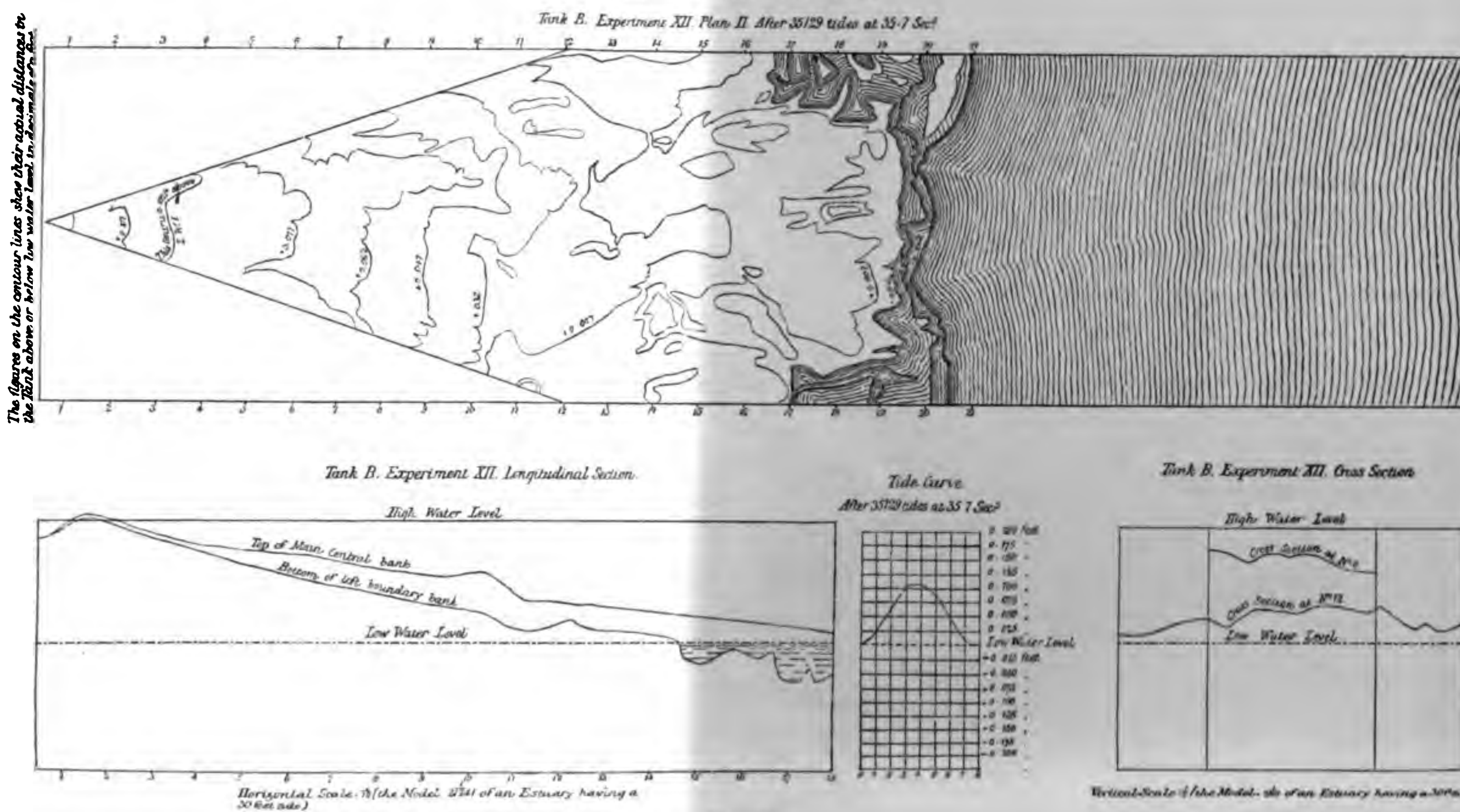

The figures on the Contour lines, show their actual distances in the Tank above or below low-water-level, in decimals of a foot.
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21
Tank B Experiment XII. Longitudinal Section I.
High Water Level
Bottom of Main right channel
Top of Main right bank
Low Water Level
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18
Tide Curve
After 15361 tides at 35·62 Sec.s
0·125 feet
0·100 "
0·075 "
0·050 "
0·025 "
Low-Water-Level
Tank B. Experiment XII. Cross Section I.
High-Water Level
Cross Section at No. 6.
Cross Section at No. 12.
Low-Water-Level

Fig. 28.

On a 30 ft. tide, distance between the contours represents about 1·04 miles.

SERIES II. WITH LAND WATER



On a 30 ft. tide, distance between the sections represents about 1·2 miles.

WITHOUT LAND WATER

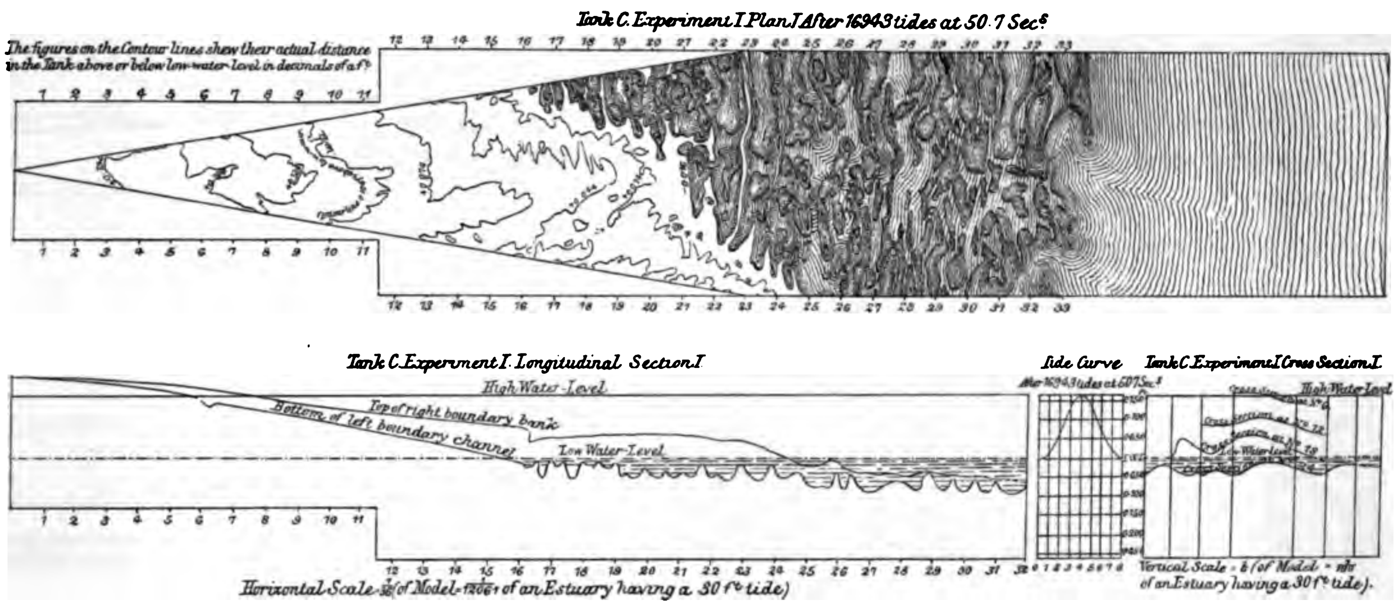


Fig. 30.

On a 30 ft. tide, distance between the sections represents about 1·2 miles.

WITHOUT LAND WATER

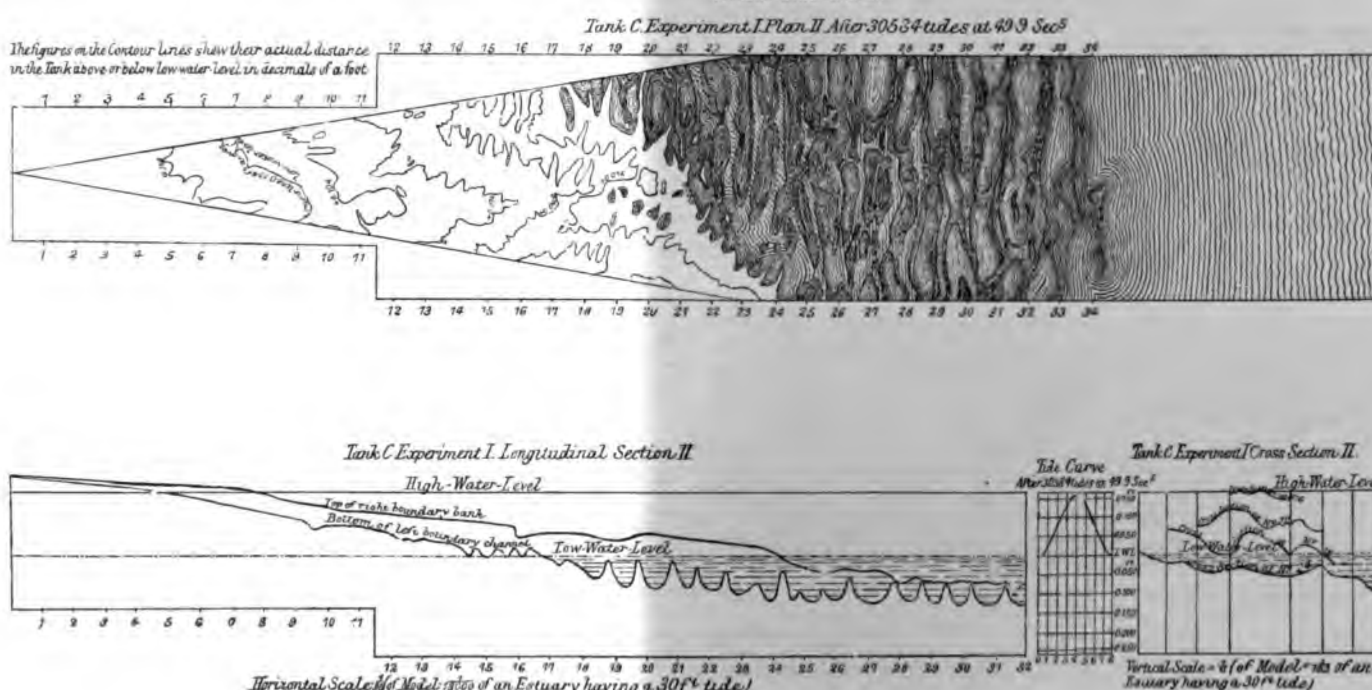


Fig. 31.

On a 30 ft. tide, distance between the sections represents about 1 mile.

WITHOUT LAND WATER

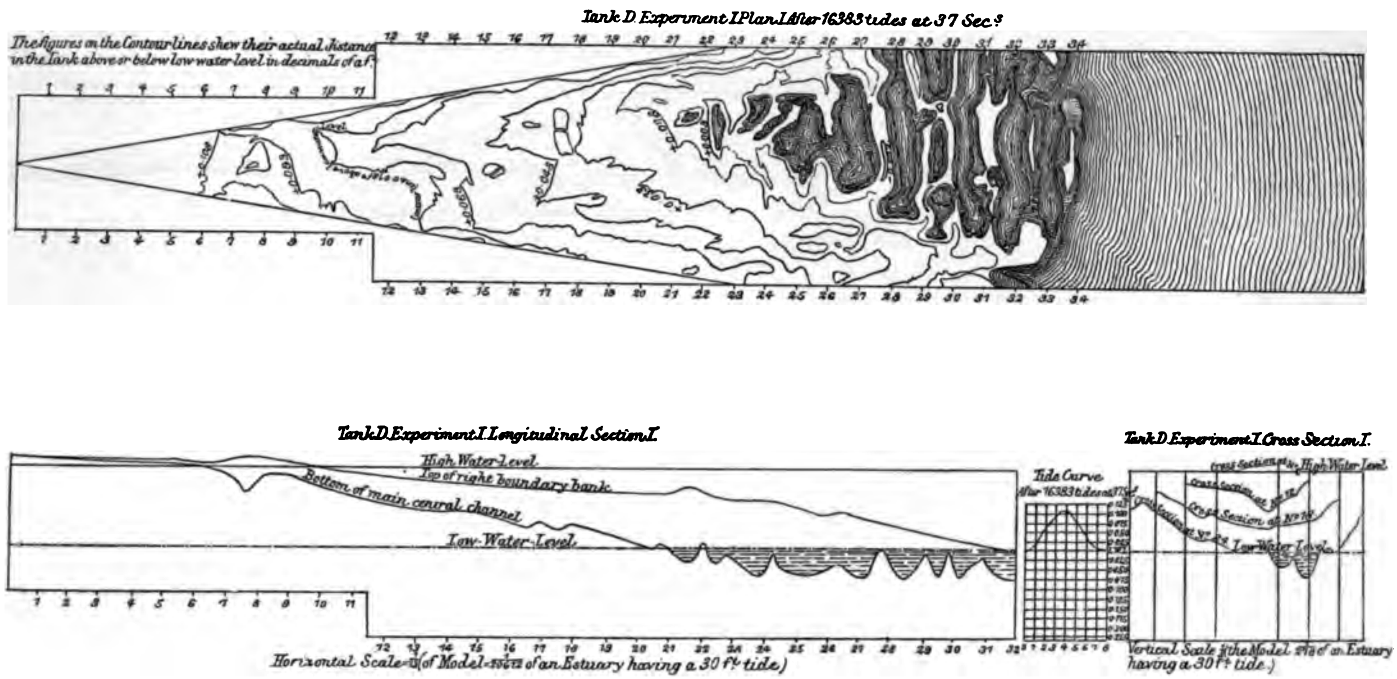


Fig. 32.

On a 30 ft. tide, distance between the sections represents about 1 mile.

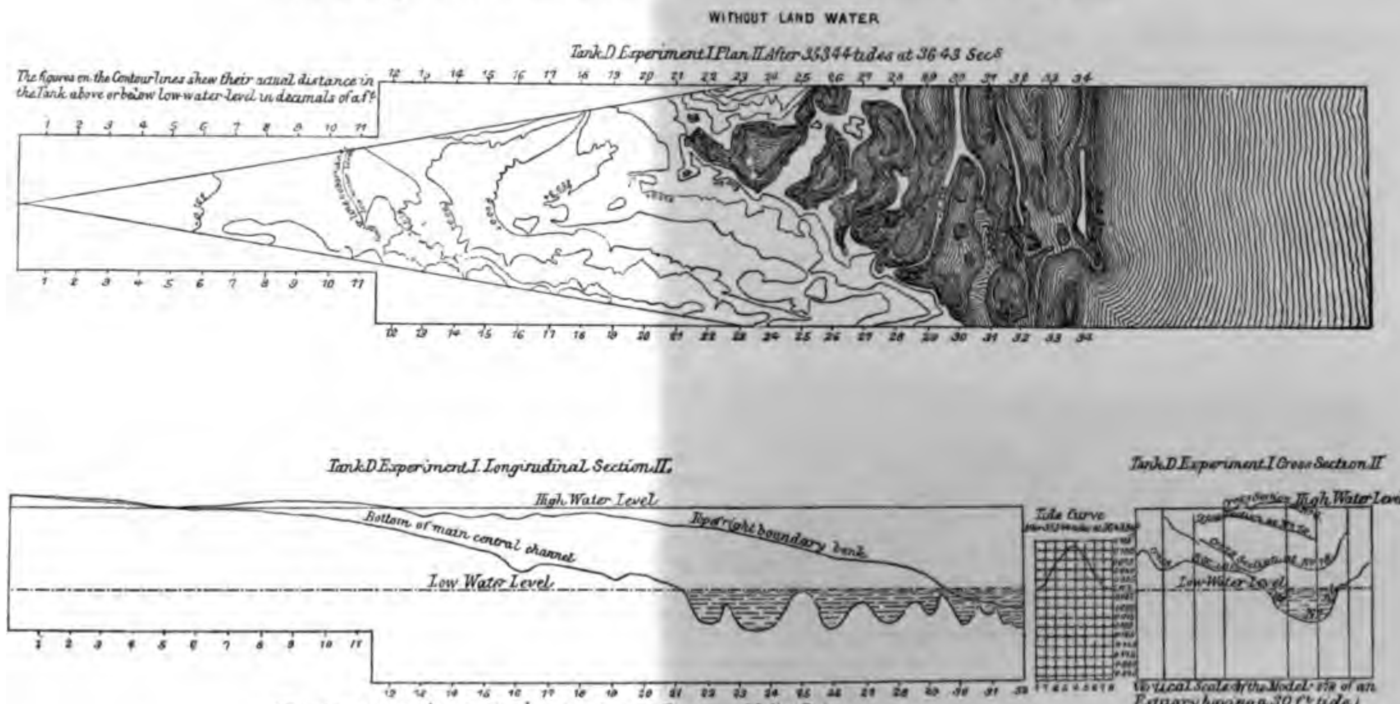


Fig. 33.

On a 30 ft. tide, distance between the sections represents 1·19 miles.

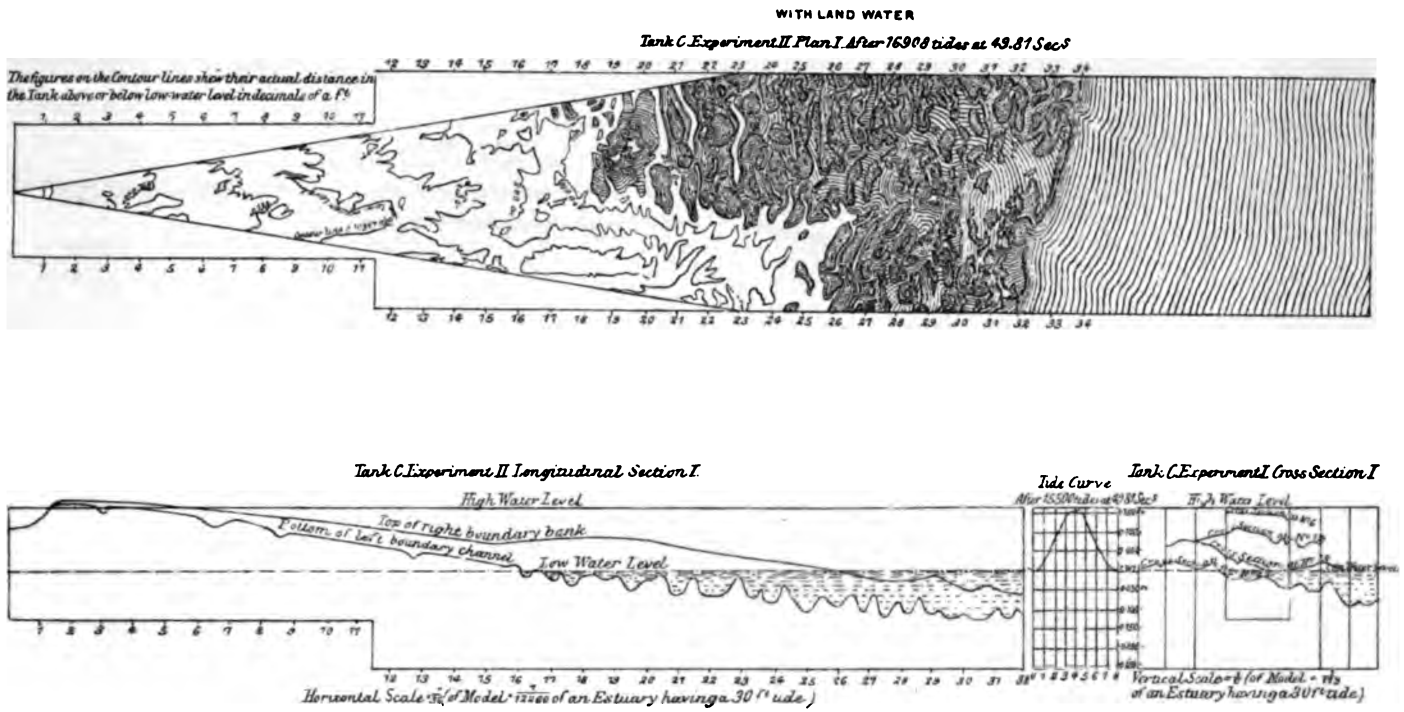


Fig. 34.

On a 30 ft. tide, distance between the sections represents 1·19 miles.

WITH LAND WATER

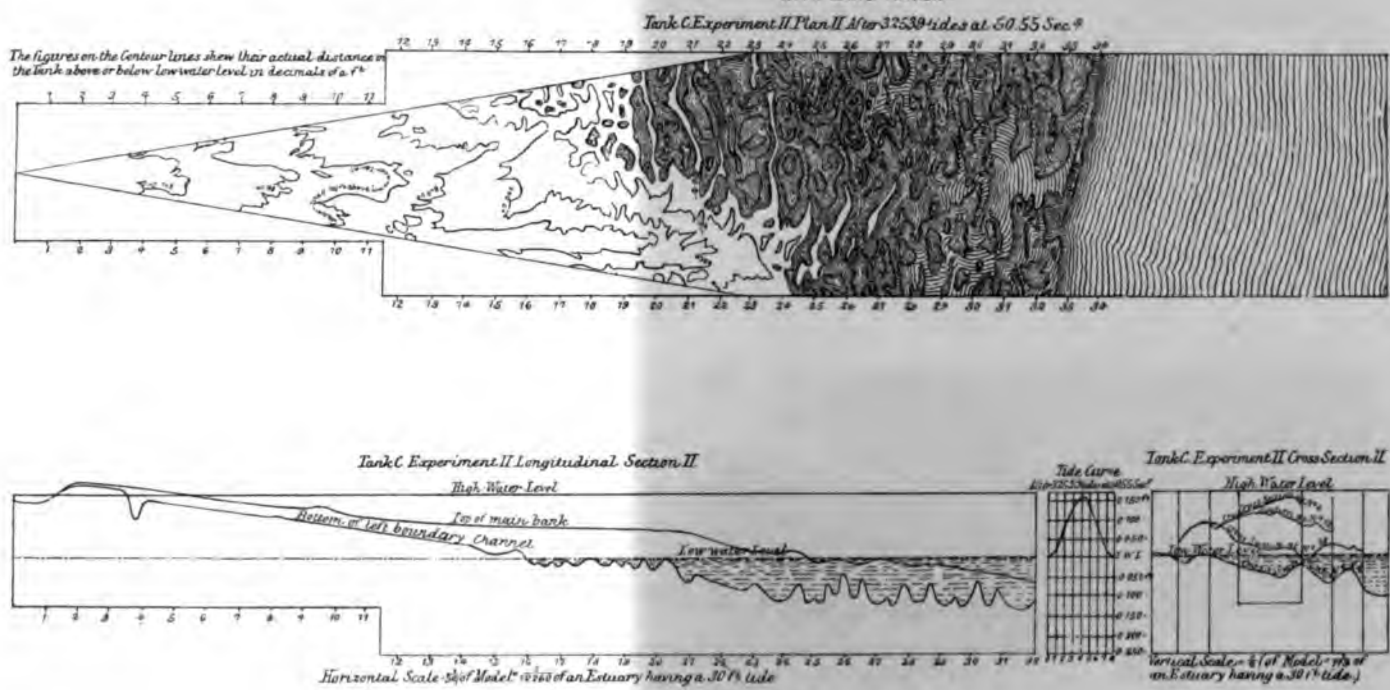
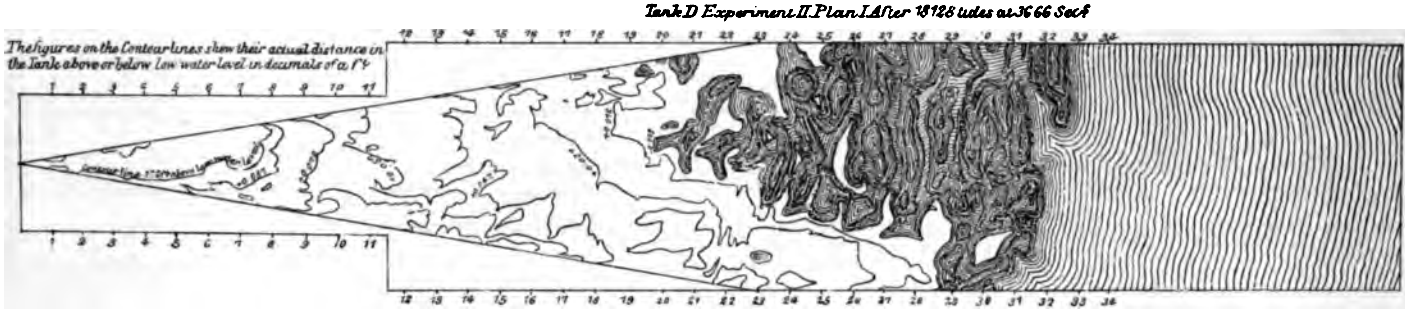


Fig. 35.

On a 30 ft. tide, distance between the sections represents 1·05 miles.

WITH LAND WATER



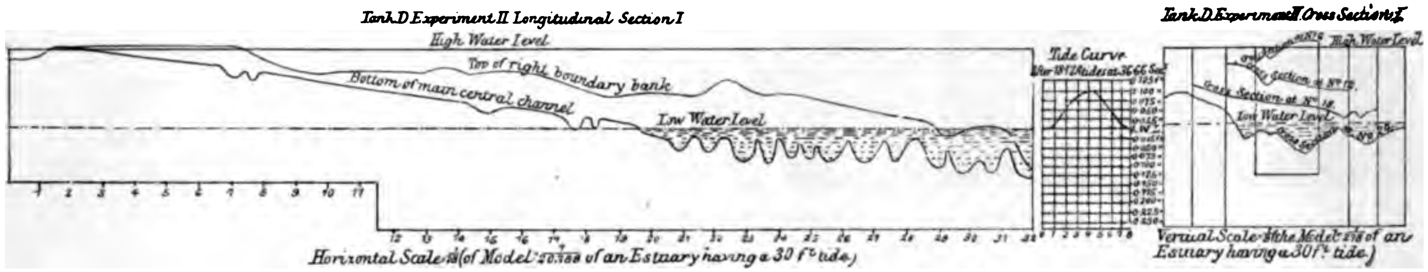


Fig. 36.

On a 30 ft. tide, distance between the sections represents 1·05 miles.

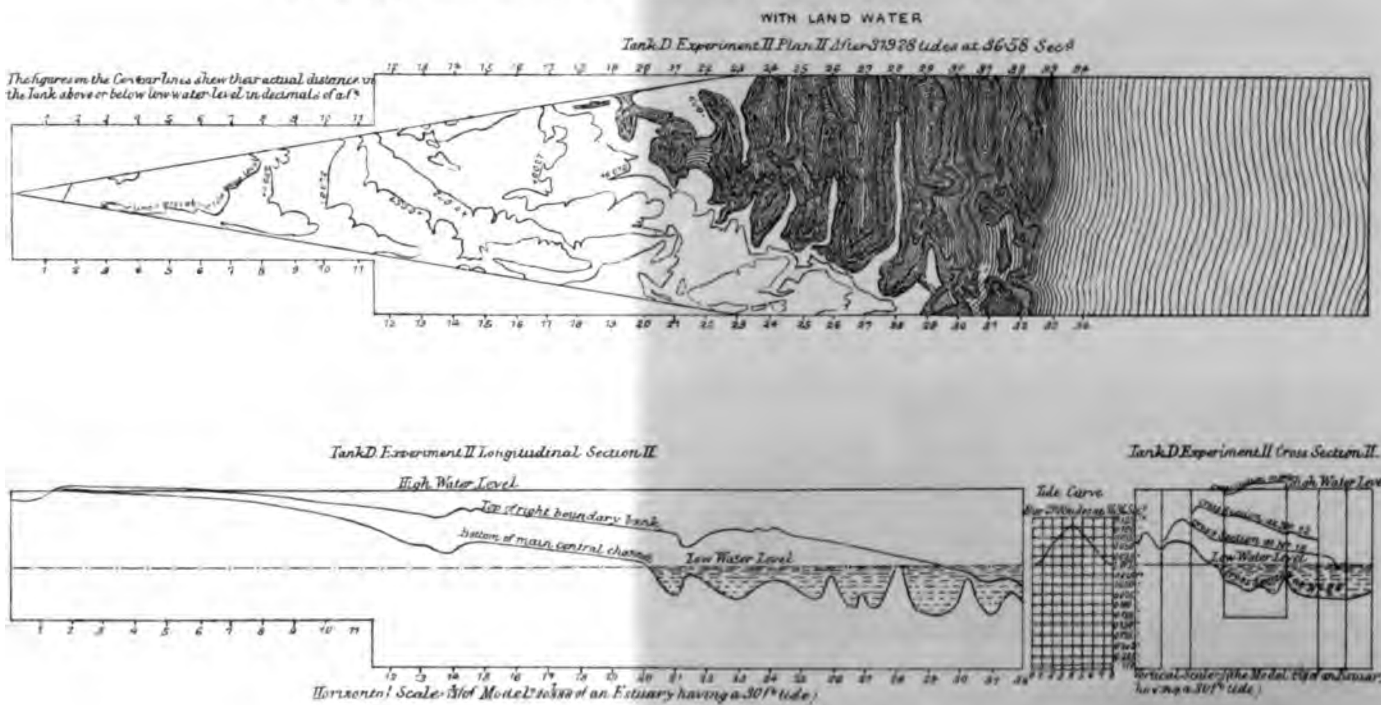


Fig. 37.

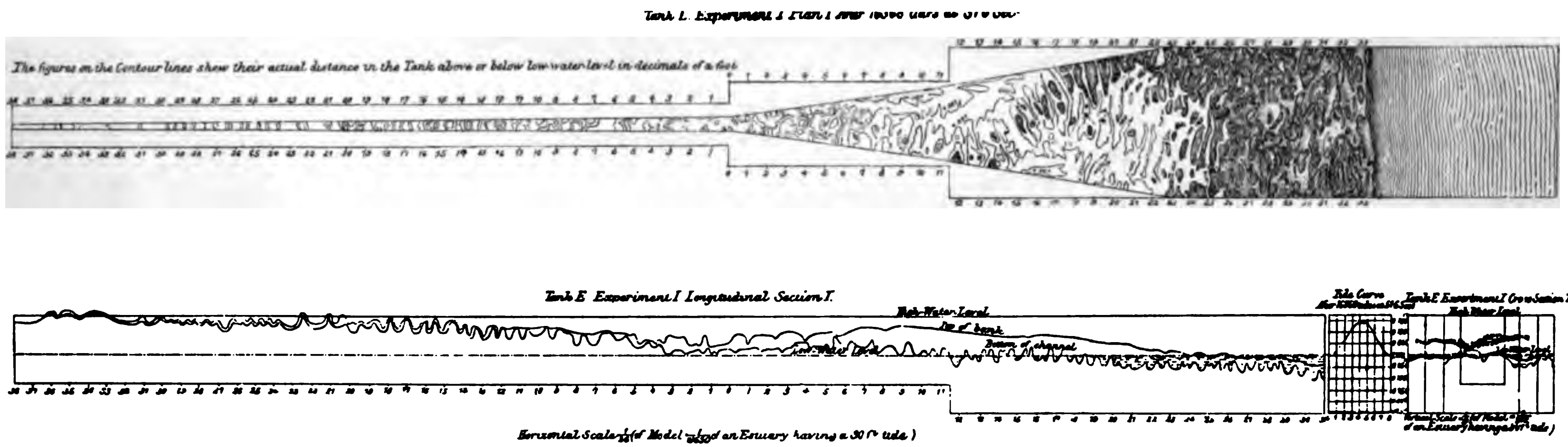

The figures on the Contour lines show their actual distance in the Tank above or below low-water-level in decimals of a foot.
Tank E. Experiment I. Longitudinal Section I.
High-Water Level
Top of bank
Bottom of channel
Low-Water Level
Tide Curve
Tank E. Experiment I. Cross Section I
High-Water Level
Horizontal Scale

Fig. 38.

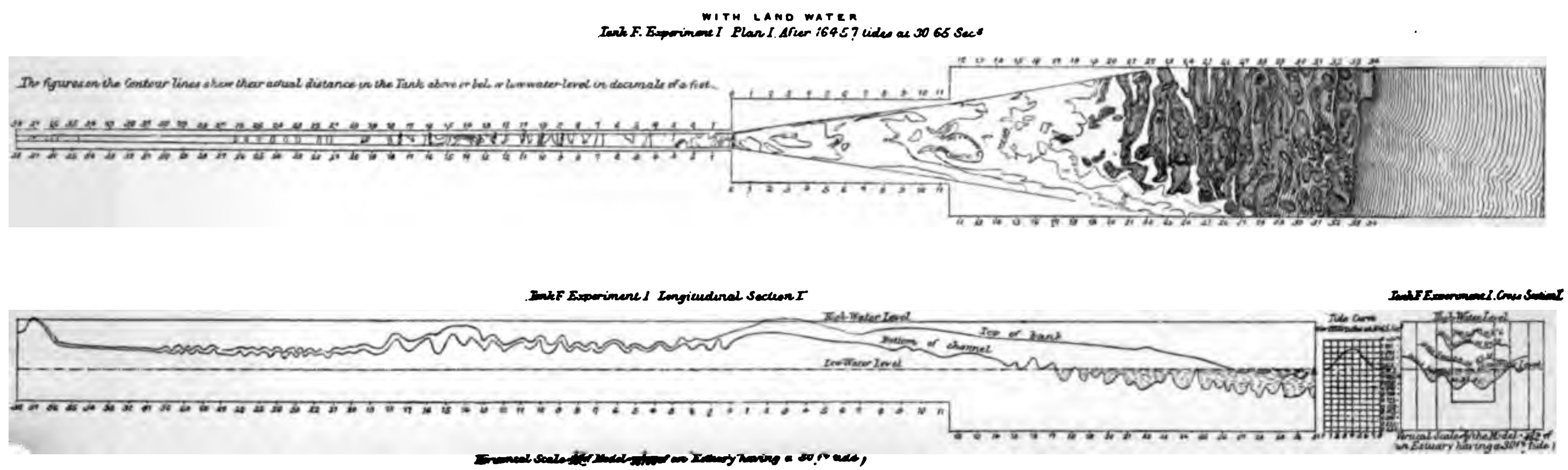

WITH LAND WATER
Tank F. Experiment I. Plan I. After 16457 tides at 30 65 Secs
The figures on the Contour lines show their actual distance in the Tank above or below low-water-level in decimals of a foot.
Tank F. Experiment I. Longitudinal Section I.
High-Water Level
Top of bank
Bottom of channel
Low-Water Level
Tide Curve
Tank F. Experiment I. Cross Section I.
High-Water Level

Fig. 39.

On a 30 ft. tide, distance between the sections represents about 1·2 miles.

WITH LAND WATER

Tank E. Experiment 1. Plan II. After 32658 tides at 50·7 Sec.

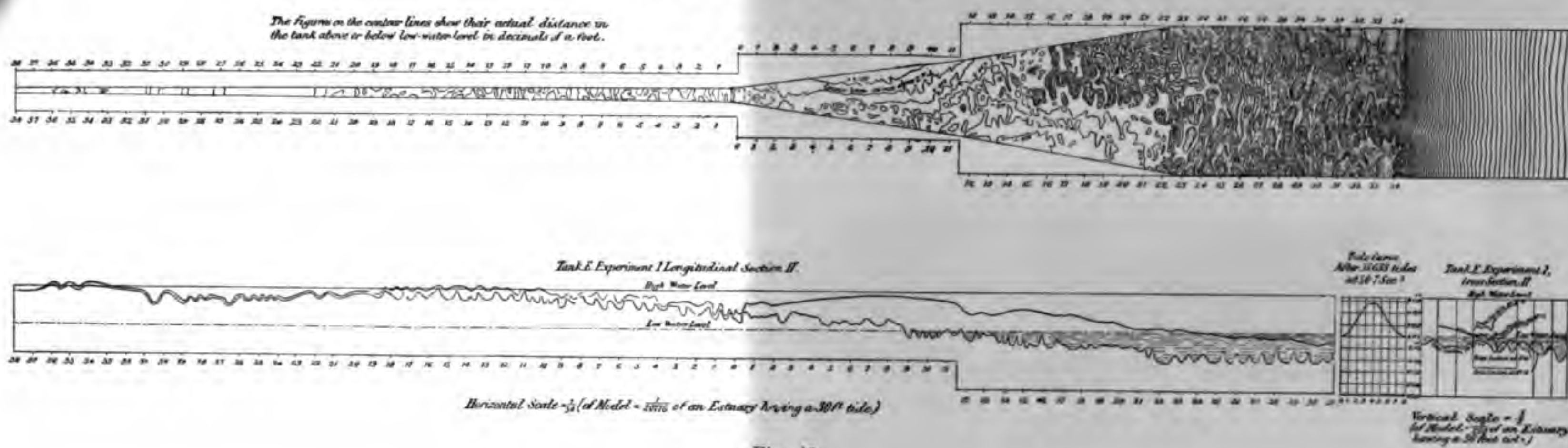
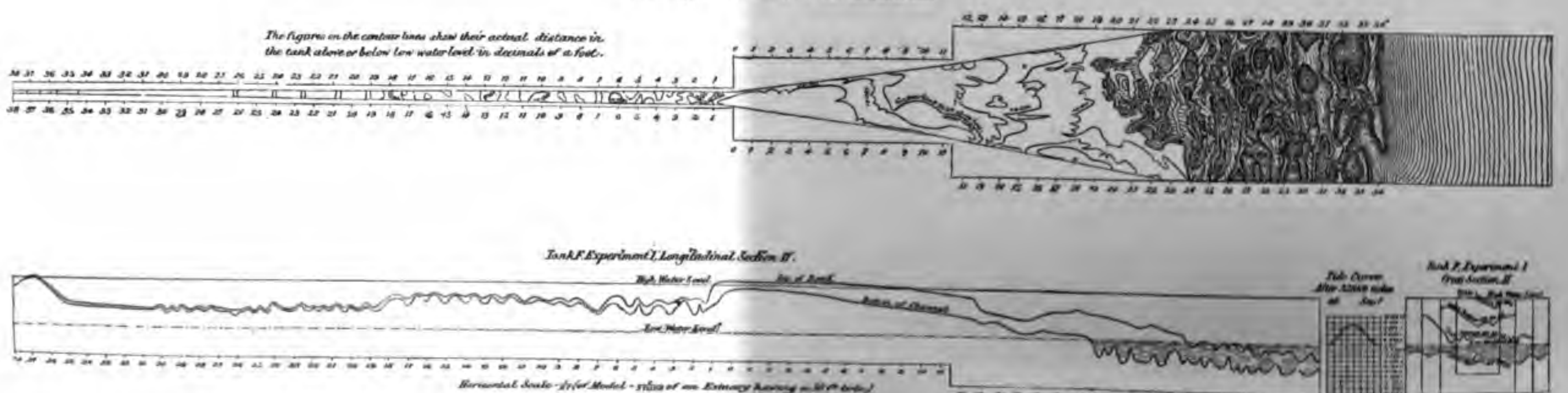


Fig. 40.

WITH LAND WATER

Tank F. Experiment 1. Plan II. After 32880 tides at 30 Sec.



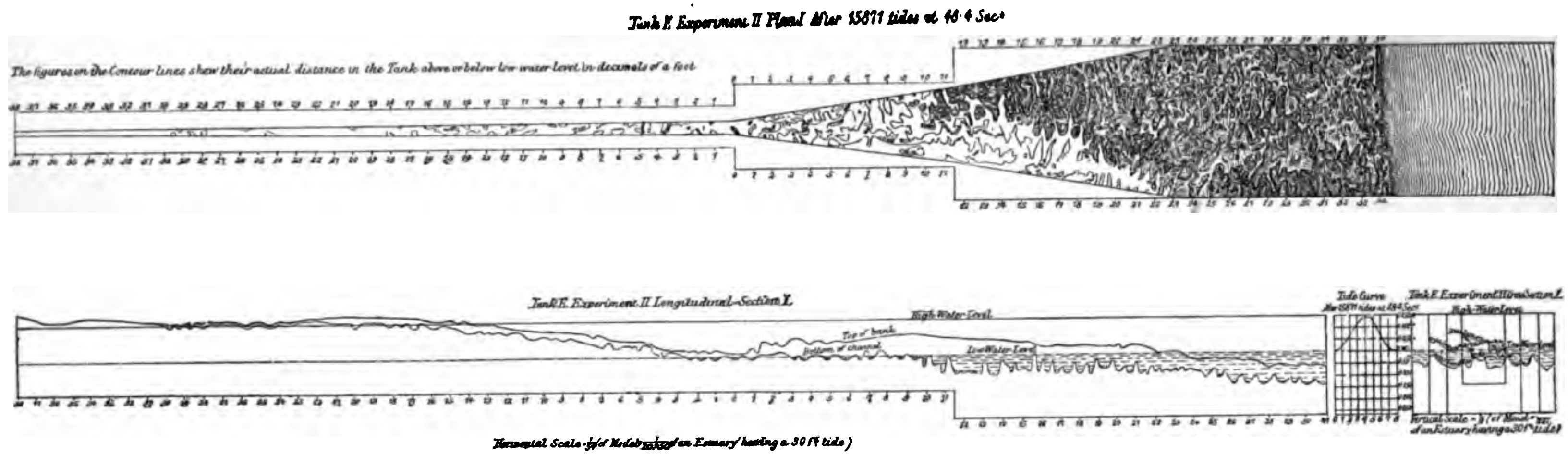

Tank F. Experiment II Plan I After 15871 tides at 48·4 Secs
The figures on the Contour lines shew their actual distance in the Tank above or below low water level in decimals of a foot
Tank F. Experiment II Longitudinal Section I
High Water Level
Top of bank
Bottom of channel
Low Water Level
Tide Curve
High Water Level

Fig. 42.

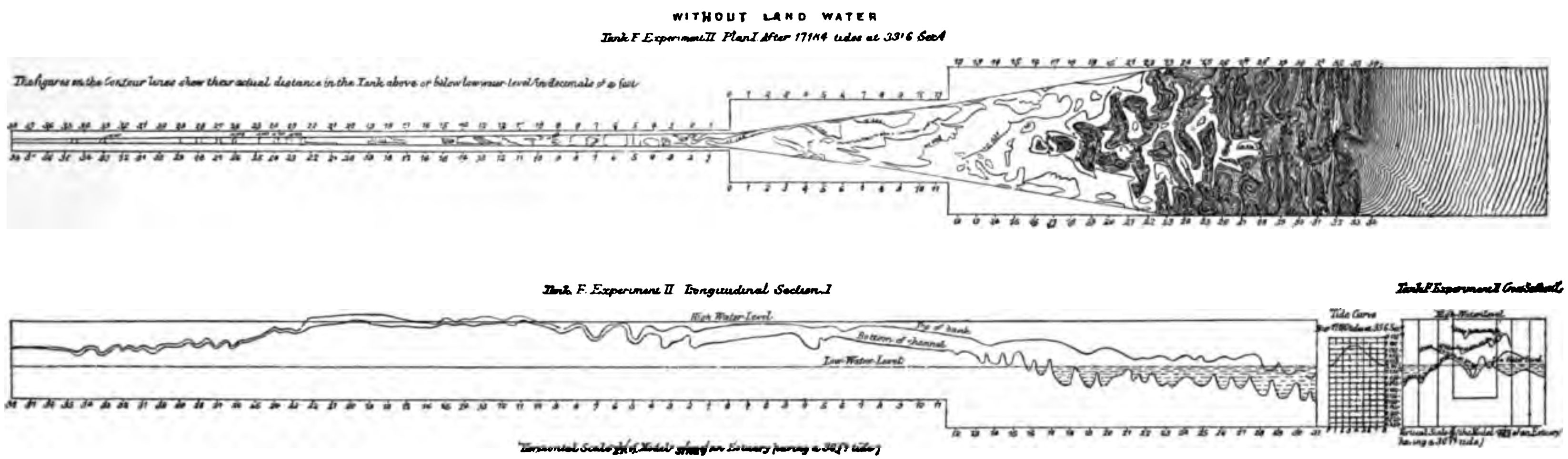

WITHOUT LAND WATER
Tank F Experiment II Plan I After 17114 tides at 33·6 Secs
The figures on the Contour lines shew their actual distance in the Tank above or below low water level in decimals of a foot
Tank F. Experiment II Longitudinal Section I
High Water Level
Top of bank
Bottom of channel
Low Water Level
Tide Curve
High Water Level

Fig. 43.

On a 30 ft. tide, distance between the sections represents about 1·25 miles.

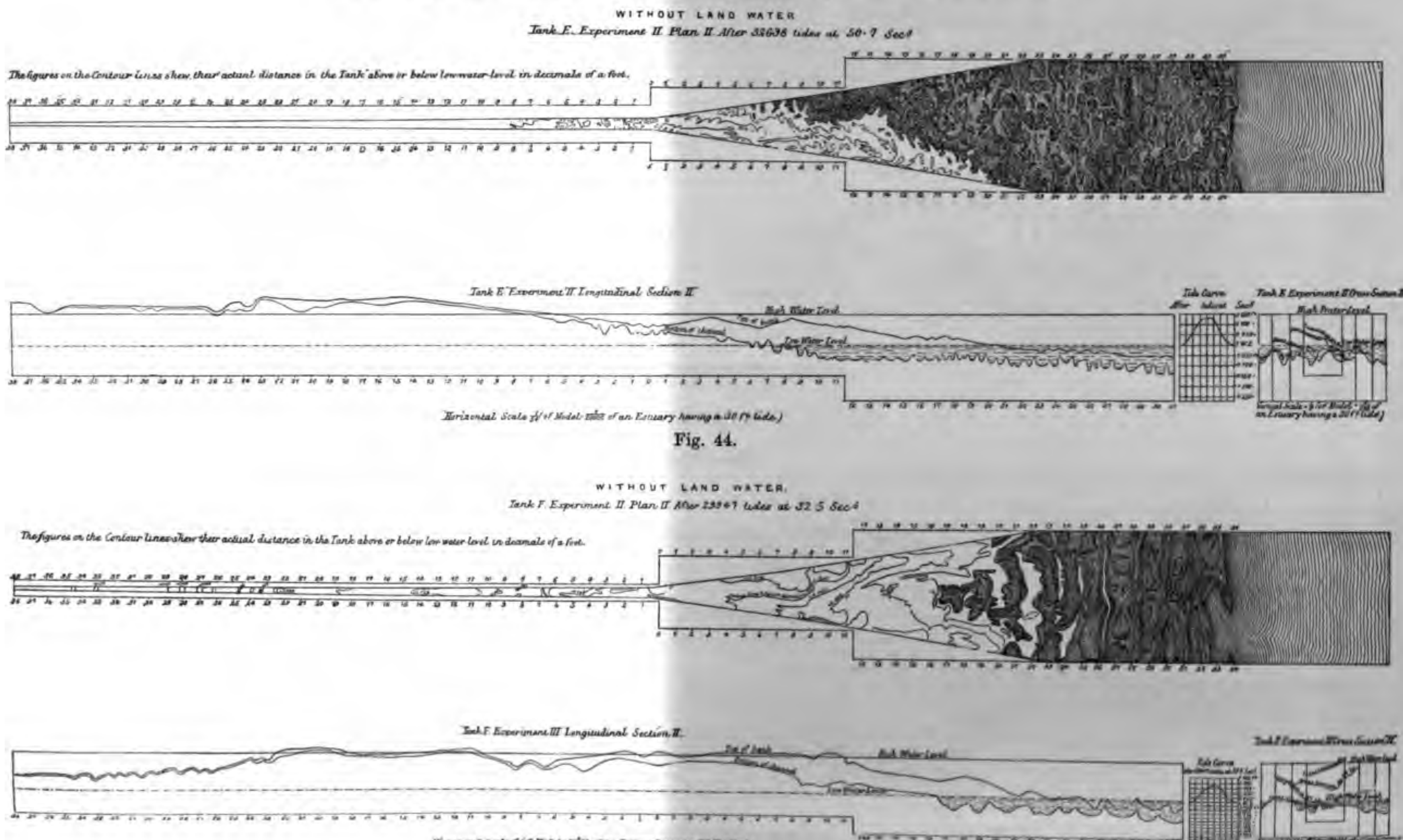


Fig. 44.

On a 30 ft. tide, distance between the sections represents 1·21 miles.

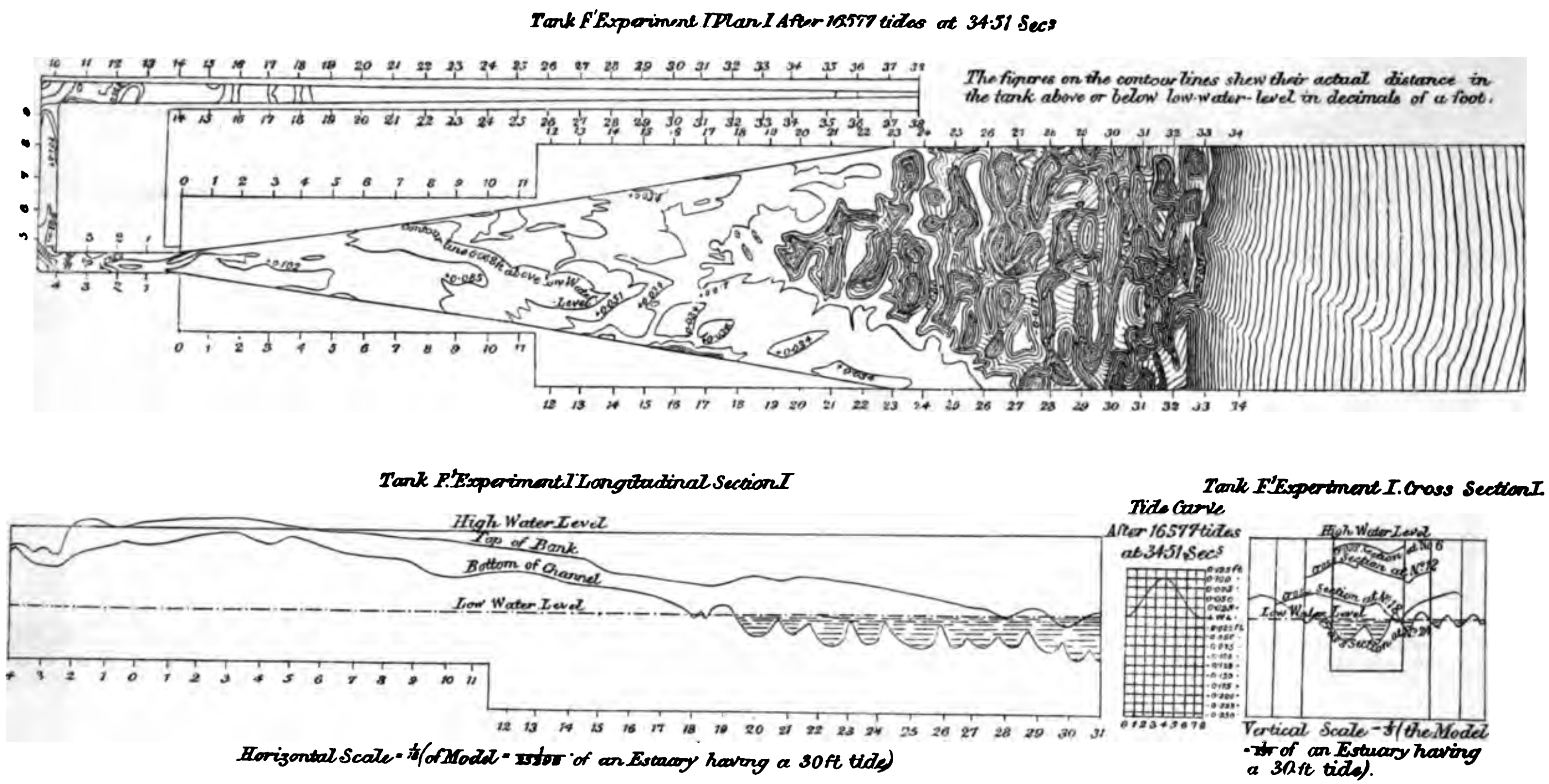
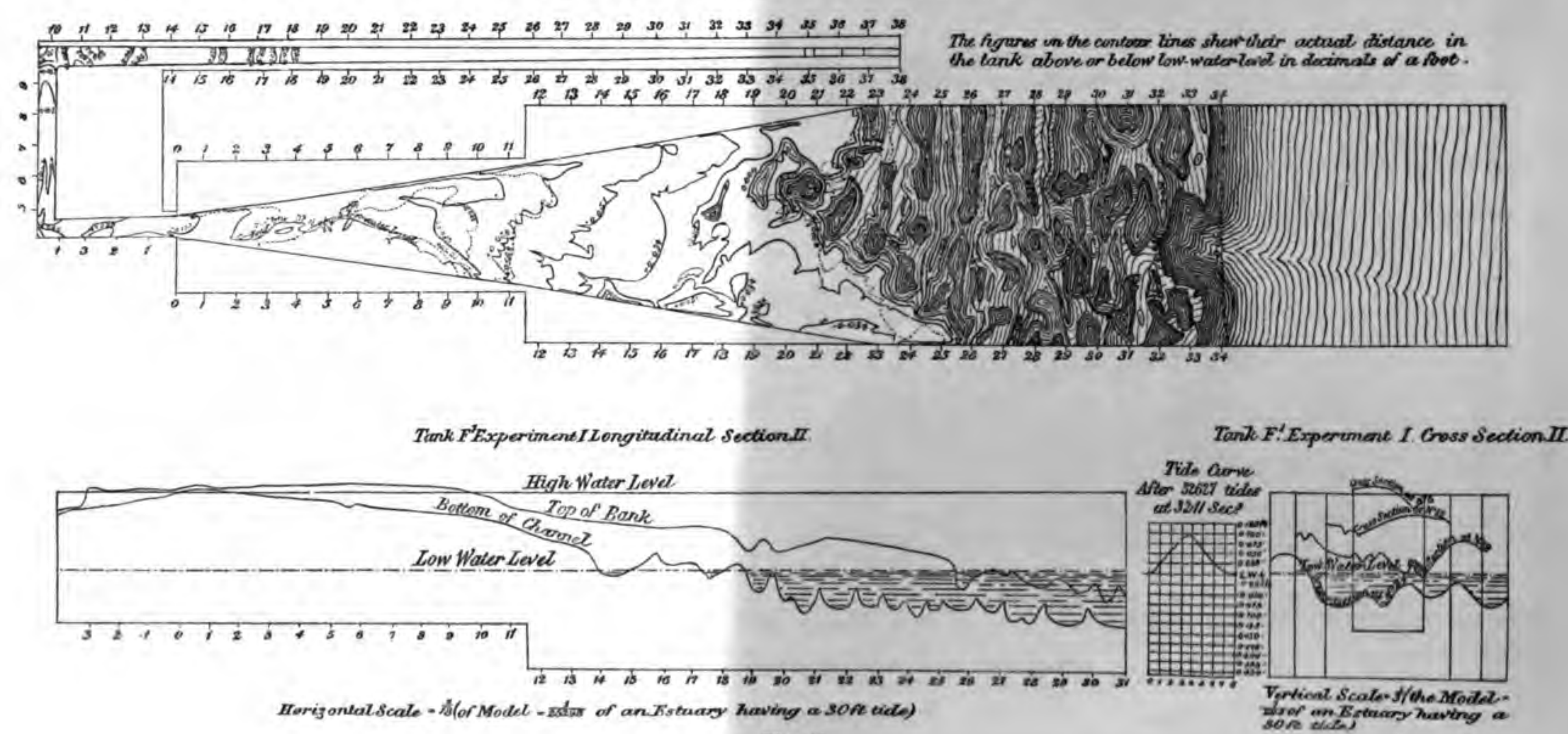


Fig. 46.

On a 30 ft. tide, distance between the sections represents 1·21 miles.

WITH LAND WATER

Tank F[1] Experiment I Plan II After 32627 tides at 32·11 Sec[s]



Horizontal Scale – *(of Model* – *of an Estuary having a 30 ft tide)*

Vertical Scale – *(of the Model* – *of an Estuary having a 30 ft tide)*

Fig. 47.

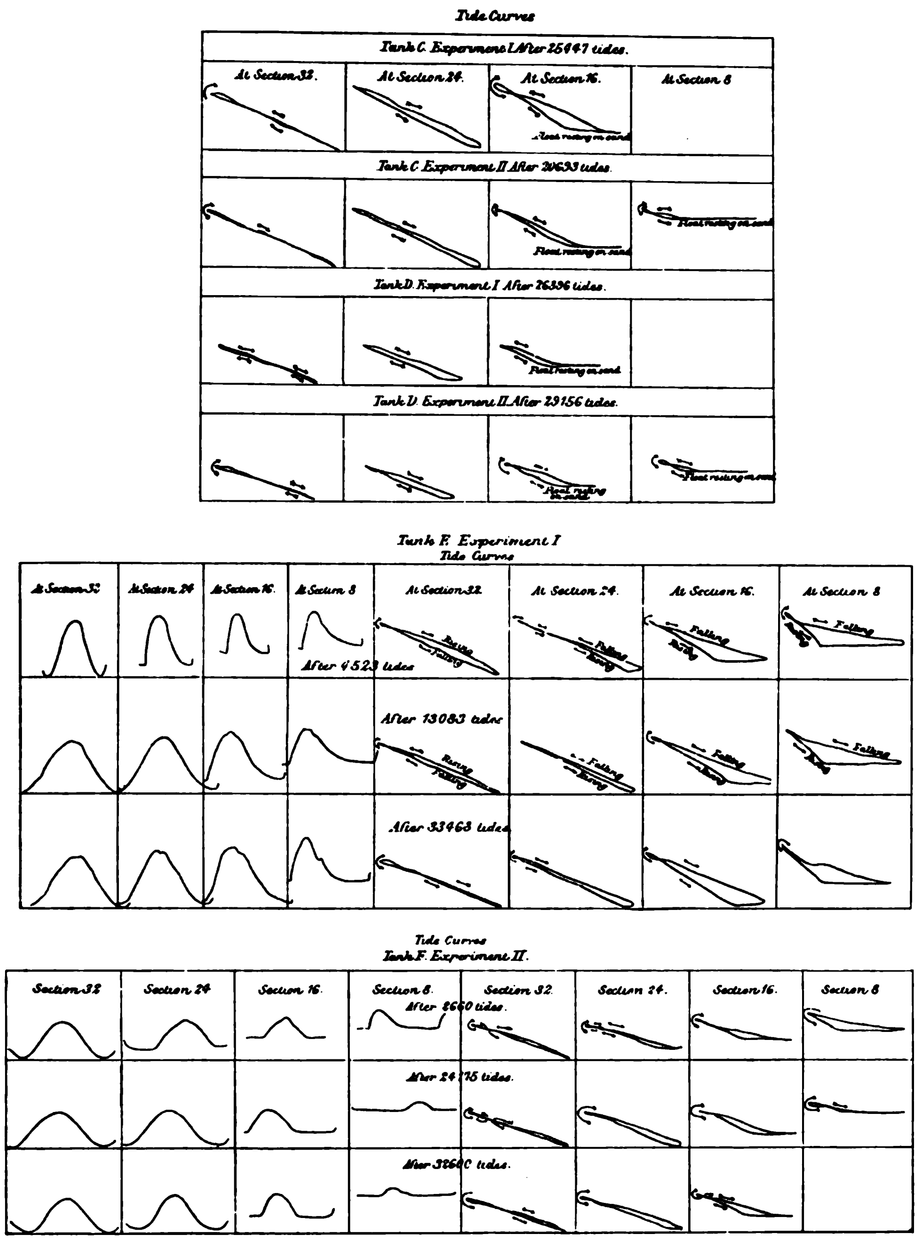

Tide Curves
Tank F. Experiment I
Tide Curves
Tide Curves
Tank F. Experiment II.

Fig. 48.

59.

THIRD REPORT OF THE COMMITTEE APPOINTED TO INVESTIGATE THE ACTION OF WAVES AND CURRENTS ON THE BEDS AND FORESHORES OF ESTUARIES BY MEANS OF WORKING MODELS.

[*From the* "British Association Report," 1891.]

THE Committee held a meeting in the rooms of Mr G. F. Deacon, 32 Victoria Street, Westminster (July 29, 1891), and considered the results obtained since the last report. Professor Reynolds reported that by the date of the meeting of the British Association the objects of the investigation would be accomplished, and suggested that it would not be necessary to continue the investigation beyond that date or to apply to the Association for reappointment. These suggestions were adopted, and it was resolved that the thanks of the Committee be communicated to the Council of the Owens College for the facilities afforded for conducting the experiments in the Whitworth Engineering Laboratory.

Having considered the disposal of the apparatus, which has no pecuniary value, the Committee resolved to recommend the Association to place it at the disposal of the Owens College.

At a second meeting held in the Committee room of Section G at Cardiff the report submitted by Professor Reynolds was adopted.

§ I.—INTRODUCTION TO REPORT III.

1. In accordance with the suggestions in the Second Report, read at the Leeds meeting of the British Association, the investigation has been continued with a view—

(1) To obtain further information as to the final condition of equilibrium with long tidal rivers entering the head of a V-shaped estuary.

(2) To obtain a more complete verification of the value of the criterion of similarity.

(3) To investigate the effect of tides in the generator diverging from simple harmonic tides.

(4) To determine the comparative effect of tides varying from spring to neap.

Opportunity has also been taken :—

(5) To investigate the effect of prolonging the walls of the river into the estuary through the bar, which was below low water, with prolongations reaching up to low water, and others reaching up to half-tide—this being done in both models, so that the similarity of the effects might be seen; and

(6) To investigate the effect of rendering the estuaries unsymmetrical by means of large groins, and so to test the laws of similarity obtained in the symmetrical estuaries as applied to unsymmetrical estuaries.

2. The two models have been continuously occupied in these investigations, when not stopped for surveying or arranging fresh experiments. In this way each of the models has run 600,000 tides, corresponding to 840 years. These tides have been distributed over six experiments in the large tank E, and four in the small tank F, in number from 50,000 to 250,000.

3. The experiments have all been conducted on the same system as described in the previous reports.

All the experiments but one have been made in tanks E and F, without further modification; and in all these land water to the extent of 0·5 per cent. of the tidal capacity per tide has been introduced at the top of the river.

Initially, the sand has been laid to the level of half-tide from Section 13 up the river to Section 26 down the estuary. The vertical sand gauges distributed along the middle line of the estuary have been read and recorded each day. Tide curves have been taken at frequent intervals. Contour surveys have been made, generally after 16,000 tides, and again after 32,000; while in the longer experiments further surveys have been made. With the spring and neap tide, the rate of action being much the slower, intervals between the surveys have been longer. In all 26 complete surveys have been made, and 20 plans showing contours corresponding to every 6 feet, reduced to a 30-foot tide, together with sections and tide curves (page 506), are given in this report.

The general conditions of each experiment, together with the general results obtained, are shown in the table, while a description of each experiment is given in § VI.

The Committee have been fortunate in retaining the services of Mr Greenshields, who has carried out the experiments, observing and recording the results, besides executing such modifications as have been required, designing the compound harmonic gearing for the spring and neap tides, which has answered excellently.

Mr Bamford has kindly continued his assistance in conducting the investigations and reducing the results.

§ II.—General Results and Conclusions.

4. *The conditions of equilibrium with a long tidal river entering at the top of a V-shaped estuary.*—The experiments in tanks C and E made last year led to the conclusion stated in Art. 11 of the Second Report: 'that the effect of a river 50 miles long, when reduced to a 30-foot tide, increasing gradually in width until it enters the top of a V-shaped estuary, is entirely to change the character of that estuary. The time occupied by the water in getting up the river and in returning causes this water to run down the estuary while the tide is low, and necessitates a certain depth at low water, which causes the channel to be much deeper at the head of the estuary. In its effects on the lower estuary the experiments with the tidal river are decisive, but as regards the action of silting up the river further investigation is required, both to establish similarity in the models, and to ascertain the ultimate condition of final equilibrium.'

From this year's experiments, III., IV., V., VI., and VII., in tank E, and V. and VI. in tank F, it appears *that if the length of the tidal river, reduced to a 30 foot tide, is 50 miles; or taking R for the length of the tidal river in miles and h for the rise of tide at the mouth of the estuary in feet, if*

$$R = 8{\cdot}5\sqrt{h}$$

the river will keep open so that the tide will rise to the top, the sand falling gradually from the top of the river to the level of about mean tide at the mouth.

That the depth of water in the river and at the top of the estuary increases rapidly with the length of the river, and when

$$R = 12\sqrt{h}$$

the level of the sand at the mouth of the river will be more than h feet below the level of low water, and the bottom will be below low water level for more than half the length of the river above its mouth.

5. *The similarity of the results in the tanks E and F.*—The experiments in the tanks E and F this year confirm those of last year, in showing that during the early stages of forming the estuary from sand at the level of mean tide, the action in the river is different in the small tank F from what it is in the large tank E, although the value of the criterion of similarity h^3e* may be but little below 0·09.

It was not found practicable to get the value of the criterion any greater in tank F, but it was found on diminishing the rise of tide in the large tank E until the criterion had a value 0·09, that the results were still similar, although the rate of action and the increase in the size of the ripple indicated that the limit was being approached. That the dissimilarity in tank F was only the result of a phase in the formation of the estuary was also definitely shown by the effects of dredging out the sand, which was above the initial level in the river during the early stages of the Experiments V. and VI., after which the action in tank F resumed the same course as that in E, and led to the same final condition of equilibrium, showing by the rate of action and size of ripple that the limit of similarity was approached.

It thus appears that, with such arrangements as these tanks represent, there are two possible conditions of final equilibrium.

The one is that which has uniformly been presented by tank E, and in Experiment V. in tank F after dredging; namely, the tide rising up to the top of the river and keeping the sand low in the estuary. The other, that which was presented in Experiments I., II., III., and IV., in tank F; namely, the sand at the top of the estuary rising to high water level, as it would do if there were no river, choking the mouth of the river, except so far as necessary to allow the land water to pass, and so preventing any tidal action from the river.

Which of these two conditions the river will assume during the process of forming the estuary appears to be a critical matter, decided by whether the tidal action of the river in lowering the sand at the head of the estuary predominates over the tendency of the tide in the estuary to raise the sand at the mouth of the river.

There is a possible condition of instability between the river and the estuary. The emphatic difference in the action of the long tidal river, and

* h is the actual rise in feet, e the vertical exaggeration as referred to a 30-foot tide.

mere tidal capacity at the head of the estuary, in keeping down the sand at the head of the estuary; and, further, the very great effect which an increase in the length of the river has on the depth of water in the estuary and in the river are clearly shown*. In Experiments III. and V. in tank E, an increase of from 50 to 70 miles in the length of the river in V. causing the depth of water to increase from by 40 to 30 feet all down the river and estuary, lowering the sand in the lower river and upper estuary from the level of half-tide to 28 feet below low water. In neither of these experiments was the condition of instability reached, but 50 miles was very near the limit.

In such a state any diminution of the upper tidal waters of the river, by shortening the river or by land reclamation, might well have caused the critical stage to be passed and caused the river to silt up—just as in the other way the increasing of the tidal capacity high up the river by dredging in Experiment V., tank F, caused the critical stage of silting up to be passed and the river to open out. The sand actually removed in this experiment by dredging was 8 per cent. of the tidal capacity, or 400 million cubic yards, removed at the rate of 7 million cubic yards a year.

In most navigable rivers two processes have been going on—dredging and land reclamation—the first tending greatly to improve the rivers and estuaries, the second to deteriorate them so that any improvement has been a question of balance. Where the rivers have improved they will probably continue to improve so long as dredging goes on, but if the dredging should stop, for example in the Thames, there would in all probability be a gradual deterioration, possibly ending in the silting up of the tidal river.

6. *The effect of Tides deviating from the simple Harmonic Law.*—One attempt was made to study this question, when it was found that it would require such modifications in the gearing as were not practicable in the time, and so it was abandoned.

7. *The action of Tides varying from Spring to Neap.*—The rates of action and conditions of final equilibrium in rectangular tanks, in V-shaped estuaries with a long tidal river, and in each estuary rendered unsymmetrical by large groins, have been investigated with tides varying harmonically from spring to neap, and again to spring in 29 tides. The ratio of these at spring and neap being 3 to 2 as compared with uniform tides, having the same rise as the spring tides, also for uniform tides having the same rise as the mean of spring and neap, the results showing definitely:

* See page 506 in which the sections of the rivers and estuaries in tank C, Experiment II., and tank E, Experiments III. and IV. are plotted to the same vertical and horizontal scales.

(1) *That the condition of Final Equilibrium in all cases with spring and neap tides was the same as that with uniform tides having the same rise as springs, and much greater, essentially different, from that with a uniform tide having a rise equal to the mean rise of spring and neap tides.*

(2) *That the Rate of Action with the varying tide is much smaller than that of a uniform tide having the rise of the spring tide. The ratios being definite, about 2·5 to 1.*

(3) *That the limits of similarity obtained for all spring tides hold approximately for tides varying from spring to neap.*

8. *The effects of prolonging the rivers into the estuaries by walls below high water.*—Experiments V. in tanks E and F having arrived at similar final conditions of equilibrium (in which the depth of the rivers for some distance above their mouths was reduced to a 30-foot tide, nearly 30 feet at low water, while the sand in the estuaries gradually rose from the mouths of the rivers until it reached to within 12 feet of low water at a distance of 14 miles below the mouth and then fell again, all the sand being below this level, there being passes which formed a crooked deep water channel), opportunity was taken to prolong the banks of the river by walls at first up to low water and extending through the bar to a distance of 44 miles from the mouths of the rivers. Then raising these walls to half-tide, and finally carrying the walls forward slowly in tank E at a rate of half a mile a year (700 tides), and in tank F dredging from between the walls at a rate of seven million cubic yards a year (700 tides).

This was done in the first place as a further test of the similarity of the action in the two tanks, and secondly as affording an interesting study as to the effect of vertical walls in the direction of the current in the bed of a tideway. The effect of these walls at the level of low water and at half-tide were precisely similar in both tanks; in neither case did they produce any sensible effect at all on the level of the sand between them. At the level of half-tide they caused in both tanks a slight silting up outside the walls and also a slight silting up in the river above its mouth, which effects were very much increased when the walls were raised to half-tide. On the walls being removed in tank E and then gradually carried forward, the silting up behind the wall and deterioration of the river increased, but there was no improvement in navigable depth between the walls.

The dredging in tank F, so long as it was continued, added about 20 feet on a 30-foot tide or 10 feet on a 15-foot tide, to the navigable depth between the walls, but there was the same silting up behind the walls and the same deterioration in the river.

It thus appears that the similarity of the results in both tanks supports the conclusion *that vertical walls having the horizontal direction of the current in a straight tideway and terminating well below high water, produce but little effect on the distribution of the sand between them, so long as the passage is freely open at both ends, but that if the passage be blocked at one end they form a bay in which the sand rises at the head.*

9. *The effects of the tide in estuaries not symmetrical.*—Having so far, in accordance with the original scheme of this investigation (First Report, 1889, p. 5), simplified the circumstances which influence the distribution of sand by maintaining the lateral boundaries perfectly symmetrical, and as nearly rectilinear as practicable, and having found definite laws connecting the distributions of sand in the beds of the model estuaries with the period and rise of the tide and the length of the estuary, besides the laws connecting the period of the tide with the horizontal and vertical scales under which the models give similar results, there remained two questions:

(1) How far such discrepancies as appear between the general distributions of sand found in the models, and those observed in actual estuaries, are attributable to irregularities in the boundaries of the latter?

(2) How far the influence of these boundaries is subject to the same laws of similarity as those already obtained?

The original experiments of the author in models of the Mersey which led to the appointment of the Committee (see page 326) had to a great extent answered these questions, showing that similar irregularities in the lateral boundaries exercise similar and predominating influences on the lateral distributions of the sand in the models and in the estuaries.

It seemed, however, desirable, so far as time allowed, to confirm these results of the author's and make this investigation complete in itself, by carrying out experiments in both models similar to those already carried out, except that the boundaries should be boldly irregular.

Such experiments also afforded opportunity for studying some general effects of great importance. The relation between the depths of water and the rise of tide had come out very definite in the symmetrical experiments, and it was desirable to see how far these relations would be disturbed by lateral irregularities. For instance: (1) Would bold irregularities in the boundaries of the estuary alter the depth of water in the river? Bold irregularities in the boundaries, causing the water to take a sinuous course, would have the effect of virtually narrowing and increasing the length of the estuary, and by causing eddies would obstruct the passage of the water to some extent. Lengthening the estuary would tend to increase its depth at corresponding points, and obstructing the water would tend to diminish the tidal action in the river; at all events, until the estuary had increased in depth.

(2) At the mouth of the estuary the flow of water had so far been straight up and down, and equal all across the estuary. By rendering the mouth unsymmetrical, circulation would be set up which would render the up-currents stronger at one part and the down-currents stronger at another, an effect which would correspond to some extent to that of tidal currents across the mouth of the estuary.

(3) The large tidal sand ripples below low water in the model estuaries, with the flood and ebb taking the same course, constitute a feature which it is impossible to overlook, yet the existence of corresponding ripples had been entirely overlooked in actual estuaries, until, when they were looked for, they were found to exist, having been first seen in the models. The reason that they were overlooked before is, no doubt, explained by the fact that the bottom is not visible below low water in actual estuaries; but this is not all. In the estuaries, these ripples, where found, have been confined to the bottoms and sides of the narrow channels between high sand-banks, and they do not occur on the level sands below low water towards the mouths of estuaries to anything like the same extent as in the models. By rendering the estuary unsymmetrical and so causing the ebb and flood to take different courses, this effect, as explaining the greater prevalence of ripples with symmetrical estuaries, would be tested.

These considerations led to the repetition of Experiment V. in tank F, at first with a single groin extending from the right bank into the middle of the estuary at the mouth, and subsequently to the introduction of three more groins from alternate sides of the estuary to the middle, up the estuary, and then to the introduction of similar groins into tank E, during Experiment VII., with spring and neap tides.

The result of these experiments is to show conclusively:

(1) *That the laws of similarity found for symmetrical channels with uniform tides hold with sinuous channels for uniform or varying tides.*

(2) *That the greater uniformity of the depth of sand on cross sections of models with symmetrical boundaries than with actual estuaries, does not exist when the banks are equally irregular.*

(3) *That the circulation caused by the unequal flow of the tide in model estuaries tends greatly to take the sand out, and that the natural tendency in an estuary to scarp the boundaries so as to increase its sinuosities tends greatly to the deepening of the channels.*

(4) *That in the models with boldly irregular boundaries the tidal ripples are much less frequent than in the symmetrical models, being confined to places where there are no cross currents, as in actual estuaries.*

10. *Conclusion of the Investigation.*—It seems that the objects of this investigation have now been accomplished.

The investigation of the action of tides on the beds of model estuaries has been found perfectly practicable. Two tanks have been kept running night and day from June 22, 1889, to August 1891, and have each accomplished upwards of 1,200,000 tides, representing the experience of 2,000 years. Such difficulties as protecting the sand from extraneous disturbance and keeping it free from fouling, regulating the levels of the water, the tidal periods, the rise of tide, forms of the tide curve and the supply of land water, observing and recording the results, have all been fairly overcome, though none of the precautions taken could have been safely dispensed with.

The limits to the conditions under which the results will conform to the simple hydrokinetic law of similarity have been fairly established; while above these limits the applicability of the simple hydrokinetic law to these experiments has been abundantly verified in models varying in scale from six inches to a mile to an inch and a half to the mile, and with vertical exaggerations, as compared with a 30-foot tide, ranging from 60 to 100.

The laws of the distribution of the sand in a tideway under circumstances of progressing complexity have been determined, and have been verified, not only by repetitions of the same experiment, but also by producing similar distributions under different circumstances, which circumstances, however, conformed to the laws of hydrokinetic similarity. Thus the distributions of sand in simple rectangular estuaries, V-shaped estuaries, and V-shaped estuaries with a long tidal river, have all been investigated and found to be definite.

Investigations have also been made, with definite results, of the separate effects of land water in moderate quantities, and of the length of the tidal river on the depth of water in the river and estuary, and of the effect of bold irregularities in the configuration of the lateral boundaries of the estuaries, also of training walls in deep water. And, lastly, the comparative rates and ultimate action of uniform tides, and tides varying from spring to neap, have been determined.

It thus appears that this system of investigation has been tested over a great portion of the ground it is likely to cover, and that most of the difficulties that are likely to occur have been met, and the necessary precautions found.

It would seem, therefore, by carefully observing these precautions, the method may now be applied with confidence to practical problems.

§ III.—The Apparatus.

11. *General Working of the Apparatus.*—All the apparatus has worked well, although certain repairs have been rendered necessary by wear; thus, the motor has required new pins, not much, considering it has made over 200 million revolutions. The knife edges, on which the generator of the large tank rests, which are of cast-iron, and 2 inches long, and each carry about 1,000 lb., were found to have, after one million oscillations, worn down $\frac{5}{16}$ of an inch, until they had become so locked in the Vs as to stop the motor.

12. *The modifications in the Tanks* have this year been confined to the introduction of training walls and groins. These have been made of paper saturated with solid paraffin (which gradually became warped by the pressure), sheet zinc, and sheet lead or wood, as was most convenient. In the last experiment the large tank was modified by taking out the partition boards and stopping the opening at the end so as to reproduce the original rectangular tank A.

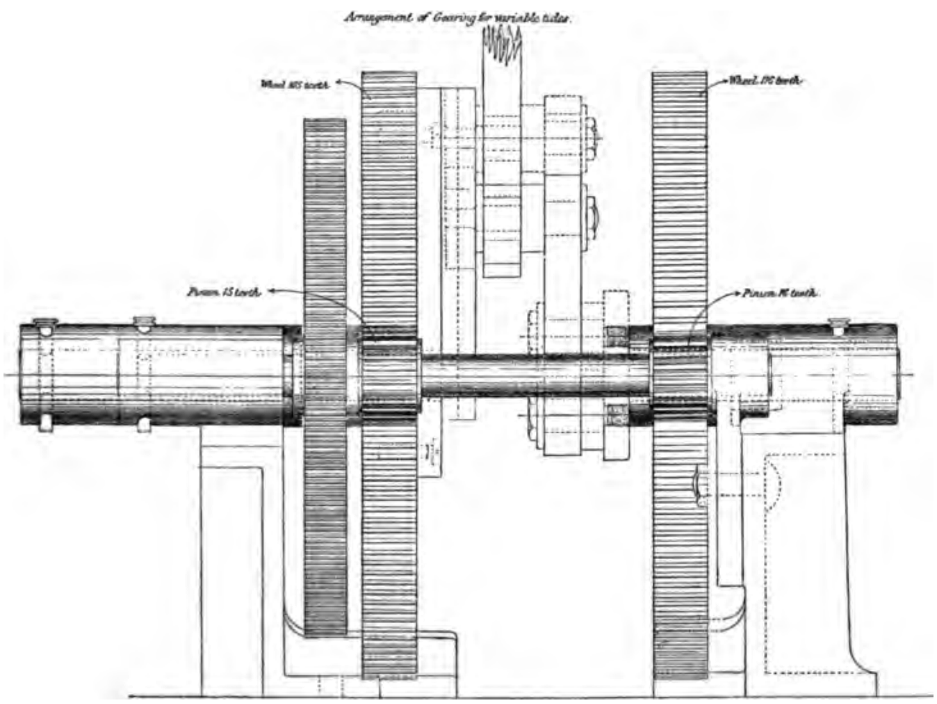


Fig. 1.

13. *Gearing for the Spring and Neap Tides.*—This arrangement, designed by Mr Greenshields, accomplished the result very neatly and effectually with a minimum of new appliances. It admits of any degree of adjustment in the *ratio* of maximum and minimum tides, and works easily and well.

On commencing the work with spring and neap tides it was found essential to have an indicator of the phase of the tide, which would be easily visible without having to examine the gearing. For this, a counter, having twenty-nine teeth in the escapement wheel, which carried a long finger over the face, was constructed by Mr Greenshields, and worked well, proving a great convenience.

§ IV.—Description of the Experiments on the Movement of Sand in a Tideway, from September 4, 1890, to August 1891.

14. *Experiment III., Plan 1, Tanks E and F, Fig. 4, Page 507.*—These experiments were intended as a repetition of Experiments I. C and D. (Second Report, p. 429), which were only continued to 36,000 tides. The only difference in the conditions being that, while in Experiment I. the sand was initially laid up to the top of the river, Section 38, in Experiment III. the sand was only laid up the river to Section 13. These experiments were carried on during the vacation, Mr Foster kindly keeping the tanks running and reading the counters daily. In this way 47,000 tides were run in tank E, and 66,000 in F, when the surveys for Plan 1 were taken.

These surveys show a rather more advanced state than is shown in Plan 2, Experiment I., but they present exactly the same characters. In tank E the sand in the estuary is slightly lower in the longer experiment than in the shorter, but shows the same distribution. In both experiments in tank E the level of the sand at the mouth of the river is that of mean tide, and in both experiments the level of the sand reaches the H.W.L. in the generator at Section 11, or 13 miles up from the mouth, and in both the tide continued to rise to the top of the river.

In tank F, also, both experiments show the same general distribution of sand in the estuary and river. In the estuary the phenomenon, previously observed, with a low value for the criterion, namely, the large ripple, is more pronounced in the longer experiment; but in both experiments the river has become barred at an early stage, showing that the conditions in F, during the formation of the estuary, have been below those essential for similarity.

The rise of tide observed at the end of the Experiment III. in both E and F is below those observed at the earlier stages. In tank E the rise of tide with the same rise in the generator has fallen to 0·125 foot at 47,000 tides, though it was 0·140 foot at 32,000; and in F it was 0·095 foot at 66,000 against 0·096 foot at 32,000. This phenomenon, which becomes more pronounced in some of the later experiments, is accounted for by the improved tideway as the experiment gets older, allowing the estuary to empty itself more completely. It requires notice, since it renders estimates, such as the value of the criterion of similarity, based upon the rise of tide, difficult. The same quantity of water passes up and down the estuary, but does not effect the same rise of tide at the generator, which falls as the experiment gets older, while the rise of tide up the estuary increases at the same time.

15. *Experiments on Increased Length of Tidal River. Experiments IV., E and F, with Land Water, Figs.* 3, 5, 11, *pp.* 506, 508, 514, *October* 22 *to November* 17, 1890.—The sand laid 0·333 foot in E, and 0·187 in F from Section 13 up the river to Section 26 down the estuary. Mean rise of the tide, 0·310 in E, 0·197 in F. Rise of the generators the same as before, periods 33·47 in E, 22·21 in F.

The conditions were thus the same as in Experiment III., with the exception that the tidal periods were reduced in the ratio 1 to $\sqrt{2}$. As reduced to a 30-foot tide, this would have the effect of increasing the horizontal scales in the ratio $\sqrt{2}$ to 1. Thus, while in Experiment III. the estuaries from generator to mouth of tidal river represented about 50 miles, and the rivers 54 miles; in Experiment IV. the estuaries were 70, and the rivers 76.

With the same tide at the mouth, the elongation of the estuary would cause the tide to rise higher at the mouth of the river, but as there was only the same quantity of water from the generator, the tides with the longer estuaries were smaller at the generators, which would again diminish the tides at the mouths of the rivers. The tides observed at the mouths of the rivers were somewhat higher than in Experiment III. And this fact must be allowed for in considering the results as representing the effect of increasing the lengths of the rivers on the distribution of sand.

In tank E the effect was very remarkable. For the first 5,000 tides the sand rose up the river as far as it was laid, the head of the sand gradually going forward, and the sand falling at the top of the estuary and in the mouth of the river. Somewhat the same appearances appeared in tank F, though it soon became apparent that the advance of the head of the sand was much slower in F, and also the lowering of the sand at the top of the estuary. Sand was going up the river, but it accumulated in the lower reaches.

In E, at 9,000 tides, there was an almost sudden change; the sand in the river was rapidly carried to the top, leaving the lower reaches empty. After 11,000 tides the bottom of the river was swept clean from the mouth to Section 15 (30 miles), and then a steady downward movement of the sand went on, all down the estuary, until there was deep water all the way down from 10 miles below the head of the river. The clearing of the bottom of the river of sand evidently increased the action of the river, increasing greatly the rise of tide.

In tank F the result was very different; instead of the sand shifting suddenly up the river, the sand reached Section 15, and then barred the river at Section 11, the river then gradually filling up. At 38,000 tides, when the second survey was made, the tide was still rising at the top of the river, and the head of the sand still proceeding forwards. The experiment was continued to 81,000 tides, and the head of the sand reached Section 19, the tide still rising at the head very slightly. This shows that the conditions of similarity were more nearly fulfilled in the river in tank F in this experiment than in III. The values of the criterion, however, given in the table, are lower in IV. than in III. This is because these values are calculated from the rise in the generators, which were in these experiments 0·110 in tank E, and 0·081 in F, against 0·125 and 0·095 in Experiments III. With the same water going out of the generator there must have been higher tides at the mouths of the rivers in IV., and as the vertical exaggeration in Experiment IV. was $\sqrt{2}$ times larger than in I. and III., assuming the rise of tide in tanks E and F, Experiments III. and IV., to be as in Experiments I., the values of the criterion in Experiments IV. would be at least 0·261 and 0·103. This is in accordance with the observed results.

It seems therefore that in order to apply the criterion to the conditions of similarity at the top of a long estuary with a tidal river, the actual rise of the tide at the mouth of the river should be taken in estimating the value of the criterion for similarity at these points. It appears, however, that in every case where the criterion, estimated from the tides in the generator, exceeded the value ·09, the conditions of similarity have been fulfilled, while in no case has it fallen decidedly below this value without decided symptoms of dissimilarity having appeared, so that this value for the criterion seems to be established as a good working rule for the formation of an estuary from sand at the level of half-tide.

If the bottom of the estuary is modelled the case is different, but the occurrence of large ripples, in experiments in tank F and in Experiment V. in tank E, when the value of the criterion fell as low as ·08, shows that the similarity of the ripple depends on the same value of the criterion as the formation of the estuary.

16. *Experiments with Limiting Value of Criterion.—Experiment V. with Land Water, Tank E, Figs.* 6, 7, *and* 12, *pp.* 509..., *from November* 20 *to December*, 24, 1890.—The conditions of this experiment were designed to bring the value of the criterion, estimated from the rise of tide in the generator in the final condition of equilibrium, to 0·09, keeping the horizontal scale as nearly as possible the same as in IV., and diminishing the rise of tide so as to increase the proportional depth of sand in the river, and thus prevent the bottom being swept clean when the final condition was reached.

The length of the crank working the generator in IV. had been 4·437 inches; this was reduced to 3·77 inches in V., reducing the rise of the tide in the ratio 0·85. To keep the horizontal scale the same the period 33·3 seconds was increased to 36 seconds, leaving the product $p\sqrt{h}$ constant.

This reduced the vertical exaggeration e in the ratio 0·85. Thus the value of h^2e is reduced $(0{\cdot}85)^4$ or 0·52.

Now the value of the criterion in Experiment IV., just before the bottom was swept with sand, was greater than 0·18, which, multiplied by 0·52, gives 0·093.

As carried out at the final condition shown in Plan 3, Page 510, the period was 35·6 seconds, the rise of tide 0·107, and the value of the criterion 0·0912.

This low value of the criterion showed itself in the rate of progress of the experiment. It was 13,000 tides before the sand in the river reached Section 19, against 4,000 in Experiment IV., and 25,000 against 9,000 in IV. before reaching the head of the river. In the early stage of the experiment it seemed doubtful whether the sand was going to bar the river as in Experiment IV., tank F. Except in rate of action, however, the motion of the sand followed the same course as in Experiment IV., taking a sudden shift at about 20,000 tides, and then rapidly lowering the sand at the head of the estuary. At the mouth of the river the bottom of the tank was reached after 50,000 tides, but only between the ripple bars, so that it was not swept clean.

The ripples in this experiment were very much larger than anything before in tank E, showing that the criterion was approaching its critical value.

The final condition of the estuary, as shown in Plan 3, after 36,000 tides, shows conclusively the effect of the upper tidal water in a long river on the bed of the lower estuary. Below Section 19, 32 miles from the top of the river, there is no sand above the level of low water in the estuary, and from this the sand falls uniformly to the mouth of the river, where there is a depth of water, at low tide, of 30 feet. In the head of the estuary there is a bar the top of which is only 12 feet below low water; this is at Section 9, or 18 miles below the mouth of the river; below this point the sand gradually falls to the generator.

Comparing this with the results in Experiments I. and III., where the reduced length of the river is only some 50 miles, but in which the rise of tide at the mouth of the river was somewhat greater, the effect of the extra 20 miles length in the river is seen to have improved the general and navigable depth of the river and estuary, from the top of the river to a distance of 40 miles down the estuary, by from 40 to 30 feet.

17. *The effects of dredging in the river, Experiment V., in Tank F, from November* 19 *to December* 23, 1890, *Plan* 3, *Page* 510. The initial conditions of this experiment were the same as those of Experiment IV. in tank F, except that the mean level of the tide was raised to 0·016 above the initial level of the sand, and the period was increased from 22 to 23·3 seconds. The experiment was undertaken with the intention of ascertaining (1) whether raising the mean level of the tide above the initial level of the sand, without altering the rise of tide, would prevent the river becoming barred; and, supposing this did not succeed, (2) to ascertain whether, if the bar, which had hitherto formed in the river during the early stages of the experiments in tank F, were kept down by dredging out the sand as it rose above the initial level, the later stages would follow the same course as in tank E.

The results were remarkable, and bring out the critical character of the conditions at the mouth of the river.

The experiment was allowed to run 30,000 tides, during which the progress of the sand was much more rapid than in IV., reaching Section 19 in 6,000 tides, as against 36,000 in Experiment IV. and 13,000 in Experiment V, E., and reaching Section 23 in 16,000. At this point it stuck, and the sand accumulated at the head of the estuary and in the river, which became barred at Section 19, on reaching 30,000 tides.

It thus appears that lowering the initial level of the sand produced an effect on the first action very nearly equal to increasing the rise of tide by double the amount, but that as the sand distributed itself this effect passed off.

At 30,000 tides the bar in the river was dredged down to the initial level of the sand, and this level was maintained by daily dredging till 70,000 tides had been run, 0·08 cubic foot of sand in all being removed.

At this stage the sand in the river suddenly shifted up to the top as in Experiments IV. and V., E. The sand at the mouth of the river and top of the estuary falling until the bottom appeared, dredging was discontinued. At 95,000 tides the final condition had been reached, which was almost identical over the whole estuary with that of Experiment V. E after 60,000 tides, as shown in Plan 3, Experiment V., E and F.

The instability of the condition which may prevail at the mouth of a river is thus clearly shown, as well as the useful effect of improving the tideway by dredging in the upper reaches in the river. In three experiments in tank F, I., III., and IV., the river became completely barred, and the estuary became a bay with a stream of land water entering at its top; in Experiment V. the bar again formed, but on being kept down, by dredging, to the level of half-tide, till the sand had fallen at the head of the estuary, the river at length prevailed, and the sand was washed out till there was 30 feet of water at low tide.

The time occupied and amount of sand removed, in producing this effect, were considerable. The tidal capacity of the river and estuary is 1 cubic foot; this reduced to a 30-foot tide is 21,700 million cubic yards, or on a 15-foot tide is 5,422 million. The amount of dredging, 0·08 cubic foot in all, represents 1,743 million cubic yards on a 30-foot tide, or 437 millions on a 15-foot tide. This was distributed over 40,000 tides, or sixty years, so that even with the 15-foot tide it would represent 7 million cubic yards a year.

After the dredging the rise of tide fell from ·081 to ·073 foot, which would result from the lowering of the sand which was above low water.

18. *Experiments with Training Walls. Experiment V. (continued) with Training Walls, Tanks E and F, from January 7 to February 20, 1891, Plan 4, Page* 516.—Having arrived at *similar* final conditions of equilibrium in tanks E and F, in which the sand was entirely below low water from Section 19 up the rivers (32 miles from the top of the river) to the generators, and in which there were bars in the estuary below the mouths of the rivers, reducing the depth of water at low tide from 28 feet in the river to a minimum of 12 on the top of the bars, it seemed an opportunity not to be lost for testing the similarity of the effect in the two tanks of prolonging the rivers by training walls through the bars.

With this view, walls of thick paper saturated with paraffin, pushed vertically into the sand and extending up to low water, were run out from the end of the river, preserving the same divergence as the walls of the river to Section 22, or 40 miles on a 30-foot tide, the tanks being stopped for the purpose.

These walls produced no apparent effect whatever on the depth of sand between the walls, during 20,000 or 30,000 tides. They were then replaced at the upper end by walls of sheet zinc extending to half-tide, which did produce an apparent effect, inasmuch as the sand accumulated outside the walls, forming an apparent channel within; also the sand rose in the river, doing away with the appearance of a bar. These effects were similar in both models after 40,000 tides had been run.

The old walls were removed in both tanks and replaced by walls commencing at $\frac{3}{4}$ tide at the mouths of the rivers, and falling during the first 4 or 5 miles to half-tide, at which they were continued to Section 22.

In tank E the walls were advanced gradually from the mouth of the river at a rate of about half a mile in 700 tides (a year). The result of this is shown in Plan 4, Page 516, tank E. There is no improvement in the navigable depth of the river.

In tank F the walls were put in and then the tops of the ripple bars were daily dredged off between the walls. This was continued for 100,000 tides, during which 5 per cent. of the tidal capacity was removed, or about 1,000 million cubic yards on a 30-foot tide, or 250 millions on a 15-foot tide, which represents 7 millions annually on the 30-foot tide, or 1·8 millions on a 15-foot tide. The effect, as shown in Plan 4, tank F, Page 516, is to add some 20 feet to the depth on a 30-foot tide, or 10 feet on a 15-foot tide.

The silting up behind the walls is the same as in tank E, and the detriment to the navigable depth of the river is also similar.

19. *Experiment V. (continued) with Tide deviating from the Simple Harmonic in Tank E, February* 23 *to March* 12, 1891. This was meant as a preliminary experiment. The balance of the generator was altered to give a rise of tide in 17 seconds and a fall in 20. The experiment was run for about 40,000 tides, and a survey taken, which showed little or no effect. On carefully examining the tide curves it was found that they showed very little inequality in the rise and fall. On attempting to increase this by further altering the balance, it was found that this could not be done. To continue this part of the investigation it would have been necessary to introduce complex gearing. Time did not suffice for this, and the study was not carried further.

20. *Experiments with Tides varying from Spring to Neap, Tank E, V., VI., VII., VIII., Tank A, XIII. Figs.* 11, 12, 13, 15, *pp.* 514..., *March* 20 *to August* 1891.—The gearing for tank E having been modified so as to cause a rise in the generator, varying to over an interval of 29 tides, the variation being harmonic and adjustable, so as to admit of any relation between the maximum and minimum rise.

These were adjusted so that the mean rise was the same as the rise in

Experiment V., the spring and neap rises being in the ratio 3 to 2. A drain with an adjustable orifice was put in the bottom of the tank to drain off nearly all the fresh water, and the scummer adjusted so as to draw off the excess of land water at low spring tide level; this being adjusted by trial until, when running, the mean tide level was the same as before.

Experiment V. was then restarted, without the sand having been disturbed, to afford a preliminary trial of the *apparatus*, the period being that of Experiment V., 36 seconds. This was continued 18,000 tides, till the apparatus was completely in hand; then the sand was relaid for Experiment VI., Fig. 11, Page 514, in which the conditions were the same as V., except the tide. The mean rise in the generator was the same in VI. as in V., and the ratio of the spring and neaps 3 to 2. This brought the rise in the generator at spring tides in VI. greater than that in Experiment IV., in the ratio of 1·1 to 1. The action on the sand was much more rapid than in Experiment V. with the uniform tide, being nearly as quick as in IV. The sand reaching the top of the river in 13,000 tides, as against 10,000 in IV. and 25,000 in V., and the bottom of the river being swept as clean in 17,000 tides in VI., as in 14,000 in IV. In other respects the action in VI. very closely resembled that in IV. The rate of action was a little slower, but the action itself seemed rather stronger, as corresponding to a higher tide. Surveys were taken at 20,000 and 34,000 tides. The experiment was then stopped, in order to make the conditions comparable with those of Experiment V.; it being quite clear that the action of spring and neap tides, having a mean rise equal to that of a uniform tide, was not only much more rapid, but led to a different state of final equilibrium.

Experiment VII., Plan 1, *Page* 515. In this the tide was adjusted until the rise of the generator at spring tide was the same as that for the uniform tide in V., the other conditions being all the same.

The character of the action now became identical with what it had been in Experiment V., but the rate was decidedly slower. Thus the sand moving up the river reaches:

Section 19 after 13,000 in V. and 39,000 in VI.
„ 27 after 20,000 „ „ 51,000 in VI.

The survey taken after

18,000 tides in Experiment V., Tank E, and
51,000 „ „ VII., „

are almost identical, the latter being a little the forwardest.

It thus appears that spring and neap tides, having a ratio 3 to 2, produce the same result as *two-fifths* the same number of tides all springs.

So far neither of these estuaries had reached the condition of final equilibrium, but the similarity that the Plans 1, Experiments V. and VII. present, seemed sufficient assurance that this would be the same.

It was intended to repeat Experiment V., tank A, as soon as the tank had been re-formed to its rectangular shape; in the meantime groins were introduced in tank E similar to those which had been used in Experiment VI. F, and Experiment VII. E was continued, to ascertain how far similar effects would be produced by varying and uniform tides in estuaries with similar but boldly irregular outlines.

Experiment VII. E, Plan 2, Page 517 was continued with groins to 123,000 tides. Similar groins had affected the condition of the sand in the estuary and river in Experiment VI., tank F, so that further comparison between Experiments VII. and V. cannot be made.

Experiment XIII., Tank A, rectangular without land-water, spring and neap tides, Plan 3, Page 512, from July 10 to August 10, 1891.—In this experiment the rates of spring and neap tides were 3 to 2, and the rise of tide at spring tides was 0·176, the same as in Experiment V., tank A. The tank was reduced to its original rectangular form (Report I.), namely, 4 feet broad, and 12 feet from the generators to the top. The sand was laid as in Experiment V., tank A, at a depth of 2 in. from Section 18 to the top of the tank, and the mean tide was adjusted in Experiment V., tank A. The period was 50 seconds, as in tank A. Thus the conditions of Experiment XIII. and V., tank A, were precisely the same, with the exception that while the tides in Experiment V. were all springs, those in Experiment XIII. varied from springs to neap; the object of Experiment XIII. being to compare the rate of action and final condition of equilibrium with varying tides, with the very definite results, as to the slopes of the sand, obtained in the rectangular tanks, and recorded in Report I., B. A. Report, 1889 (see page 380).

These results are shown in the plans on page 510. The period in Experiment XIII., tank A, being shorter than in V. The actual slope is greater, but the slopes reduced to a 30-foot tide agree.

21. *Experiments on Estuaries not Symmetrical. Experiment VI., in Tank F, with large groins, Plans 1 and 4, Pages 517, 518, from April 8 to June 16, 1891.*—This experiment was started under conditions in all respects similar to those in Experiment V., tank F, with the exception of a vertical groin extending from the right bank to the middle of the estuary, with an inclination of 45° towards the generator, and rising from the bottom of the tank above high water. This groin, which appears in the charts to represent an artificial structure, is, in fact, out of all proportion to anything

of that kind which has yet been attempted. As reduced to a 30-foot tide, it is 11 miles long, 100 feet high up to H.W.L., and half a mile broad. Thus it corresponds rather to such a natural feature as Spurn Head, at the mouth of the Humber, than to a breakwater such as that at Harwich.

In starting the experiment, the end of the sand at Section 26 was 20 miles above the point of the groin at Section 36. The groin had deep water on both sides of it, so that its only effect was to deflect the flood on to the left bank of the estuary.

This effect was very decided, the strength of the flood on the right carrying the sand up the estuary in spite of the effect of the ebb to bring it down. But this in itself was not so much; it was the large eddy caused by the groin which produced the greatest effect. The water entering on the left of the estuary crossed over to the right, and returned along the right bank. In other words, during flood the right side of the estuary for 30 miles from the generator was in back water. This back water also gave the ebb a start down the right bank which rendered the ebb stronger on this side.

The sand came down rapidly on the right side, and besides was carried over from the left to the right, and formed a bank along the right middle of the estuary, reaching the generator after a very few tides. Round this bank the water circulated, carrying the sand with it up on the left and down on the right, the bank growing all the time. The ripple round this bank was very striking, arranged with the ripple heads all down on the right side and up on the left. After about 3,000 tides the sand began to pass from the point of this bar in a fine stream across the open channel, dividing this point from the point of the groin, and commenced the formation of a bank in the generator corresponding to that in the tank. This bank had to be removed from the generator, and after 6,000 tides 4 lbs. of sand were so removed. In Experiment V. the first sand removed from the generator was after 120,000 tides had been run.

The sand also went more rapidly up the river in Experiment VI. than in Experiment V. But this was accounted for by dredging in the river having begun much earlier, after 20,000 tides as against 30,000.

In all 8 lbs. of sand were removed from the river in Experiment VI., against 10 lbs. in V., or about 0·004 of the tidal capacity in VI. against 0·08 in V. In both cases the dredging stopped when the sand began to shift up the river after 70,000 tides.

At 100,000 tides a condition of final equilibrium had been arrived at. The sand in the river was just the same as in V., Plan 3, Experiments V.

and VI. in tank F. There is deep water in VI. up to Section 21, 30 miles from the generator, the levels of the sand being much the same from this point up as in V.

A similar groin was then introduced at Section 16, extending from the left bank to the middle of the estuary. This groin was $4\frac{1}{2}$ miles long and 100 feet high to H.W.L., and 50,000 more tides were run, the river all the time slightly improving. Thus having brought deep water up to Section 14, or about 44 miles from the generator, a groin extending from the right bank to mid-channel at Section 8, about 2·5 miles long and 70 feet high, and another from the left bank to mid-channel at Section 5, 2 miles long and 70 feet high, were put in.

The first effect of these groins was to raise the sand slightly in the mouth of the river; but this improved again, and after 50,000 more tides there was deep water extending from the mouth of the river to the generator, and the river was better than in Experiment V. with the training walls, though not quite so good as before these were put in.

In the meantime the banks had risen in the estuary below the groins, extending down from nearly H.W.L. to the point of the next groin, where there was a pass with water nearly to the bottom of the tank.

The sand carried down into the generator during the experiment amounted to 69 lbs., or 57 per cent. of the tidal capacity. In Experiment V. 24 lbs. were removed in like manner, or 20 per cent. of the tidal capacity. 37 per cent. of the tidal capacity on a 30-foot tide would represent a mean increase of depth over the entire estuary of 11 feet; and as the increase was by no means over the whole estuary, the increase in the channels and lower estuary was much more than this, and although by this time the sand in the estuary had for the most part become quite yellow, sand was still being carried down into the generator.

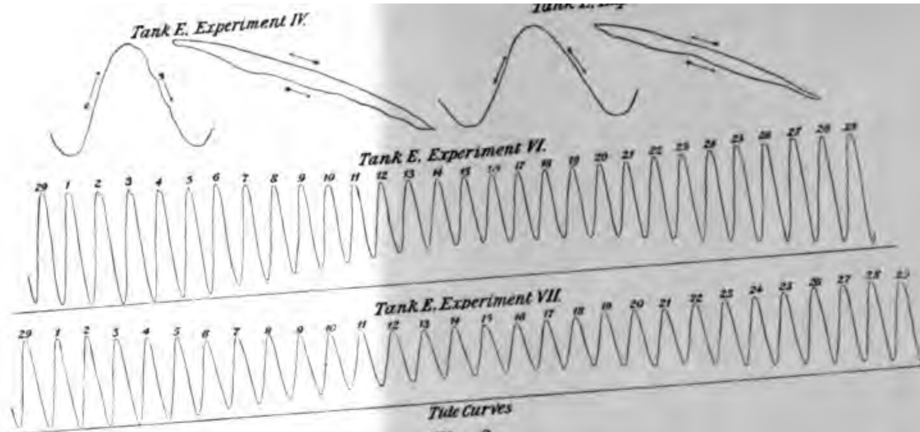
In the meantime, as already stated, groins similar to those in Experiment VI. in tank F, had been introduced into experiment VII. in tank E, after 64,000 tides had been run with spring and neap tides. 60,000 more tides, which would be equivalent to about 27,000 spring tides, were run, the effect being that, notwithstanding the difference in the initial conditions, the state of the lower estuary was closely approximating to the state of VI. in F after 36,000 tides (Plan 2, Experiment VII., tank E; VI., tank F).

In the upper estuary in VII., tank E, the distribution of the sand is precisely similar to that in VI., tank F, but there is rather more of it, which is explained partly by the fact of the difference in the equivalent tides run, 30,000 in E as against 50,000 in F, after the upper groins were put in, and partly by the much greater amount of sand still left in the lower estuary in tank E. Had it been possible to run 250,000 more spring and neap tides in VII., tank E, there is every reason to suppose that the final condition would have been precisely similar to that obtained in Experiment VI. in tank F.

TABLE I. GENERAL CONDITIONS

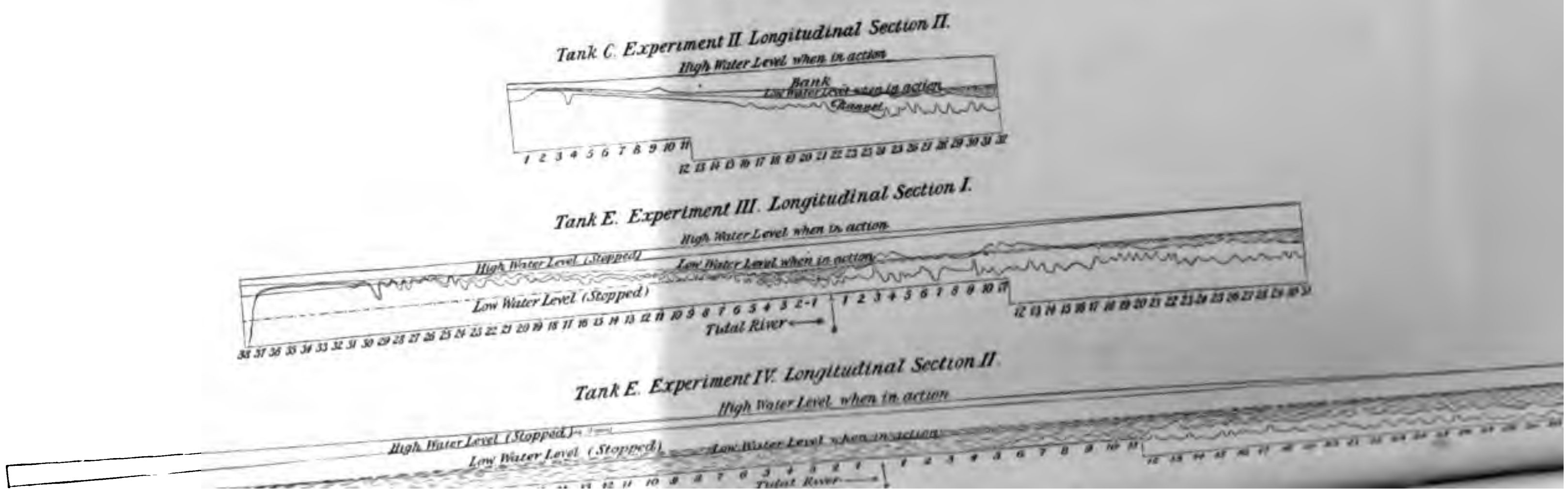
Shape of the Estuary			Percentage of Land Water	Experiment	Tank	Plan	Plan on Page	Shortest period in seconds	Horizontal scale		Vertical scale
									1 in.	Inches to a mile	
V-shaped Estuaries with long Tidal River		50 miles	0·5	III	E	1	507	46·16	14,901	4·25	240
			,,	,,	F	1	507	30·53	25,844	2·45	315
	River, 70 miles		,,	IV	E	1	514	33·47	20,550	3·01	240
			,,	,,	F	1		22·20	38,256	1·65	365
			,,	,,	E	2	508	33·20	22,090	2·78	273
			,,	,,	F	2	508	22·03	38,788	1·63	370
			,,	V	E	1	515	35·6	19,558	3·24	246
			,,	,,	F	1	509	23·68	36,310	1·74	375
			,,	,,	E	2	509	35·6	19,972	3·172	256
			,,	,,	F	2		23·32	36,890	1·718	375
			,,	,,	E	3	510	35·60	20,833	3·03	280
			,,	,,	F	3.	510	23·32	38,955	1·63	416
		Training Walls	,,	,,	E	4	516	35·60	20,691	3·06	275
			,,	,,	F	4	516	23·32	39,700	1·60	435
		Quick rise	,,	,,	E	5		35·60	21,285	2·97	291
		Spring and Neap Tides	,,	,,	,,	6		35·60	19,095	3·318	234
			,,	VI	,,	1		35·78	18,230	3·475	215
			,,	,,	,,	2	514	35·25	20,000	3·168	252
			,,	VII	,,	1	515	35·10	19,756	3·207	244
			,,	,,	,,	2	517	35·10	20,890	3·033	273
			,,	,,	,,	4	518	—	—	—	—
		Unsymmetrical	,,	VI	F	1	517	23·40	39,564	1·605	434
			,,	,,	,,	2		23·40	38,465	1·647	411
			,,	,,	,,	3		23·40	39,854	1·589	411
			,,	,,	,,	4	518	23·40	39,280	1·613	428
Rectangular	Spring and Neap Tides		0·0	XIII	A	3	512	48·00	12,473	5·08	182
	Uniform Tides		,,	V	,,	1	511	50·00	11,758	5·45	170

Rise of tide in feet	Vertical exaggeration on a 30-feet tide e	Criterion of similarity		Height of initial sand in feet	Height of mean tide in feet	Excess of mean tide over initial sand in feet d	Number of tides from the start	Remarks
		$C' = h^3 e$	$C' = (h + 2d)^3 e$					
0·125	62·00	0·121	—	0·333	0·322	—	47,183	Normal.
0·095	81·84	0·070	0·070	0·187	0·187	—	66,369	River blocked.
0·125	85·63	0·167	—	0·333	0·310	—	18,530	River cleaned.
0·082	104·57	0·057	—	0·187	0·182	0·005	21,135	River blocking.
0·110	80·98	0·108	—	0·333	0·308	—	37,755	River cleaned.
0·081	104·73	0·056	—	0·187	0·179	0·008	38,719	River nearly blocked.
0·122	79·53	0·144	—	0·333	0·336	—	17,923	Slow.
0·080	96·82	0·049	—	0·187	0·203	0·016	19,416	Quicker.
0·117	77·88	0·124	—	0·333	0·321	—	37,359	River cleaned.
0·080	98·32	0·050	0·165	0·187	0·203	0·016	37,181	Blocking—Dredged.
0·107	74·48	0·091	—	0·333	0·320	—	65,404	River clear.
0·072	93·49	0·035	—	0·187	0·207	0·020	95,558	River clear.
0·109	75·18	0·097	—	0·333	0·306	—	167,186	Similar.
0·069	91·32	0·030	—	0·187	0·204	0·017	255,200	
0·103	73·08	0·080	—	0·333	0·335	—	208,264	Failure.
0·128	81·47	0·170	—	0·333	0·328	—	226,930	Preliminary.
0·139	84·46	0·2268	—	0·333	0·325	—	20,822	Quick.
0·119	79·33	0·1336	—	0·333	0·317	—	34,394	River clear.
0·123	81·00	0·1507	—	0·333	0·333	—	51,591	Normal.
0·110	76·60	0·1017	—	0·333	0·332	—	101,790	—
—	—	—	—	—	—	—	122,989	—
0·069	91·01	0·0299	—	0·187	0·192	0·005	18,972	—
0·073	93·60	0·0360	—	0·187	0·193	0·006	36,511	—
0·068	93·33	0·0284	—	0·187	0·193	0·006	99,558	—
0·070	91·66	0·0314	—	0·187	0·192	0·005	196,651	—
0·165	68·54	0·3084	—	0·250	—	—	51,240	Similar.
0·176	69·16	0·3769	—	0·250	—	—	16,282	



Tide Curves

Fig. 2.



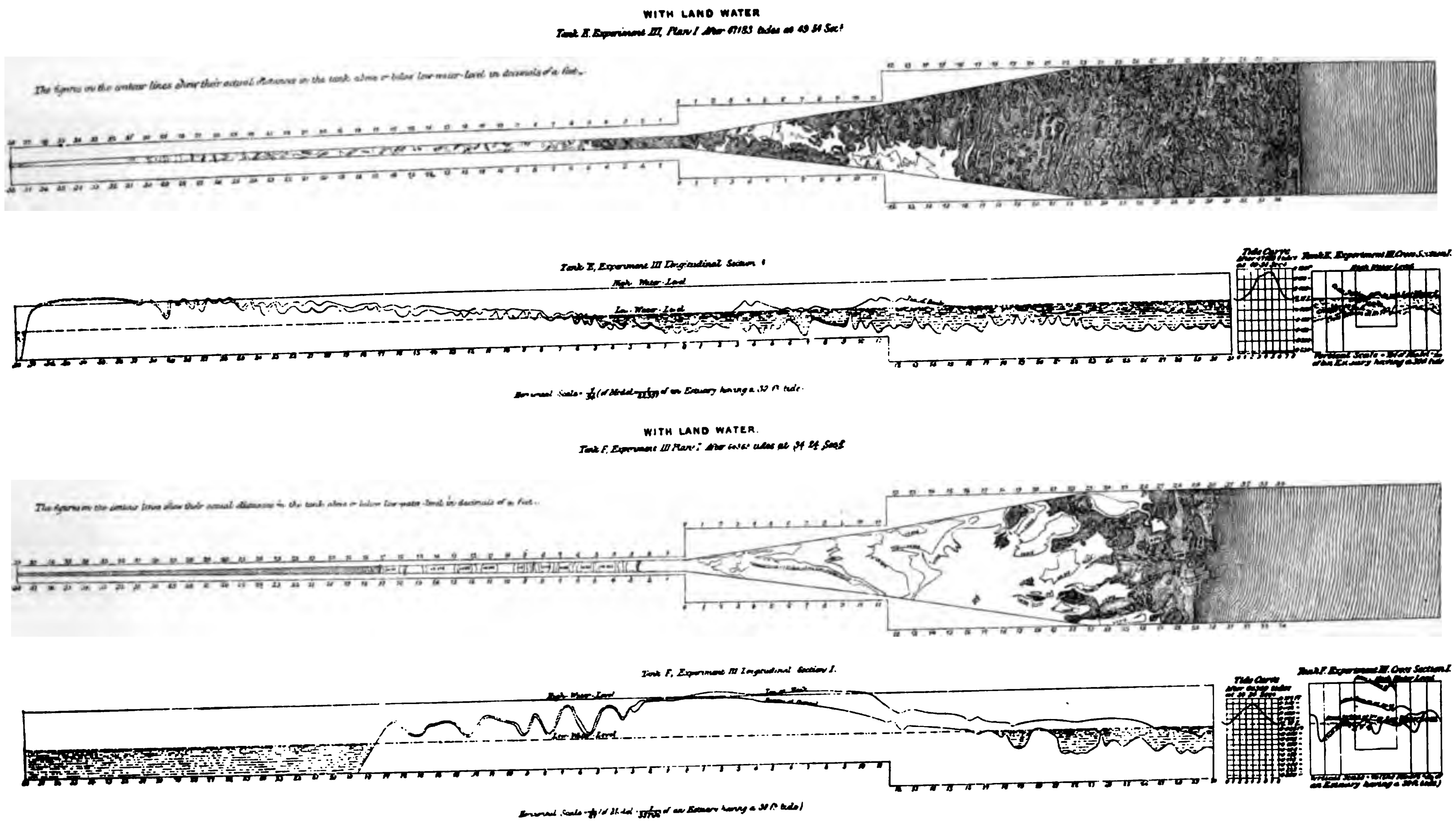

WITH LAND WATER
High Water Level
Low Water Level
WITH LAND WATER
High Water Level
Low Water Level

Fig. 4.

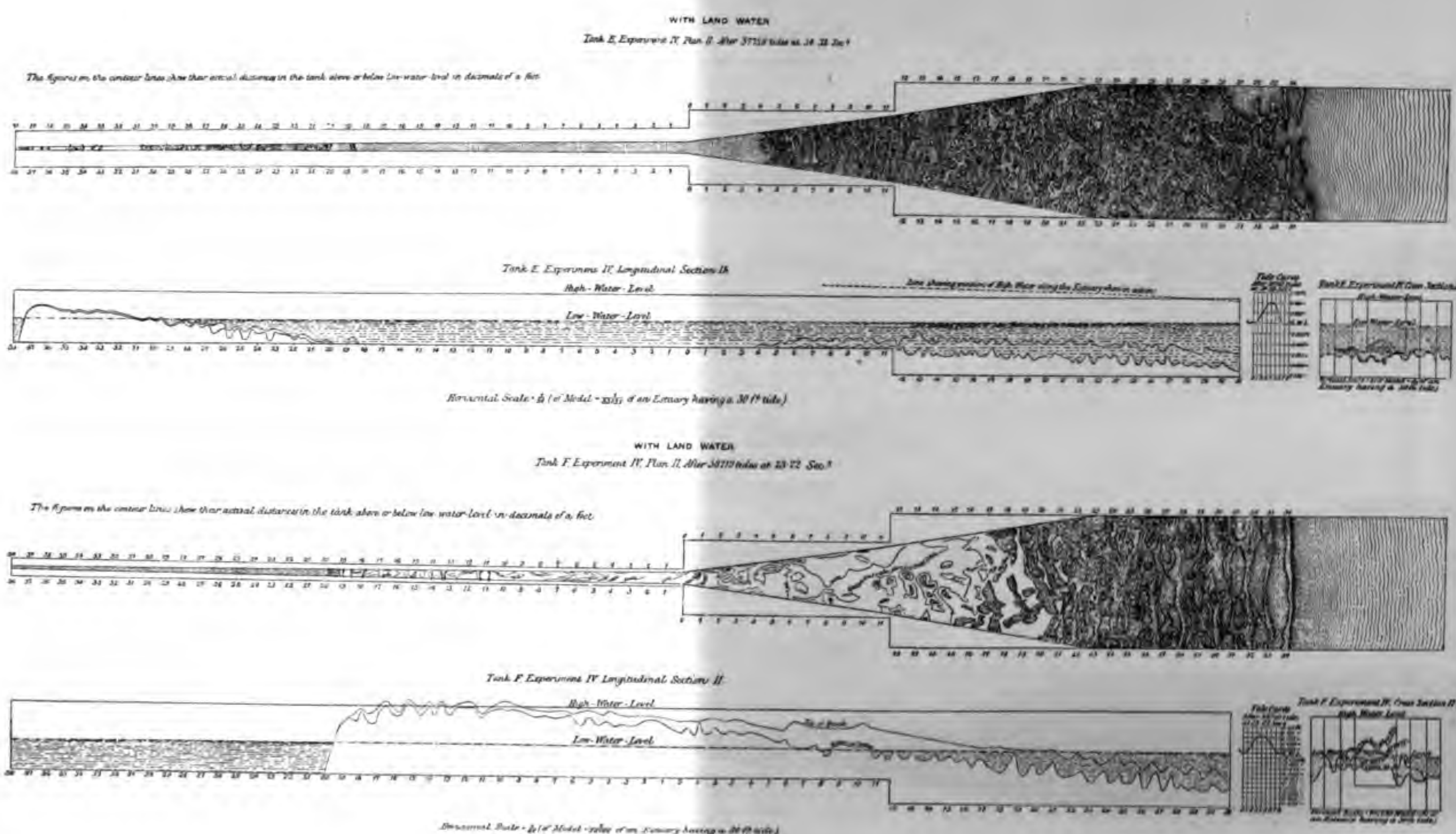

WITH LAND WATER
The figures on the contour lines show their actual distances in the tank above or below low-water-level in decimals of a foot.
Tank E, Experiment IV, Longitudinal Section II
High-Water-Level
Low-Water-Level
WITH LAND WATER
The figures on the contour lines show their actual distances in the tank above or below low-water-level in decimals of a foot.
Tank F, Experiment IV, Longitudinal Section II
High-Water-Level
Low-Water-Level

Fig. 5.

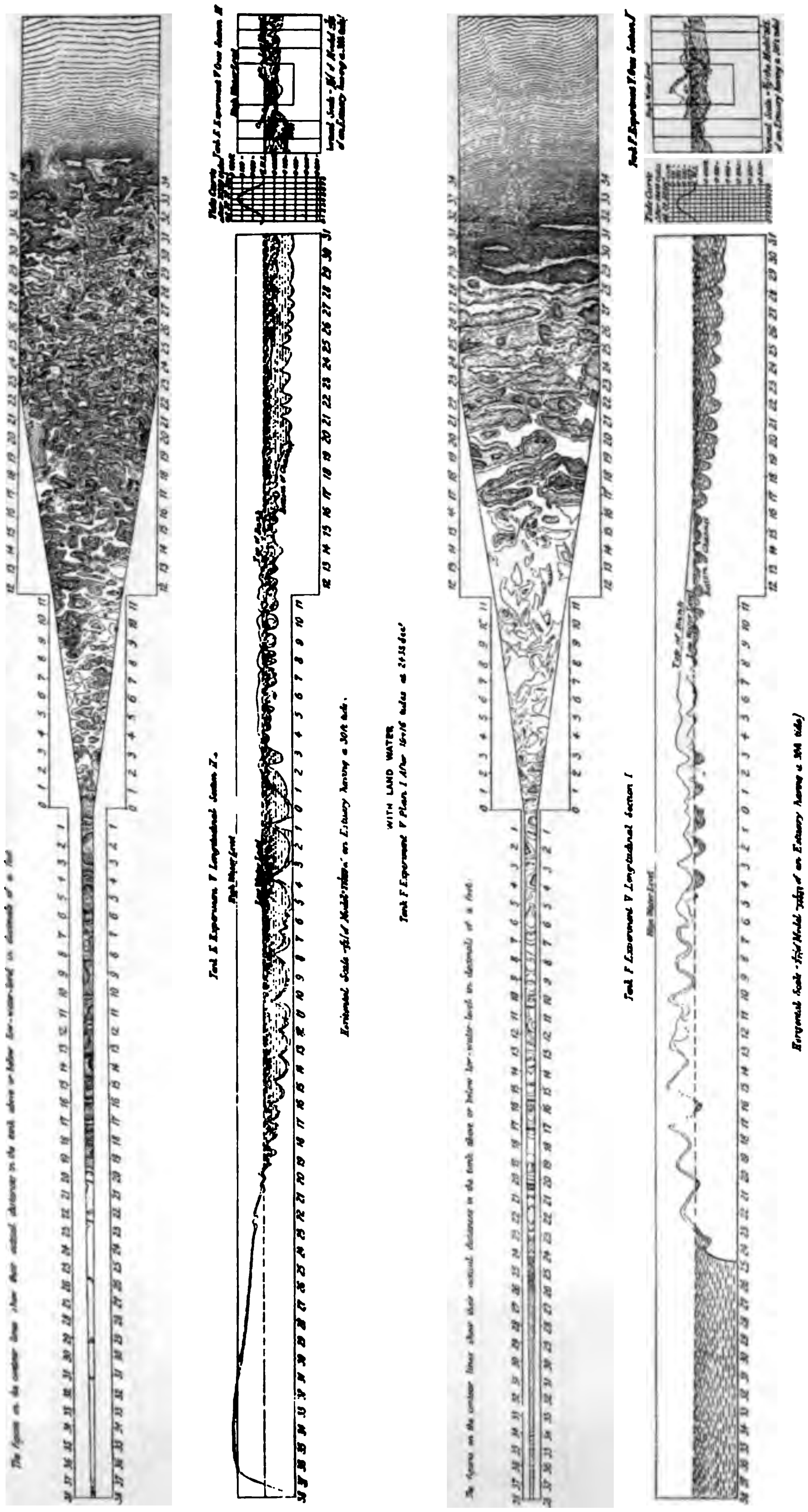


Fig. 6.

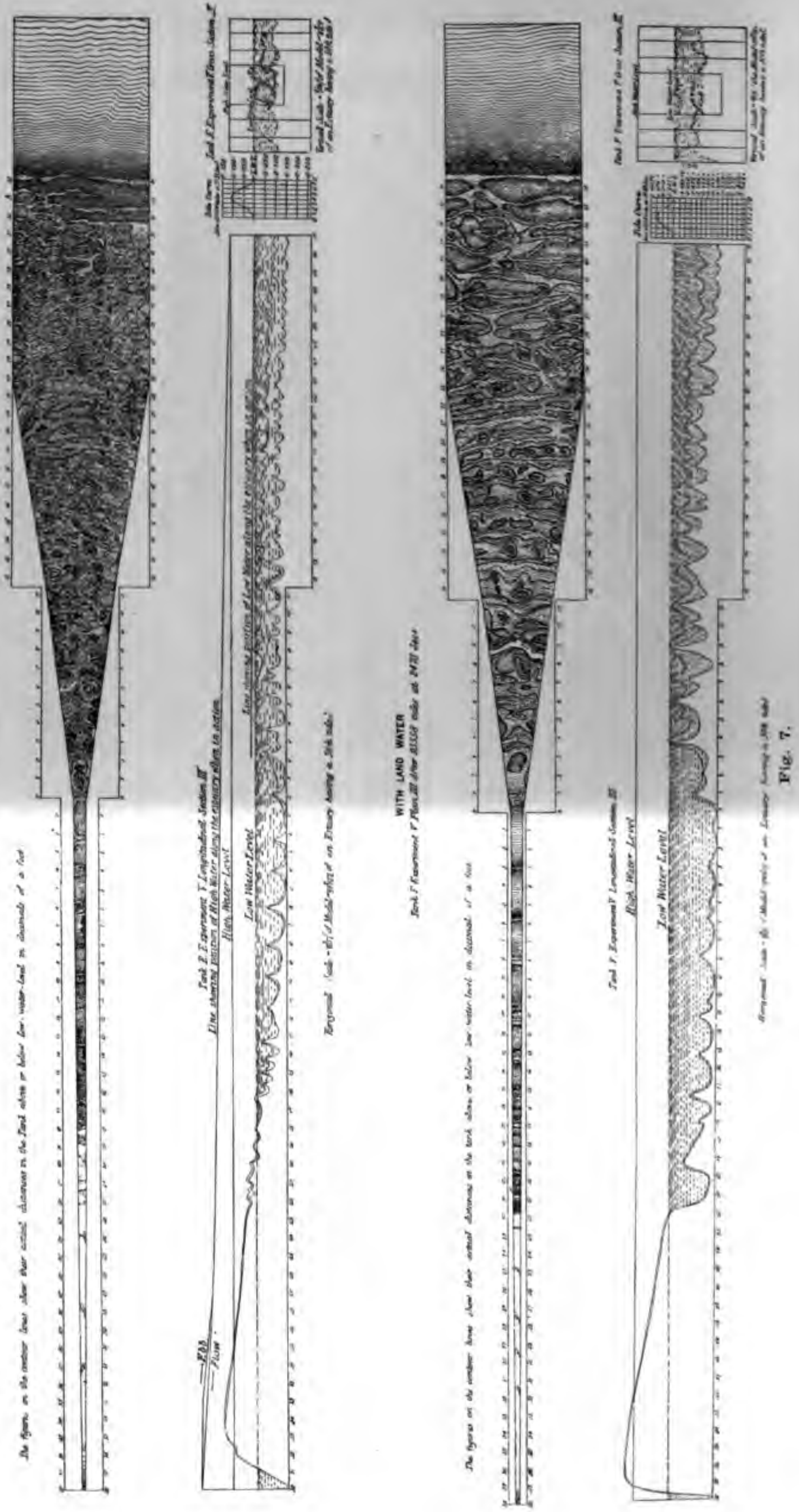
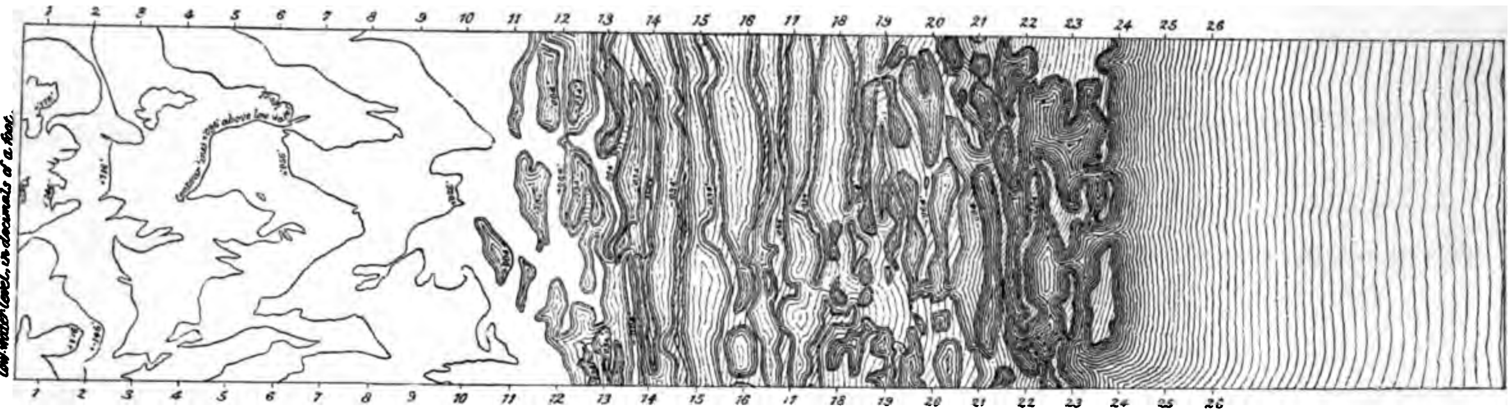

WITH LAND WATER
High Water Level
Low Water Level
WITH LAND WATER
High Water Level
Low Water Level

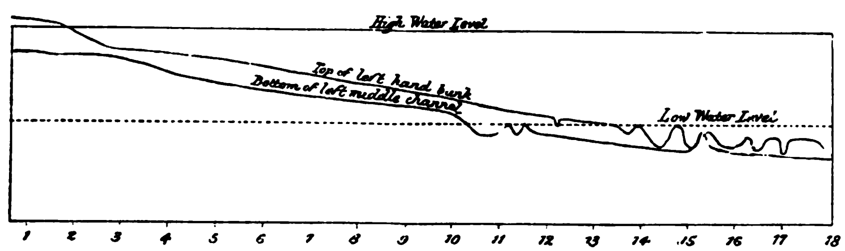
Fig. 7.

Tank A, Experiment V, Plan I. After 12697 tides at 53·4 Secs & 3589 at 49·3 Secs



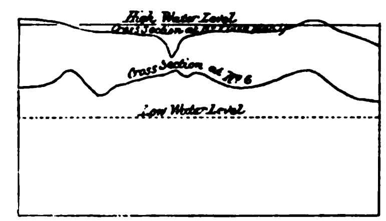
The figures on the contour lines show their actual distances in the model above or below low-water level, in decimals of a foot.

Tank A. Experiment V, Longitudinal Section I.



Horizontal Scale = 1/36 (of Model = 1/11758 of an Estuary having a 30 ft tide)

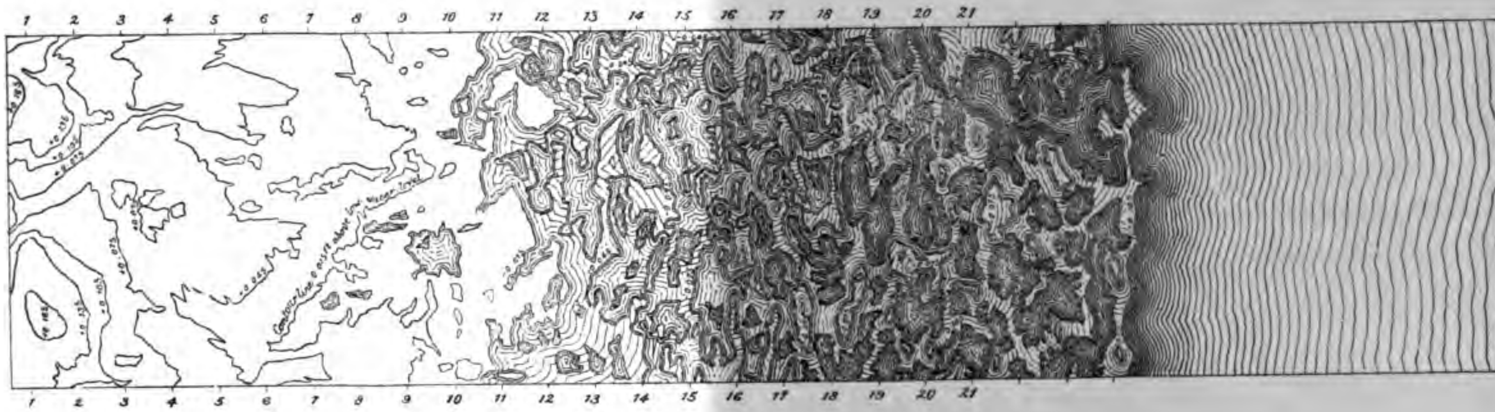
Tank A Experiment V, Cross Section I.



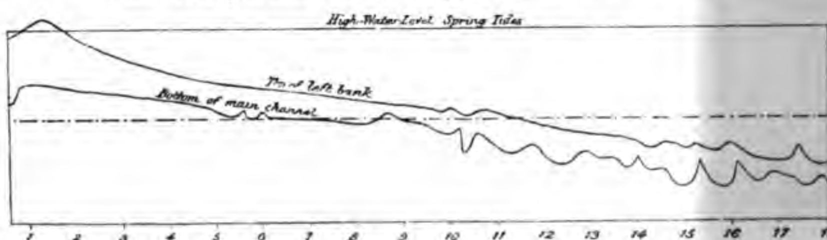
Vertical Scale = 1/6 (of Model = 1/110 of an Estuary having a 30 ft tide.

Fig. 8.

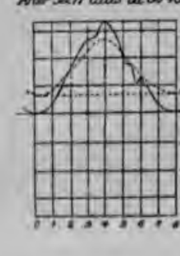
Tank A, Experiment XIII, Plan III, After 51240 tides at 50·45 Secs



Tank A, Experiment XIII, Longitudinal Section III.



Tide Curves
After 51211 tides at 50·45 Secs

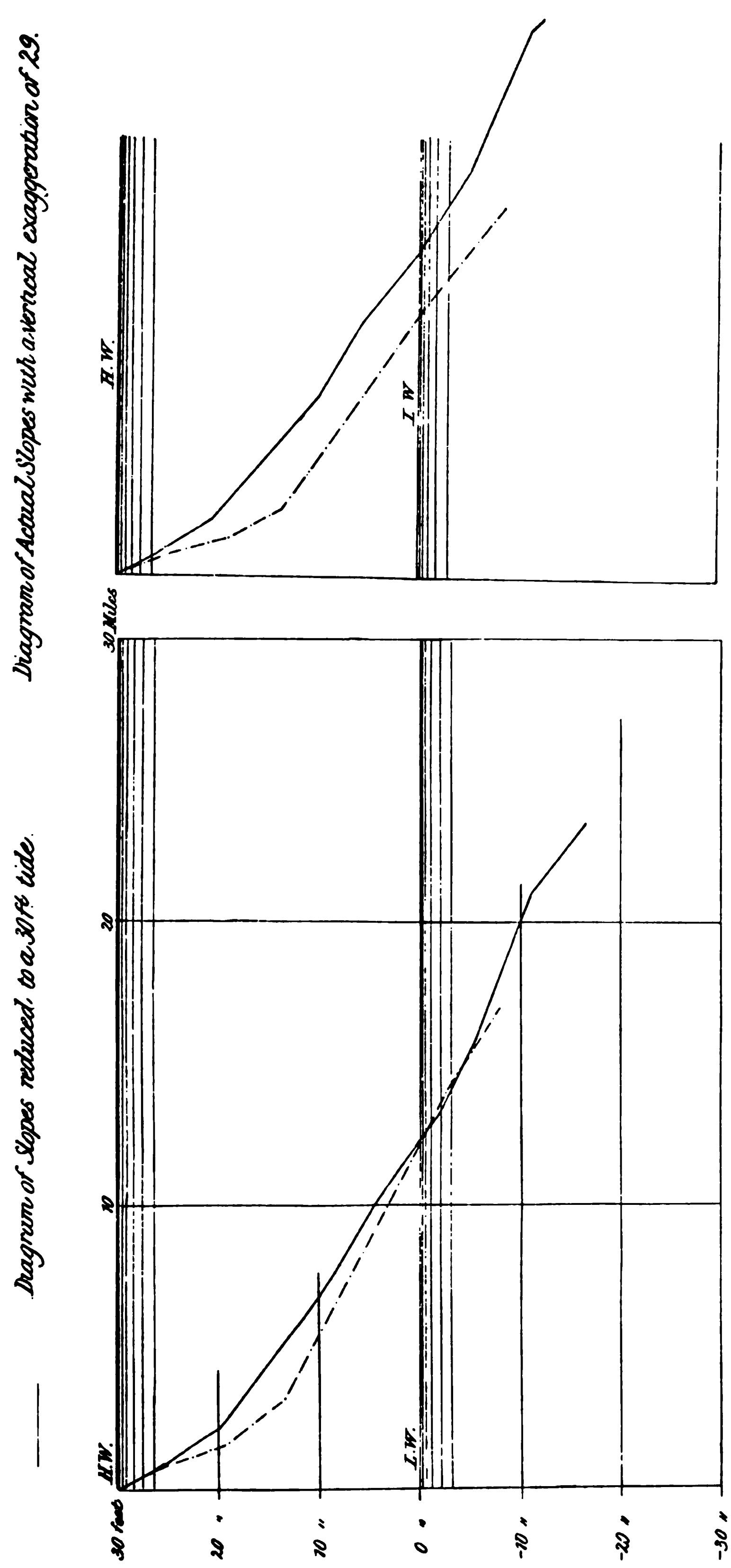


Tank A, Experiment XIII Cross Section III

Horizontal Scale - 1/36 (of Model = 1/12475 of an Estuary having a 30 ft tide)

Vertical Scale - 1/8 (of Model = 1/182 of an Estuary having a 30 ft tide)

Fig. 9.



Tank A, Experiment V, Plan I ——————
" " " XIII, " III ·—·—·—

Fig. 10.

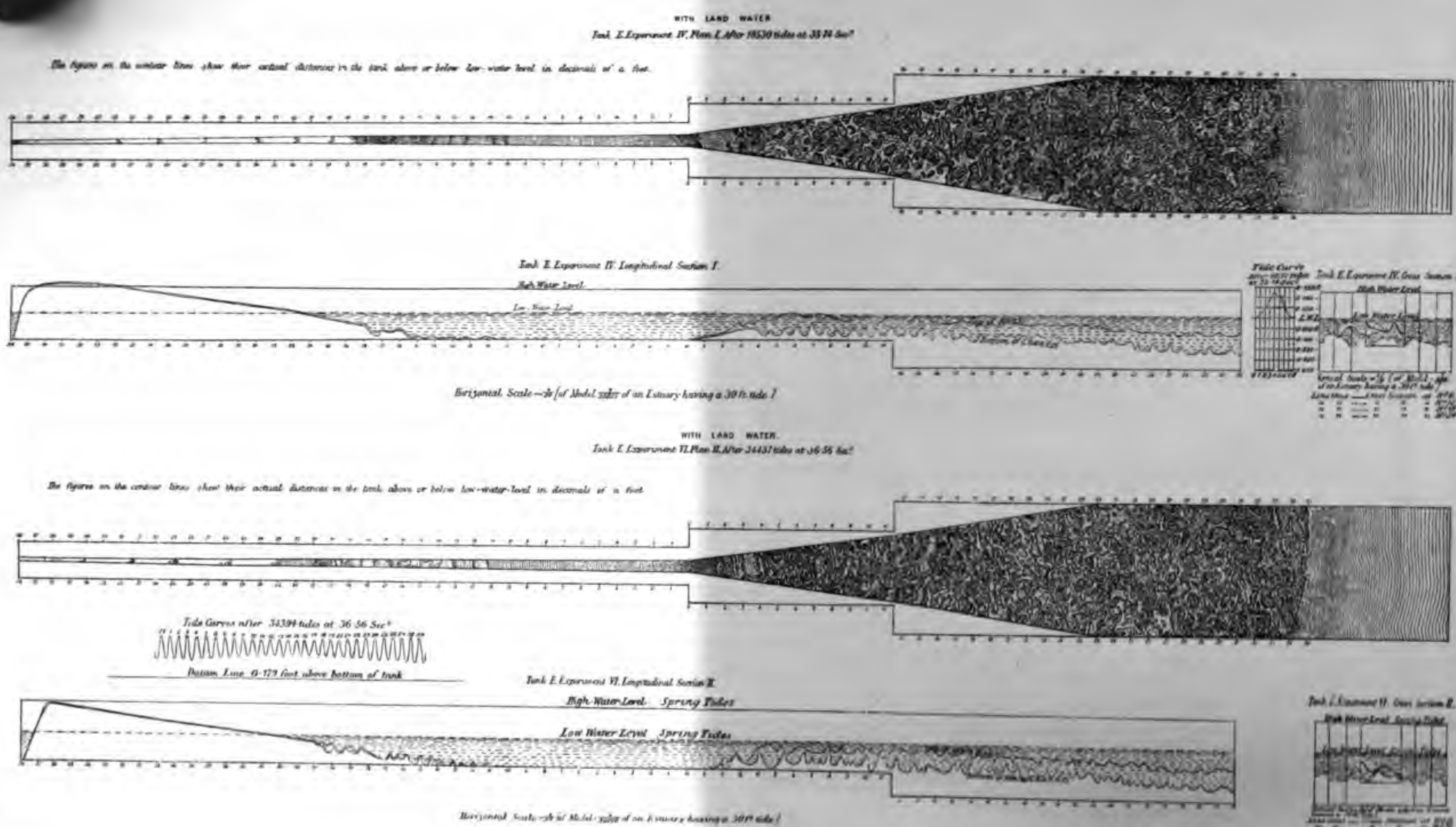

WITH LAND WATER
Tank E. Experiment IV. Plan I. After 19530 tides at 35·14 Sec.
The figures on the contour lines show their actual distances in the tank above or below low-water level in decimals of a foot.
Tank E. Experiment IV. Longitudinal Section I.
High Water Level
Low Water Level
Horizontal Scale
WITH LAND WATER.
Tank E. Experiment VI. Plan II. After 34431 tides at 36·56 Sec.
The figures on the contour lines show their actual distances in the tank above or below low-water-level in decimals of a foot.
Tide Curves after 34394 tides at 36·56 Sec.
Datum Line 0·179 feet above bottom of tank
Tank E. Experiment VI. Longitudinal Section II.
High-Water-Level Spring Tides
Low Water Level Spring Tides
Horizontal Scale

Fig. 11.

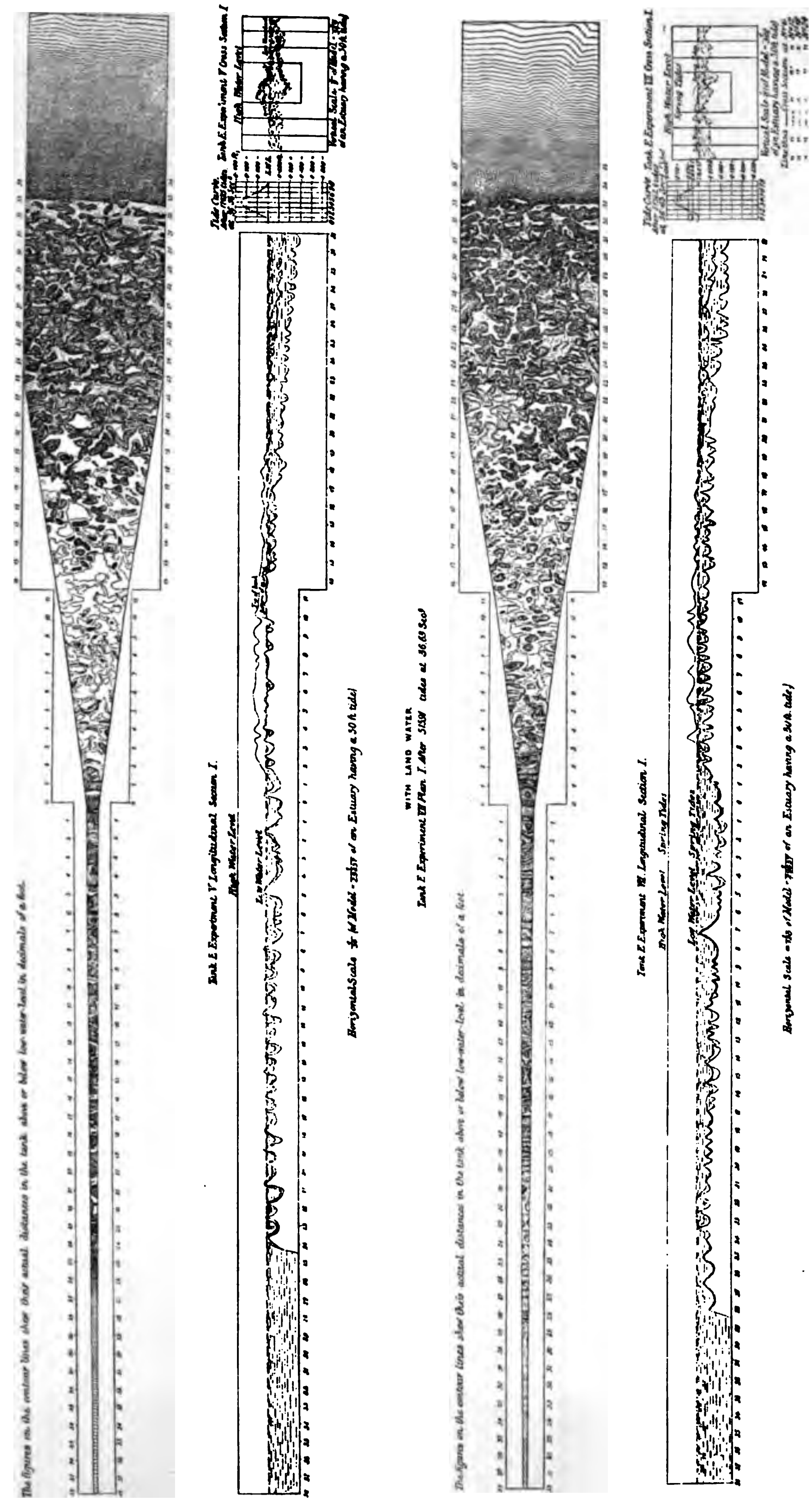

The figures on the contour lines show their actual distances in the tank above or below low-water-level in decimals of a foot.
Tank E Experiment V Longitudinal Section I.
High Water Level
Low Water Level
WITH LAND WATER
Tank E Experiment VI Plan I After 5138 tides at 36.83 secs
The figures on the contour lines show their actual distances in the tank above or below low-water-level in decimals of a foot.
Tank E Experiment VI Longitudinal Section I.
High Water Level
Spring Tides
Low Water Level

Fig. 12.

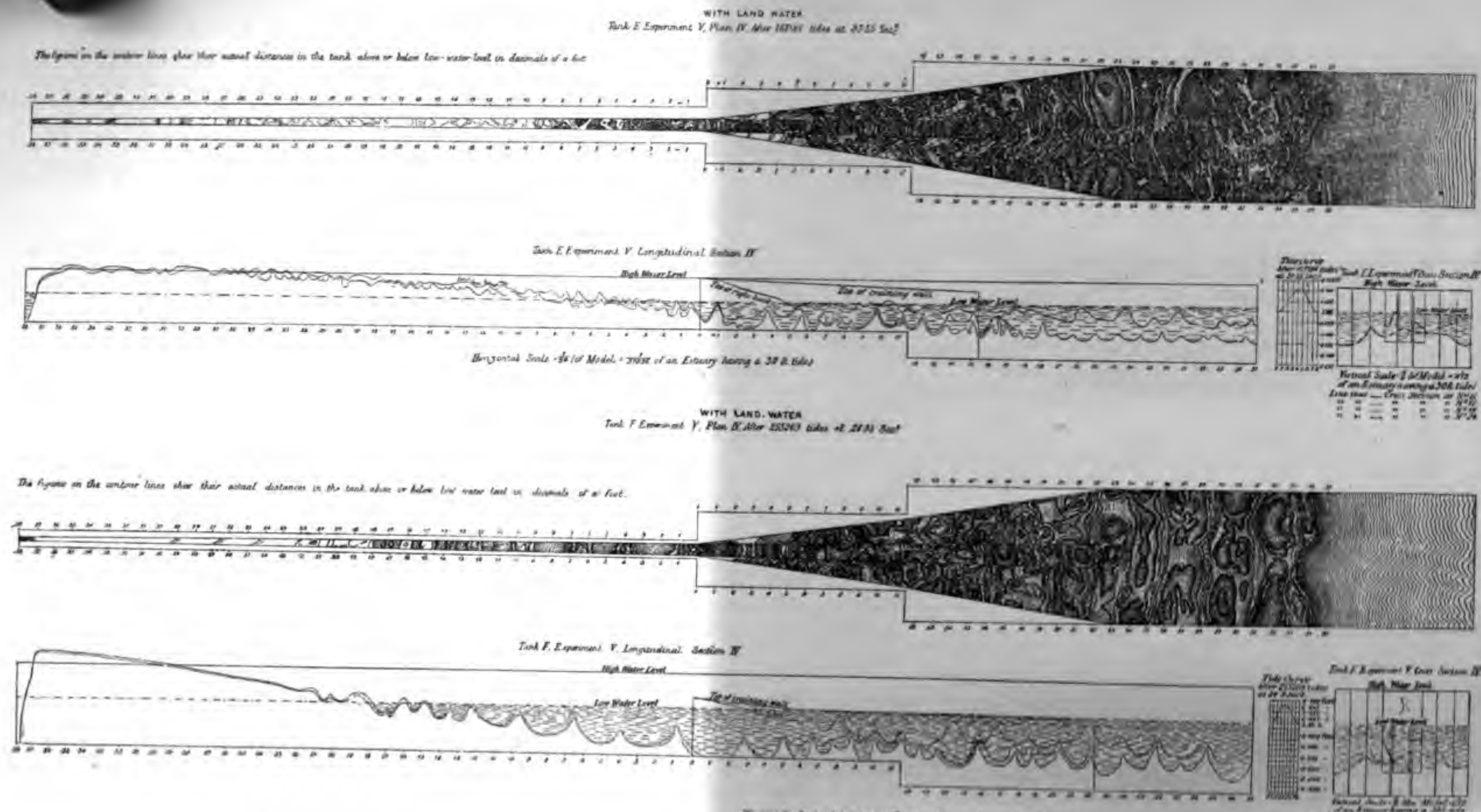

WITH LAND WATER
Tank E Experiment V, Plan IV
Tank E Experiment V Longitudinal Section IV
High Water Level
Low Water Level
WITH LAND WATER
Tank F Experiment V, Plan IV
Tank F. Experiment V. Longitudinal Section IV
High Water Level
Low Water Level

Fig. 18.

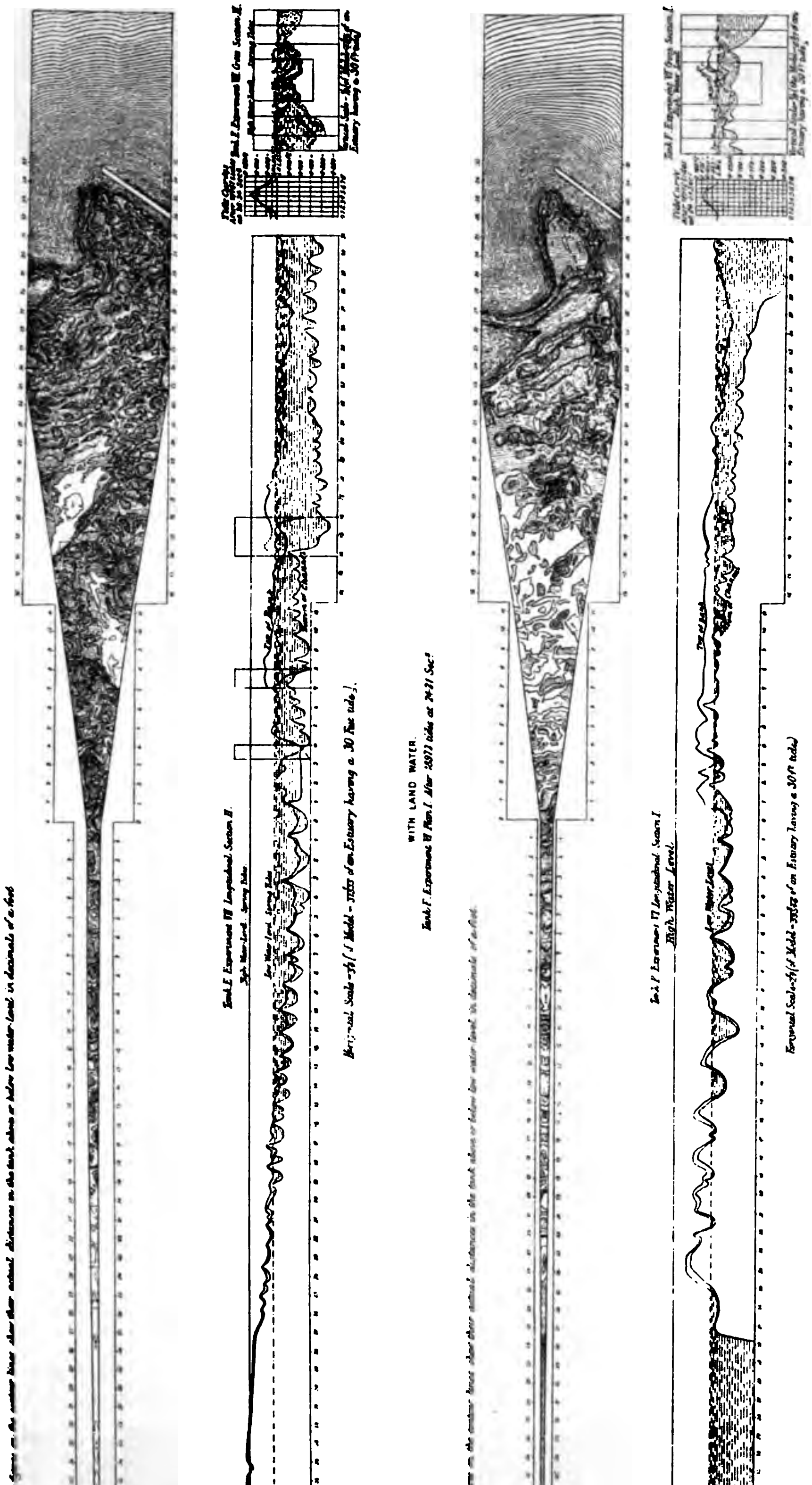


Fig. 14.

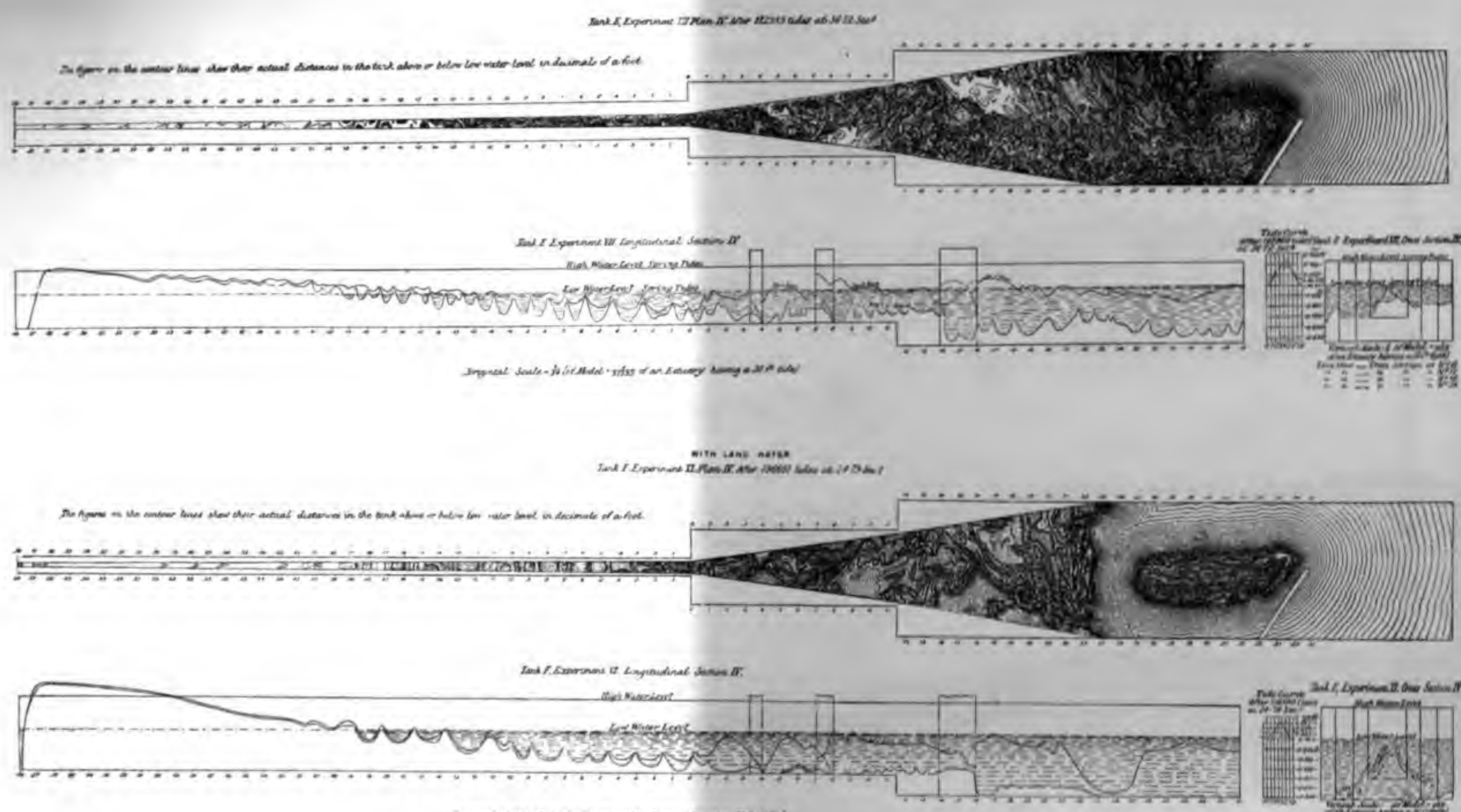


Fig. 15.

60.

ON TWO HARMONIC ANALYZERS.

[*From the Fourth Volume of the Fourth Series of* "Memoirs and Proceedings of the Manchester Literary and Philosophical Society." *Session* 1890—91.]

(*Received* April 2nd, 1891.)

THE object of these instruments is to afford a ready means of ascertaining the *periods* of *free vibration* of structures or members of structures. If any portion of a material structure (*i.e.*, an elastic structure) is disturbed from its normal position of equilibrium and suddenly released, the structure is thrown into a complex state of vibration, which gradually subsides. While the vibration lasts each point in the structure goes through movements which may be very complex, but which are, nevertheless, compounded of simple periodic or harmonic movements, each simple movement taking place in a definite direction, as well as having a definite period.

The art of measuring and recording the complex movements at a point of the earth during an earthquake has long been a study, and the seismometer of Professor Ewing has been applied to record the movements of points of various structures when subjected to disturbances. The principle of these seismometers consists in attaching a weight to the point of the structure to be examined, by attachments of such slight elasticity, that the disturbances communicated to the weight are insensibly small, and the weight remains sensibly steady amid the surrounding vibrations, and forms a steady observatory from which the vibrations may be measured. This measurement is effected by causing pencils vibrating with the structure to describe lines on cards attached to the steady weight, or *vice versa*, the cards being fixed, or having a time movement. In this way the complex motions of the points are beautifully recorded, as in Prof. Ewing's experiments on the Tay Bridge, and Prof. J. Milne's numerous experiments in railway carriages, &c.

Such curves represent the complex movements of the point of the structure examined; and any analysis of the motion into its simple periodic components remains to be accomplished by mathematical reduction—or by such instrumental synthesis as that which may be effected in Sir William Thomson's "Harmonic Analyzer."

The Harmonic Analyzers about to be described differ essentially from the seismometer in that they do not measure or record the actual motions of the structure, while they single out and exaggerate any component periodic motion according to its *period* and direction, which are defined in the instruments. The principle of these Harmonic Analyzers is that of the accumulation of motion which takes place, when a weight is subject to a periodic disturbance which coincides in period and direction with that of free vibration of which the weight is susceptible.

If a small weight w be elastically attached to a much heavier weight so that it requires a definite force (El) to disturb the weight (w) through a distance l, the large weight remaining at rest; then, if released after any disturbance, the small weight w will vibrate in the direction of disturbance, and with a constant period of:

$$2\pi\sqrt{\frac{w}{gE}} \text{ seconds,}$$

i.e. in the period of free vibration of the small weight.

If the small weight be at rest and the large weight be subject to a periodic disturbance having a period $1/n$; then, if this period is larger than the period of free vibration of the small weight, *i.e.*, if

$$\frac{1}{n} \text{ is smaller than } 2\pi\sqrt{\frac{w}{gE}},$$

the small weight will follow essentially the movements of the larger weight as if rigidly attached, while if the period of motion of the larger weight is smaller than that of the period of free vibration of the small weight, the small weight will remain virtually at rest. But when the period of motion of the large weight coincides with the period of free vibration of the small weight, the small weight will take and accumulate the disturbance, oscillating with increasing amplitude until it reaches such an extent that the energy dissipated is equal to that received from the disturbance. If the elasticity of the connections be fairly perfect, the amplitude of the small weight will be very considerable, although the disturbing motion is otherwise insensible.

If the small weight (w) has only one degree of freedom, *i.e.*, if the elasticity of the connections is not equal in all directions, there will be three axes of elasticity, and if the elasticities along two of these directions are much greater than the third this is the direction of freedom; then, when the period of free vibration along the third axis, *i.e.*, in the direction of freedom, coincides with the period of disturbance, the small weight will only take up the disturbance when this has a component in the direction of freedom; that is, if the direction of the disturbance is at right angles to the direction of freedom, there will be no vibration. So that in this way the direction of the disturbance may be ascertained, or *vice versa*.

Similar results follow if, instead of the disturbance coming through the elastic supports, the body be subject to a synchronous periodic force. If the period of the force were not synchronous with any of the three periods of free vibration corresponding respectively to the three axes of elasticity, the resulting vibration would, as before, merely correspond with the time effect of the force, but on coincidences with any one of these, unless the direction of the disturbance were at right angles to that of the axis of elasticity, the body would accumulate the disturbance.

It thus appears that, if a structure is in a state of vibration, the periods of free vibration and their directions may be ascertained by an Harmonic Analyzer consisting of a small weight with elastic attachments, so adjustable that the period of free vibration of the weight can be varied to any required extent, and the direction of such free vibration turned through all requisite angles.

This may be accomplished in many ways. That which I have so far adopted with satisfactory success has been very simple.

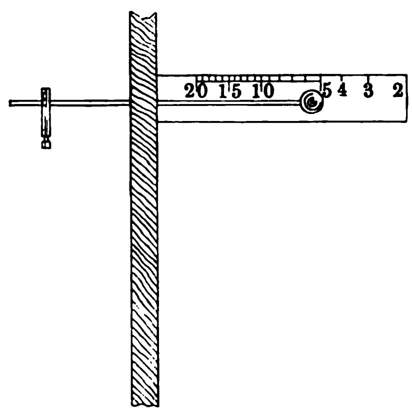


Fig. 1.

It consists, as shown in Fig. 1, essentially of a base formed of a bar of hard wood, one-and-a-half inches square, and two feet long, a cross notch being cut in one end to enable this end to be held against any point of the structure with less chance of slipping. About four inches from the notched end, right across the axis of this bar, is a hole, in which is fitted, with moderate tightness, a piece of straight steel wire, one-eighth of an inch in diameter, and 18 inches long. On one end of the wire is a ball of lead, about 2 oz., through the centre of which is a small hole at right angles to the wire, in which is fixed a small graphite pencil. On the other end of the wire is a carrier, to afford handhold for the purpose of adjusting the wire in the hole.

When the carrier is pushed right up to the wood, the ball, if disturbed, will vibrate in any direction perpendicular to the wire so as to make about 200 oscillations a minute, which is slower than any period it is required to measure. As the carrier is pulled back, and the wire between the base and the ball shortened, the rate of vibration increases, until, when the wire is only $1\frac{1}{2}$ inches long, the ball, when disturbed, gives out an audible note of about 2,000 vibrations a minute.

The instrument is used by holding in one hand the longer end of the wood, and pressing the notched end hard against the point of the structure of which the motion is to be analyzed, the carrier having previously been pushed up to the wood, then, with the free hand, the carrier is pulled steadily back, the ball being carefully watched. As by the shortening of the wire between the base and the ball the free period of vibration of the ball is diminished, and comes near to any period amongst the vibrations in the structure, the ball is seen to take up the vibration in beats with intervals of rest; and a very little more careful adjustment is sufficient to bring the period into coincidence, when the ball continues vibrating with the structure, having the appearance in Fig. 2

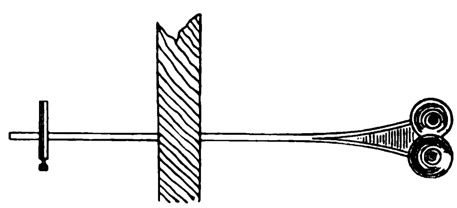


Fig. 2.

The period of the Analyzer having been thus adjusted to that of one of the periods of free vibration of the structure, the period is ascertained either by adjusting the Analyzer so that the pencil in the ball may oscillate in contact with the paper on a chronograph, or by measuring the distance of the ball from the wood on a scale, previously adjusted by aid of the chronograph to give the number of vibrations per minute.

Extreme accuracy of determining the periods has not so far been an important consideration. The readings on the chronograph were only taken to about 10°/₀. But that the Analyzer is susceptible of much greater accuracy is shown by the fact that several different adjustments to the same period in the structure brought the wire into exactly the same position.

Its power of analyzing complex vibrations is so far unqualified. It was invented for the purpose of determining the period of a particular vibration—in a very stiff iron structure subject to the periodic disturbance of the belts from two engines running at high speed, and the centrifugal action of such want of balance as there might be in heavy pulleys, three feet in diameter, and running at 500 revolutions per minute. The vibration was very slight—nothing more than a slight tremor could be felt with the hand. The periodic disturbances were about 500 per minute, and these came out clearly, but small, in the Analyzer when adjusted to these periods—but the periods of free vibration of one of the members, 720 per minute, caused an amplitude of half an inch in the ball, and that of another, 1,270, was easily identified.

The instrument already described can clearly only be used on a structure while it is so disturbed as to set its members vibrating. Such disturbances can generally be set up by a shock of some sort, but when it is necessary to cause artificial disturbance, it is better to adopt a periodic disturbance of such varying period as will come gradually into coincidence with the periods of free vibration, bringing these vibrations out separately, when they will be readily identified with the Analyzer, if not otherwise perceptible.

For this purpose, in 1887, I adopted the following method:—A small cast-iron pulley, 6 inches in diameter, very much out of balance, was mounted on a small frame that could be clipped on to any part of the structure, and a cord passed over this pulley on to a larger wheel, which was turned by hand. In this way the unbalanced wheel was driven at a gradually increasing rate until steady vibrations in the structure were observed, then these coincided with the period of the unbalanced wheel, and this was ascertained to be about 1,200 by counting the revolutions of this hand-wheel. At this speed the disturbing force resulting from the unbalanced weight, 2 lbs. on a radius of 2 inches, would be 40 lbs. The structure thus under examination was an iron standard, very stiff. A theodolite was adjusted, with the cross wires on a mark on the top of the standard, which, when the period of the small unbalanced wheel coincided with that of free vibration, was seen to move as much as one-twentieth of an inch. Chains were then attached to the top of the standard, and by means of blocks, a horizontal force of a ton was thrown on to the top of the standard, when it did not yield more than two-hundredths of an inch. So that the deviation caused by the periodic force of 40 lbs., in such coincidence with the period of free vibration as could be attained with the hand-wheel, was three times as great as that which resulted from a direct statical force of one ton.

61.

STUDY OF FLUID MOTION BY MEANS OF COLOURED BANDS.

[*From the* "Proceedings of the Royal Institution of Great Britain."]

(*Read* June 2, 1893.)

In his charming story of *The Purloined Letter*, Edgar Allan Poe tells how all the efforts and artifices of the Paris police to obtain possession of a certain letter, known to be in a particular room, were completely baffled for months by the simple plan of leaving the letter in an unsealed envelope in a letter-rack, and so destroying all *curiosity* as to its contents; and how the letter was at last found there by a young man who was not a professional member of the force. Closely analogous to this is the story I have to set before you to-night—how certain mysteries of fluid motion, which have resisted all attempts to penetrate them are at last explained by the simplest means and in the most obvious manner.

This indeed is no new story in science. The method adopted by the minister, *D*, to secrete his letter, appears to be the favourite of Nature in keeping her secrets, and the history of science teems with instances in which keys, after being long sought amongst the grander phenomena, have been found at last not hidden with care, but scattered about, almost openly, in the most commonplace incidents of every-day life which have excited no curiosity.

This was the case in physical astronomy—to which I shall return after having reminded you that the motion of matter in the universe naturally divides itself into three classes.

1. The motion of bodies as a whole—as a grand illustration of which we have the heavenly bodies, or more humble, but not less effective, the motion of a pendulum or a falling body.

2. The relative motion of the different parts of the same fluid or elastic body—for the illustration of which we may go to the grand phenomena presented by the tide, the whirlwind, or the transmission of sound, but which is equally well illustrated by the oscillatory motion of the wave, as shown by the motion of its surface, and by the motion of this jelly, which, although the most homely illustration, affords by far the best illustration of the properties of an elastic solid.

3. The inter-motions of a number of bodies amongst each other—to which class belong the motions of the molecules of matter resulting from heat, as the motions of the molecules of a gas, in illustration of which I may mention the motions of individuals in a crowd, and illustrate by the motion of the grains in this bottle when it is shaken, during which the white grains at the top gradually mingle with the black ones at the bottom —which interdiffusion takes an important part in the method of coloured bands.

Now of these three classes of motion, that of the individual body is incomparably the simplest. Yet, as presented in the phenomena of the heavens, which have ever excited the greatest curiosity of mankind, it defied the attempts of all philosophers for thousands of years, until Galileo discovered the laws of motion of mundane matter. It was not until he had done this, and applied these laws to the heavenly bodies, that their motions received a rational explanation. Then Newton, taking up Galileo's parable and completing it, found that its strict application to the heavenly bodies revealed the law of gravitation, and developed the theory of dynamics.

Next to the motions of the heavenly bodies, the wave, the whirlwinds, and the motions of clouds, had excited the philosophical curiosity of mankind from the earliest time. Both Galileo and Newton, as well as their followers, attempted to explain these by the laws of motion, but although the results so obtained have been of the utmost importance in the development of the theory of dynamics, it was not till this century that any considerable advance was made in the application of this theory to the explanation of fluid phenomena, and although during the last fifty years splendid work has been done, work which, in respect of the mental effort involved, or the scientific importance of the results, goes beyond that which resulted in the discovery of Neptune, yet the circumstances of fluid motion are so obscure and complex, that the theory has yet been interpreted only in the simplest cases.

To illustrate the difference between the interpretation of the theory of the heavenly bodies and that of fluid motion, I would call your attention to the fact that solid bodies, on the behaviour of which the theory of the motion of the planets is founded, move as one piece, so that their motion is exactly represented by the motion of their surfaces; that they are not subject to any internal disorder which may affect their general motion. So surely is this the case, that even those who have never heard of dynamics can predict with certainty how any ordinary body will behave under any ordinary circumstances, and so much so that any departure is a matter of surprise. Thus I have here a cube of wood, to one side of which a string is attached. Now hold it on one side, and holding the string you naturally suppose that when I let go it will turn down so as to hang with the string vertical; it does not do so, that is a matter of surprise; I place it on the other side and it still remains as I place it. If I swing it as a pendulum it does not behave like one.

Would Galileo have discovered the laws of motion had his pendulum behaved like this? Why is its motion peculiar? There is internal motion. Of what sort? Well, I think my illustration may carry more weight if I do not tell you; you can all, I have no doubt, form a good idea. It is not fluid motion or I should feel bound to explain it. You have here an ordinary looking object which behaves in an extraordinary manner, which is yet very decided and clear, to judge by the motion of its surface, and from the manner of the motion I wish you to judge of the cause of the observed motion*.

This is the problem presented by fluids, in which there may be internal motion which has to be taken into account before the motion of the surface can be explained. You can see no more of what the motion is within a homogeneous fluid, however opaque or clear, than you can see what is going on within the box. Thus, without colour bands the only visual clue to what is going on within the fluids is the motion of their bounding surfaces. Nor is this all; in most cases the surfaces which bound the fluid are immovable.

In the case of the wave on water the motion of the surface shows that there is motion, but because the surface shows no wave it does not do to infer that the fluid is at rest.

The only surfaces of the air within this room are the surfaces of the floor, walls, and objects within it. By moving the objects we move the air, but how far the air is at rest you cannot tell unless it is something familiar to you.

Now I will ask you to look at these balloons. They are familiar objects enough, and yet they are most sensitive anemometers, more sensitive than anything else in the room; but even they do not show any motion; each of them forms an internal bounding surface of the air. I send an *aerial*

* In this experiment a cubical box of wood, apparently a solid block, contained a heavy spinning top.

messenger to them, and a small but energetic motion is seen by which it acknowledges the message, and the same message travels through the rest, as if a *ghost* touched them. It is a wave that moves them. You do not feel it, and, but for the surfaces of the air formed by the balloons, would have no notion of its existence*.

In this tank of beautifully clear distilled water, I project a heavy ball in from the end, and it shows the existence of the water by stopping almost dead within two feet. The fact that it is stopped by the water, being familiar, does not raise the question, Why does it stop?—a question to which, even at the present day, a complete answer is not forthcoming. The question is, however, suggested, and forcibly suggested, when it appears that with no greater or other evidence of its existence, I can project a disturbance through the water which will drive this small disc the whole length of the tank.

I have now shown instances of fluid motion of which the manner is in no way evident without colour bands, and were revealed by colour bands, as I showed in this room sixteen years ago. At that time I was occupied in setting before you the manners of motion revealed, and I could only incidentally notice the means by which this revelation was accomplished.

Amongst the ordinary phenomena of motion there are many which render evident the internal motion of fluids. Small objects suspended in the fluid are important, and that their importance has long been recognised is shown by the proverb—straws show which way the wind blows. Bubbles in water, smoke and clouds, afford the most striking phenomena, and it is doubtless these that have furnished philosophers with such clues as they have had. But the indications furnished by these phenomena are imperfect, and, what is more important, they only occur casually, and in general only under circumstances of such extreme complexity that any deduction as to the elementary motions involved is impossible. They afford indication of commotion, and perhaps of the general direction in which the commotion is tending, but this is about all.

For example, the different types of clouds; these have always been noticed and are all named. And it is certain that each type of clouds is an indication of a particular type of motion in the air; but no deductions as to what definite manner of motion is indicated by each type of cloud have ever been published.

Before this can be done it is necessary to reverse the problem, and find to what particular type of cloud a particular manner of motion would give

* By means of a large box, having a hinged door on one side, and a circular aperture on the side opposite, invisible vortex rings of air were projected towards the balloons.

rise. Now a cloud, as we see it, does not directly indicate the internal motion of which it is the result. As we look at clouds, it is not in general their motion that we notice, but their figure. It is hard to see that this figure changes while we are watching a cloud, though such a change is continually going on, but is apparently very slow on account of the great distance of the cloud and its great size. However, types of clouds are determined by their figure, not by their motion. Now what their figure shows is not motion, but is the history or result of the motion of particular strata of the air in and through surrounding strata. Hence, to interpret the figures of the clouds we must study the changes in shape of fluid masses, surrounded by fluid, which result from particular motions.

The ideal in the method of colour bands is to render streaks or lines in definite position in the fluid visible, without in any way otherwise interfering with these properties as part of the homogeneous fluid. If we could by a wish create coloured lines in the water, these would be ideal colour bands. We cannot do this, nor can we exactly paint lines in the air or water.

I take this ladle full of highly coloured water, lower it slowly into the surface of the surrounding water till that within is level with that without; then turn the ladle carefully round the coloured water; the mass of coloured water will remain where placed.

I distribute the colour slowly. It does not mix with the clear water, and although the lines are irregular they stand out very beautifully. Their edges are sharp here. But in this large sphere, which was coloured before the lecture, although the coloured lines have generally kept their places, they have, as it were, swollen out and become merged in the surrounding water in consequence of molecular motion. The sphere shows, however, one of the rarest phenomena in Nature—the internal state in almost absolute internal rest. The forms resemble nothing so much as stratus clouds, as seen on a summer day, though the continuity of the colour bands is more marked. A mass of coloured water once introduced is never broken. The discontinuity of clouds is thus seen to be due to other causes than mere motion.

Now, having called your attention to the rarity of water at rest, I will call your attention to what is apt to be a very striking phenomenon, namely, that when water is contained, like this, in a spherical vessel of which you cannot alter the shape, it is impossible by moving the vessel suddenly to set up relative motion in the interior of the water. I may swing this vessel about and turn it, but the colour band in the middle remains as it was, and when I stop shows the water to be at rest.

This is not so if the water has a free surface, or if the fluid is of unequal density. Then a motion of the vessel sets up waves, and the colour band shows at once the beautifully lawful character of the internal motion. The colour bands move backwards and forwards, showing how the water is distorted like a jelly, and as the wave dies out the colour bands remain as they were to begin with.

This illustrates one of the two classes of internal motion of water or fluid. Wherever fluid is not in contact with surfaces over which it has to glide, or surfaces which fold on themselves, the internal motions are of this purely wave character. The colour bands, however much they may be distorted, cannot be relatively displaced, twisted, or curled up, and in this case motion in water once set up continues almost without resistance. That wave motion, in water with a free surface, is one of the most difficult things to stop, is directly connected with the difficulty of setting still water in motion; in either case the influence must come through the surfaces. Thus it is that waves once set up will traverse thousands of miles, establishing communication between the shores of Europe and America. Wave motion in water is subject to enormously less resistance than any other form of material motion.

In wave motion, if the colour bands are across the wave they show the motion of the water; nevertheless, their chief indication is of the change of shape while the fluid is in motion.

This is illustrated in this long bottle, with the coloured water less heavy than the clear water. If I lay it down in order to establish equilibrium, the blue water has to leave the upper end of the bottle and spread itself over the clear water, while the clear water runs under the coloured. This sets up wave motion, which continues after the bottle has come to rest. But as the colour bands are parallel with the direction of motion of the waves, the motion only becomes evident in thickening and bending of the colour bands.

The waves are entirely between the two fluids, there being no motion in the outer surfaces of the bottle, which is everywhere glass. They are owing to the slight differences in the density of the fluids, as is indicated by the extreme slowness of the motion. Of such kind are the waves in the air, that cause the clouds which make the mackerel sky, the vapour in the tops of the waves being condensed and evaporated again as it descends, showing the results of the motion.

The distortional motions, such as alone occur in simple wave motion, or where the surfaces of the fluid do not fold in on themselves, or wind in, are the same as occur in any homogeneous continuous material which completely fills the space between the surfaces.

If plastic material is homogeneous in colour, it shows nothing as to the internal motion; but if I take a lump built of plates, blue and white, say a square, then I can change the surfaces to any shape without folding or turning the lump, and the coloured bands which extend throughout the lump show the internal changes. Now the first point to illustrate is that, however I change its shape, if I bring it back to the original shape the colour bands will all come back to their original positions, and there is no limit to the extent of the change that may thus be effected. I may roll this out to any length, or draw it out, and the diminution in thickness of the colour bands shows the extent of the distortion. This is the first and simplest class of motion to which fluids are susceptible. By this motion alone elements of the fluid may be, and are, drawn out to an indefinitely fine line, or spread out in an indefinitely thin sheet, but they will remain of the same general figure.

By reversing the process they change back again to the original form. No colour band can ever be broken, even if the outer surface be punched in till the punch head comes down on the table; still all the colour bands are continuous under the punch, and there is no folding or lapping of the colour bands unless the external surface is folded.

The general idea of mixture is so familiar to us that the vast generalization to which these ideas afford the key, remains unnoticed. That continued mixing results in uniformity, and that uniformity is only to be obtained by mixing, will be generally acknowledged, but how deeply and universally this enters into all the arts can but rarely have been apprehended. Does it ever occur to any one that the beautiful uniformity of our textile fabrics has only been obtained by the development of processes of mixing the fibres? Or, again, the uniformity in our construction of metals; has it ever occurred to any one that the inventions of Arkwright and Cort were but the application of the long-known processes by which mixing is effected in culinary operations? Arkwright applied the draw-rollers to uniformly extend the length of the cotton sliver at the expense of the thickness; Cort applied the rolling-mill to extend the length of the iron bloom at the expense of its breadth; but who invented the rolling-pin by which the pastry-cook extends the length at the expense of the thickness of the dough for the pie-crust?

In all these processes the object, too, is the same throughout—to obtain some particular shape, but chiefly to obtain a uniform texture. To obtain this nicety of texture it is necessary to mix up the material, and to accomplish this it is necessary to attenuate the material, so that the different parts may be brought together.

The readiness with which the fluids are mixed and uniformity obtained is a by-word; but it is only when we come to see the colour bands that we realize that the process by which this is attained is essentially the same as that so laboriously discovered for the arts—as depending first on the attenuation of each element of the fluid—as I have illustrated by distortion.

In fluids, no less than in cooking, spinning, and rolling—this attenuation is only the first step in the process of mixing—all involve the second process, that of folding, piling, or wrapping, by which the attenuated layers are brought together. This does not occur in the pure wave motion of water, and constitutes the second of the two classes of motion. If a wave on water is driven beyond a certain height it leaps or breaks, folding in its surface. Or, if I but move a solid surface through the water it introduces tangential motion, which enables the fluid to wind its elements round an axis. In these ways, and only in these ways, we are released from the restriction of not turning or lapping. And in our illustration, we may fold up our dough, or lap it—roll it out again and lap it again; cut up our iron bar, pile it, and roll it out again, or bring as many as we please of the attenuated fibres of cotton together to be further drawn. It may be thought that this attenuation and wrapping will never make perfect admixture, for, however thin, each element will preserve its characteristic, the coloured layers will be there, however often I double and roll out the dough. This is true. But in the case of some fluids, and only in the case of some fluids, the physical process of diffusion completes the admixture. These colour bands have remained in this water, swelling but still distinct; this shows the slowness of diffusion. Yet such is the facility with which the fluid will go through the process of attenuating its elements and enfolding them, that by simply stirring with a spoon these colour bands can be drawn and folded so fine that the diffusion will be instantaneous, and the fluid become uniformly tinted. All internal fluid motion other than simple distortion, as in wave motion, is a process of mixing, and it is thus from the arts that we get the clue to the elementary forms and processes of fluid motion.

When I put the spoon in and mixed the fluid you could not see what went on—it was too quick. To make this clear, it is necessary that the motion should be very slow. The motion should also be in planes at right angles to the direction in which you are looking. Such is the instability of fluid that to accomplish this at first appeared to be difficult. At last, however, as the result of much thought, I found a simple process which I will now show you, in what I think is a novel experiment, and you will see, what I think has never been seen before by any one but Mr Foster and myself, namely, the complete process of the formation of a cylindrical vortex sheet resulting from the motion of a solid surface. To make it visible to all I am obliged to limit the colour band to one section of the sheet, otherwise only those immediately in front would be able to see between the convolutions of the spiral. But you will understand that what is seen is a section, a similar state of motion extending right across the tank. From the surface you see the plane vane extending half-way down right across the tank; this is attached to a float.

Out of the tube I now institute a colour band on the right of the vane. There is no motion in the water, and the colour descends slowly from the tube. I now give a small impulse to the float to move it to the right, and at once the spiral form is seen from the tube. Similar spirals would be formed all across the tank if there were colours. The float has moved out of the way, leaving the revolving spiral with its centre stationary, showing that the horizontal axis of the spiral is half-way between the bottom and surface of the tank, in which the water is now simply revolving round this axis.

This is the vortex in its simplest and rarest form (for a vortex cannot exist with its ends exposed). Like an army it must have its flanks protected; hence a straight vortex can only exist where it has two surfaces to cover its flanks, and parallel vertical surfaces are not common in nature. The vortex can bend, and, as with a horse-shoe axis, can rest both its flanks on the same surface, as this piece of clay, or with a ring axis, which is its commonest form, as in the smoke ring. In both these cases the vortex will be in motion through the fluid, and less easy to observe.

These vortices have no motion beyond the rotation because they are half-way down the tank. If the vane were shorter they would follow the vane; if it were longer they would leave it.

In the same way, if instead of one vortex there were two vortices, with their axis parallel, extending right across, the one above another, they would move together along the tank.

I replace the float by another which has a vane suspended from it, so that the water can pass both above and below the vane extending right across the middle portion of the tank. In this case I institute two colour bands, one to pass over the top, the other underneath, the vane, which colour bands will render visible a section of each vortex just as in the last case. I now set the float in motion and the two vortices turn towards each other in opposite directions. They are formed by the water moving over the surface of the vane, downwards to get under it, upwards to get over it, so that the rotation in the upper vortex is opposite to that in the lower. All this is just the same as before, but that instead of these vortices standing still as before they follow at a definite distance from the vane, which continues its motion along the tank without resistance.

Now this experiment shows, in the simplest form, the *modus operandi* by which internal waves can exist in fluid without any motion in the external boundary. Not only is this plate moving flatwise through the water, but it is followed by all the water, coloured and uncoloured, enclosed in these cylindrical vortices. Now, although there is no absolute surface visible, yet there is a definite surface which encloses these moving vortices, and separates them from the water which moves out of their way. This surface will be rendered visible in another experiment I shall show you. Thus the water which has only wave motion is bounded by a definite surface, the motion of which corresponds to the wave; but inside this closed surface there is also water, so that we cannot see the surface, and this water inside is moving round and round, but so that its motion at the bounding surface is everywhere the same as that of the outside water.

The two masses of water do not mix. That outside moves over the bounding surface, out of the way of and past the vortices, while the vortices move round and round inside the surface in such a way that they are moving in exactly the same manner at the surface as the wave surface outside.

This is the key to the internal motion of water. You cannot have a pure wave motion inside a mass of fluid with its boundaries at rest, but you have a compound motion, a wave motion outside, and a vortex within, which fulfils the condition that there shall be no sliding of the fluid over fluid at the boundary.

A means, which I hope may make the essential conditions of this motion clearer, occurred to me while preparing this lecture, and to this I will now ask your attention. I have here a number of layers of cotton-wool (wadding). Now I can force any body along between these layers of wadding. They yield, as by a wave, and let it go through; but the wadding must slide over the surface of the body so moving through it. And this it must *not* do if it illustrate the conditions of fluid motion. Now there is one way, and only one way, in which material can be got through between the sheets of wadding without slipping. It must roll through; but this is not enough, because if it rolls on the under surface it will be slipping on the upper. But if we have two rollers, one on the top of the other, between the sheets, then the lower roller rolls on the bottom sheet, the upper roller rolls against the upper sheet, so that there is no slipping between the rollers or the wadding, and, equally important, there is no slipping between the rollers, as they roll on each other. I have only to place a sheet of canvas between the rollers and draw it through; both the flannel rollers roll on the canvas and on the wadding, which they pass through without slipping, causing the wadding to move in a wave outside them, and affording a complete parallel of the vortex motion.

I will now show by colour bands some of the more striking phenomena of internal motion, as presented by Nature's favourite form of vortex, the vortex ring, which may be described as two horse-shoe vortices with their ends founded on each other.

To show the surface separating the water moving with the vortex, from that which gives way outside, I discharge from this orifice a mass of coloured water, which has a vortex ring in it formed by the surface as already described. You see the beautifully defined mass moving on slowly through the fluid, with the proper vortex ring motion, but very slow. It will not go far before a change takes place, owing to the diffusion of the vortex motion across the bounding surface; then the coloured surface will be wound into the ring which will appear. The mass approaches the disc in front. It cannot pass, but will come up and carry the disc forward; but the disc, although it does not destroy the ring, disturbs the motion.

If I send a more energetic ring it will explain the phenomenon I showed you at the beginning of this lecture; it carries the disc forward as if struck with a hammer. This blow is not simply the weight of the coloured ring, but of the whole moving mass and the wave outside. The ring cannot pass the disc without destruction, with the attendant wave.

Not only can a ring follow a disc, but as with the plane vane so with the disc, if we start a disc we must start a ring behind it.

I will now fulfil my promise to reveal the silent messenger I sent to those balloons. The messenger appears in the form of a large smoke ring, which is a vortex ring in air rendered visible by smoke instead of colour. The origination of these rings has been carefully set so that the balloons are beyond the surface which separates the moving mass of water from the wave, so that they are subject to the wave motion only. If they are within this surface they will disturb the direction of the ring, if they do not break it up.

These are, if I may say so, the phenomenal instances of internal motion of fluids. Phenomenal in their simplicity, they are of intense interest, like the pendulum, as furnishing the clue to the more complex. It is by the light we gather from their study that we can hope to interpret the parallel of the vortex wrapped up in the wave, as applied to the wind of heaven, and the grand phenomenon of the clouds, as well as those things which directly concern us, such as the resistance of ships.

62.

ON THE DYNAMICAL THEORY OF INCOMPRESSIBLE VISCOUS FLUIDS AND THE DETERMINATION OF THE CRITERION.

[*From the* "Philosophical Transactions of the Royal Society," 1895.]

(*Read* May 24, 1894.)

SECTION I.

Introduction.

1. THE equations of motion of viscous fluid (obtained by grafting on certain terms to the abstract equations of the Eulerian form, so as to adapt these equations to the case of fluids subject to stresses depending in some hypothetical manner on the rates of distortion, which equations Navier* seems to have first introduced in 1822, and which were much studied by Cauchy† and Poisson‡) were finally shown by St Venant§ and Sir Gabriel Stokes||, in 1845, to involve no other assumption than that the stresses, other than that of pressure uniform in all directions, are linear functions of the rates of distortion, with a coefficient depending on the physical state of the fluid.

By obtaining a singular solution of these equations as applied to the case of pendulums in steady periodic motion, Sir G. Stokes¶ was able to compare the theoretical results with the numerous experiments that had

* *Mém. de l'Académie*, vol. VI. p. 389.
† *Mém. des Savants Étrangers*, vol. I. p. 40.
‡ *Mém. de l'Académie*, vol. X. p. 345.
§ *B.A. Report*, 1846.
|| *Cambridge Phil. Trans.*, 1845.
¶ *Ibid.*, vol. IX. 1857.

been recorded, with the result that the theoretical calculations agreed so closely with the experimental determinations as seemingly to prove the truth of the assumption involved. This was also the result of comparing the flow of water through uniform tubes with the flow calculated from a singular solution of the equations, so long as the tubes were small and the velocities slow. On the other hand, these results, both theoretical and practical, were directly at variance with common experience as to the resistance encountered by larger bodies moving with higher velocities through water, or by water moving with greater velocities through larger tubes. This discrepancy Sir G. Stokes considered as probably resulting from eddies, which rendered the actual motion other than that to which the singular solution referred, and not as disproving the assumption.

In 1850, after Joule's discovery of the Mechanical Equivalent of Heat, Stokes showed, by transforming the equations of motion—with arbitrary stresses—so as to obtain the equations of ("Vis-viva") energy, that this equation contained a definite function, which represented the difference between the work done on the fluid by the stresses and the rate of increase of the energy, per unit of volume, which function, he concluded, must, according to Joule, represent the Vis-viva converted into heat.

This conclusion was obtained from the equations irrespective of any particular relation between the stresses and the rates of distortion. Sir G. Stokes, however, translated the function into an expression in terms of the rates of distortion, which expression has since been named by Lord Rayleigh the *Dissipation-Function*.

2. In 1883 I succeeded in proving, by means of experiments with colour bands—the results of which were communicated to the Society*—that when water is caused by pressure to flow through a uniform smooth pipe, the motion of the water is *direct*, *i.e.*, parallel to the sides of the pipe, or *sinuous*, *i.e.*, crossing and re-crossing the pipe, according as U_m, the mean velocity of the water, as measured by dividing Q, the discharge, by Δ, the area of the section of the pipe, is below or above a certain value given by

$$K\mu/D\rho,$$

where D is the diameter of the pipe, ρ the density of the water, and K a numerical constant, the value of which according to my experiments, and, as I was able to show, to all the experiments by Poiseuille and Darcy, is for pipes of circular section between

$$1900 \text{ and } 2000,$$

* *Phil. Trans.*, 1883, Part III. p. 935. (See this vol. p. 51.)

or, in other words, steady direct motion in round tubes is stable or unstable according as

$$\rho \frac{DU_m}{\mu} > 1900 \text{ or } < 2000,$$

the number K being thus a criterion of the possible maintenance of sinuous or eddying motion.

3. The experiments also showed that K was equally a criterion of the law of the resistance to be overcome—which changes from a resistance proportional to the velocity, and in exact accordance with the theoretical results obtained from the singular solution of the equation, when direct motion changes to sinuous, *i.e.*, when

$$\rho \frac{DU_m}{\mu} = K.$$

4. In the same paper I pointed out that the existence of this sudden change in the law of motion of fluids between solid surfaces when

$$DU_m = \frac{\mu}{\rho} K$$

proved the dependence of the manner of motion of the fluid on a relation between the product of the dimensions of the pipe multiplied by the velocity of the fluid, and the product of the molecular dimensions multiplied by the molecular velocities which determine the value of

$$\mu$$

for the fluid, also that the equations of motion for viscous fluid contained evidence of this relation.

These experimental results completely removed the discrepancy previously noticed, showing that, whatever may be the cause, in those cases in which the experimental results do not accord with those obtained by the singular solution of the equations, the actual motions of the water are different. But in this there is only a partial explanation, for there remains the mechanical or physical significance of the existence of the criterion to be explained.

5. [My object in this paper is to show that the theoretical existence of an inferior limit to the criterion follows from the equations of motion as a consequence:—

(1) Of a more rigorous examination and definition of the geometrical basis on which the analytical method of distinguishing between molar-motions and heat-motions in the kinetic theory of matter is founded; and

(2) Of the application of the same method of analysis, thus definitely founded, to distinguish between mean-molar-motions and relative-molar-motions, where, as in the case of steady-mean-flow along a pipe, the more rigorous definition of the geometrical basis shows the method to be strictly applicable, and in other cases where it is approximately applicable.

The geometrical relation of the motions respectively indicated by the terms mean-molar-, or MEAN-MEAN-MOTION, and relative-molar-, or RELATIVE-MEAN-MOTION, being essentially the same as the relation of the respective motions indicated by the terms molar-, or MEAN-MOTION, and relative-, or HEAT-MOTION, as used in the theory of gases.

I also show that the limit to the criterion obtained by this method of analysis, and by integrating the equations of motion in space, appears as a *geometrical limit* to the *possible simultaneous distribution of certain quantities in space*, and in no wise depends on the physical significance of these quantities. Yet the physical significance of these quantities, as defined in the equations, becomes so clearly exposed as to indicate that further study of the equations would elucidate the properties of matter and mechanical principles involved, and so be the means of explaining what has hitherto been obscure in the connection between thermodynamics and the principles of mechanics.

The geometrical basis of the method of analysis used in the kinetic theory of gases has hitherto consisted:—

(1) Of the geometrical *principle* that the motion of any point of a mechanical system may, at any instant, be abstracted into the mean-motion of the whole system at that instant, and the motion of the point relative to the mean-motion; and

(2) Of the *assumption* that the component, in any particular direction, of the velocity of a molecule, may be abstracted into a mean-component-velocity (say u) which is the mean-component-velocity of all the molecules in the immediate neighbourhood, and a relative-velocity (say ξ), which is the difference between u and the component-velocity of the molecule*; u and ξ being so related that, M being the mass of the molecule, the integrals of $(M\xi)$, and $(Mu\xi)$, &c., over all the molecules in the immediate neighbourhood are zero, and $\Sigma\,[M\,(u+\xi)^2] = \Sigma\,[M\,(u^2+\xi^2)]$†.

The geometrical principle (1) has only been used to distinguish between the energy of the mean-motion of the molecule, and the energy of its internal motions taken relatively to its mean-motion; and so to eliminate the internal motions from all further geometrical considerations which rest on the assumption (2).

* "Dynamical Theory of Gases," *Phil. Trans.*, 1866, p. 67.
† *Phil. Trans.*, 1866, p. 71.

That this assumption (2) is purely geometrical, becomes at once obvious, when it is noticed that the argument relates solely to the distribution in space of certain quantities at a particular instant of time. And it appears that the questions as to whether the assumed distinctions are possible under any distributions, and, if so, under what distribution, are proper subjects for geometrical solution.

On putting aside the apparent obviousness of the assumption (2), and considering definitely what it implies, the necessity for further definition at once appears.

The mean-component-velocity (u) of all the molecules in the immediate neighbourhood of a point, say P, can only be the mean-component-velocity of all the molecules in some space (S) enclosing P. u is then the mean-component-velocity of the mechanical system enclosed in S, and, for this system, is the mean-velocity at every point within S, and, multiplied by the entire mass within S, is the whole component momentum of the system. But according to the assumption (2), u with its derivatives are to be continuous functions of the position of P, which functions may vary from point to point even within S; so that u is not taken to represent the mean-component-velocity of the system within S, but the mean-velocity at the point P. Although there seems to have been no specific statement to that effect, it is presumable that the space S has been assumed to be so taken that P is the centre of gravity of the system within S. The relative positions of P and S being so defined, the shape and size of the space S requires to be further defined, so that u, &c., may vary continuously with the position of P, which is a condition that can always be satisfied if the size and shape of S may vary continuously with the position of P.

Having thus defined the relation of P to S and the shape and size of the latter, expressions may be obtained for the conditions of distribution of u, for which $\Sigma(M\xi)$ taken over S will be zero, *i.e.*, for which the condition of mean-momentum shall be satisfied.

Taking S_1, u_1, &c., as relating to a point P_1 and S, u, &c., as relating to P, another point, of which the component distances from P_1 are x, y, z; P_1 is the C.G. of S_1, and by however much or little S may overlap S_1, S has its centre of gravity at x, y, z, and is so chosen that u, &c., may be continuous functions of x, y, z; u may, therefore, differ from u_1 even if P is within S_1. Let u be taken for every molecule of the system S_1. Then according to assumption (2), $\Sigma(Mu)$ over S_1 must represent the component of momentum of the system within S_1, that is, in order to satisfy the condition of mean-momentum, the mean-value of the variable quantity u over the system S_1 must be equal to u_1 the mean-component-velocity of the system S_1, and this is a condition which, in consequence of the geometrical definition already mentioned, can only be satisfied under certain distributions of u. For since u is a continuous function of x, y, z, $M(u-u_1)$ may be expressed as a function of the derivatives of u at P_1 multiplied by corresponding powers and products of x, y, z, and again by M; and by equating the integral of this function over the space S_1 to zero, a definite expression is obtained, in terms of the limits imposed on x, y, z, by the already-defined space S_1 for the geometrical condition as to the distribution of u under which the condition of mean-momentum can be satisfied.

From this definite expression it appears, as has been obvious all through the argument, that the condition is satisfied if u is constant. It also appears that there are certain other well-defined systems of distribution for which the condition is strictly satisfied, and that for all other distributions of u the condition of mean-momentum can only be approximately satisfied to a degree for which definite expressions appear.

Having obtained the expression for the condition of distribution of u, so as to satisfy the condition of mean-momentum, by means of the expression for $M(u-u')$, &c., expressions are obtained for the conditions as to the distribution of ξ, &c., in order that the integrals over the space S_1 of the products $M(u\xi)$, &c. may be zero when $\Sigma[M(u-u_1)]=0$, and the conditions of mean-energy satisfied as well as those of mean-momentum. It then appears that in some particular cases of distribution of u, under which the condition of mean-momentum is strictly satisfied, certain conditions as to the distribution of ξ, &c., must be satisfied in order that the energies of mean- and relative-motion may be distinct. These conditions as to the distribution of ξ, &c., are, however, obviously satisfied in the case of heat-motion, and do not present themselves otherwise in this paper.

From the definite geometrical basis thus obtained, and the definite expressions which follow for the condition of distribution of u, &c., under which the method of analysis is strictly applicable, it appears that this method may be rendered generally applicable to any system of motion by a slight adaptation of the meaning of the symbols, and that it does not necessitate the elimination of the internal motion of the molecules, as has been the custom in the theory of gases.

Taking u, v, w to represent the motions (continuous or discontinuous) of the matter passing a point, and ρ to represent the density at the point, and putting $\bar{u}$, &c., for the mean-motion (instead of u as above), and u', &c., for the relative-motion (instead of ξ as before), the geometrical conditions as to the distribution of $\bar{u}$, &c., to satisfy the conditions of mean-momentum and mean-energy are, substituting ρ for M, of precisely the same form as before, and as thus expressed, the theorem is applicable to any mechanical system however abstract.

(1) In order to obtain the conditions of distribution of molar-motion, under which the condition of mean-momentum will be satisfied, so that the energy of molar-motion may be separated from that of the heat-motion, u, &c., and ρ are taken as referring to the actual motion and density at a point in a molecule, and S_1 is taken of such dimensions as may correspond to the scale, or periods in space, of the molecular distances, then the conditions of distribution of $\bar{u}$, under which the condition of mean-momentum is satisfied, become the conditions as to the distribution of molar-motion, under which it is possible to distinguish between the energies of molar-motions and heat-motions.

(2) And, when the conditions in (1) are satisfied to a sufficient degree of approximation by taking u to represent the molar-motion ($\bar{u}$ in (1)), and the dimensions of the space S to correspond with the period in space or scale of any possible periodic or eddying motion, the conditions as to the distribution of $\bar{u}$, &c. (the components of mean-mean-motion), which satisfy the condition of mean-momentum, show the conditions of mean-molar-motion, under which it is possible to separate the energy of mean-molar-motion from the energy of relative-molar- (or relative-mean-) motion.

Having thus placed the analytical method used in the kinetic theory on a definite geometrical basis, and adapted so as to render it applicable to all systems of motion, by applying it to the dynamical theory of viscous fluid, I have been able to show:—Feb. 18, 1895.]

(*a*) That the adoption of the conclusion arrived at by Sir Gabriel Stokes, that the dissipation function represents the rate at which heat is produced, adds a definition to the meaning of u, v, w—the components of mean or fluid velocity—which was previously wanting.

(*b*) That as the result of this definition the equations are true, and are only true, as applied to fluid in which the mean-motions of the matter, excluding the heat-motions, are steady.

(*c*) That the evidence of the possible existence of such steady mean-motions, while at the same time the conversion of the energy of these mean-motions into heat is going on, proves the existence of some *discriminative cause*, by which the *periods* in space and time of the mean-motion are prevented from approximating in magnitude to the corresponding *periods* of the heat-motions, and also proves the existence of some general action by which the energy of mean-motion is continually *transformed* into the energy of heat-motion, without passing through any intermediate stage.

(*d*) That as applied to fluid in unsteady mean-motion (excluding the heat-motions), however steady the mean integral flow may be, the equations are approximately true in a degree which increases with the ratios of the magnitudes of the *periods*, in time and space, of the mean-motion, to the magnitude of the corresponding periods of the heat-motions.

(*e*) That if the *discriminative cause* and the *action of transformation* are the result of general properties of matter, and not of properties which affect only the ultimate motions, there must exist evidence of similar actions as between the mean-mean-motion, in directions of mean-flow, and the periodic mean-motions taken relative to the mean-mean-motion but excluding heat-motions. And that such evidence must be of a general and important kind, such as the unexplained laws of the resistance of fluid motions, the law of the universal dissipation of energy, and the second law of thermo-dynamics.

(*f*) That the *generality* of the effects of the properties on which the *action of transformation* depends, is proved by the fact that resistance, other than proportional to the velocity, is caused by the relative (eddying) mean-motion.

(*g*) That the existence of the *discriminative cause* is directly proved by the existence of the *criterion*, the dependence of which on circumstances which limit the magnitudes of the periods of relative-mean-motion, as compared with the heat-motion, also proves the *generality* of the effects of the properties on which it depends.

(*h*) That the proof of the generality of the effects of the properties on which the discriminative cause, and the action of transformation depend, shows that—if in the equations of motion the mean-mean-motion is distinguished from the relative-mean-motion in the same way as the mean-motion is distinguished from the heat-motions—(1) the equations must contain expressions for the *transformation* of the energy of mean-mean-motion to energy of relative-mean-motion; and (2) that the equations, when integrated over a complete system, must show that the possibility of relative-mean-motion depends on the ratio of the possible magnitudes of the periods of relative-mean-motion, as compared with the corresponding magnitude of the periods of the heat-motions.

(*i*) That when the equations are transformed so as to distinguish between the mean-mean-motions, of infinite periods, and the relative-mean-motions of finite periods, there result two distinct systems of equations, one system for mean-mean-motion, as affected by relative-mean-motion and heat-motion, the other system for relative-mean-motion as affected by mean-mean-motion and heat-motions.

(*j*) That the equation of energy of mean-mean-motion, as obtained from the first system, shows that the rate of increase of energy is diminished by conversion into heat, and by transformation of energy of mean-mean-motion in consequence of the relative-mean-motion, which transformation is expressed by a function identical in form with that which expresses the conversion into heat; and that the equation of energy of relative-mean-motion, obtained from the second system, shows that this energy is increased only by transformation of energy from mean-mean-motion expressed by the same function, and diminished only by the conversion of energy of relative-mean-motion into heat.

(k) That the difference of the two rates (1) transformation of energy of mean-mean-motion into energy of relative-mean-motion as expressed by the transformation function, (2) the conversion of energy of relative-mean-motion into heat, as expressed by the function expressing dissipation of the energy of relative-mean-motion, affords a discriminating equation as to the conditions under which relative-mean-motion can be maintained.

(l) That this discriminating equation is independent of the energy of relative-mean-motion, and expresses a relation between variations of mean-mean-motion of the first order, the space periods of relative-mean-motion, and μ/ρ, such that any circumstances which determine the maximum periods of the relative-mean-motion, determine the conditions of mean-mean-motion under which relative-mean-motion will be maintained, that is, determine *the criterion.*

(m) That as applied to water in steady mean-flow between parallel plane surfaces, the boundary conditions, and the equation of continuity, impose limits to the maximum space periods of relative-mean-motion, such that the discriminating equation affords definite proof that when an indefinitely small sinuous or relative disturbance exists, it must fade away if

$$\rho D U_m/\mu$$

is less than a certain number, which depends on the shape of the section of the boundaries, and is constant as long as there is geometrical similarity. While for greater values of this function, in so far as the discriminating equation shows, the energy of sinuous motion may increase until it reaches to a definite limit, and rules the resistance.

(n) That besides thus affording a mechanical explanation of the existence of the criterion K, the discriminating equation shows the purely geometrical circumstances on which the value of K depends, and although these circumstances must satisfy geometrical conditions required for steady mean-motion other than those imposed by the conservations of mean-energy and momentum, the theory admits of the determination of an inferior limit to the value of K under any definite boundary conditions, which, as determined for the particular case, is

517.

This is below the experimental value for round pipes, and is about half what might be expected to be the experimental value for a flat pipe, which leaves a margin to meet the other kinematical conditions for steady mean-mean-motion.

(*o*) That the discriminating equation also affords a definite expression for the resistance, which proves that, with smooth fixed boundaries, the conditions of dynamical similarity under any geometrical similar circumstances depend only on the value of

$$\frac{\rho}{\mu^2}\frac{dp}{dx}b^3,$$

where b is one of the lateral dimensions of the pipe; and that the expression for this resistance is complex, but shows that above the critical velocity the relative-mean-motion is limited, and that the resistances increase as a power of the velocity higher than the first.

SECTION II.

The Mean-motion and Heat-motions as distinguished by Periods.—Mean-mean-motion and Relative-mean-motion.—Discriminative Cause and Action of Transformation.—Two Systems of Equations.—A Discriminating Equation.

6. Taking the general equations of motion for incompressible fluid, subject to no external forces to be expressed by

$$\left.\begin{aligned}\rho\frac{du}{dt}&=-\left\{\frac{d}{dx}(p_{xx}+\rho uu)+\frac{d}{dy}(p_{yx}+\rho uv)+\frac{d}{dz}(p_{zx}+\rho uw)\right\}\\ \rho\frac{dv}{dt}&=-\left\{\frac{d}{dx}(p_{xy}+\rho vu)+\frac{d}{dy}(p_{yy}+\rho vv)+\frac{d}{dz}(p_{zy}+\rho vw)\right\}\\ \rho\frac{dw}{dt}&=-\left\{\frac{d}{dx}(p_{xz}+\rho wu)+\frac{d}{dy}(p_{yz}+\rho wv)+\frac{d}{dz}(p_{zz}+\rho ww)\right\}\end{aligned}\right\}\ldots\ldots(1),$$

with the equation of continuity

$$0=du/dx+dv/dy+dw/dz\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2),$$

where p_{xx}, &c., are arbitrary expressions for the component forces per unit of area, resulting from the stresses, acting on the negative faces of planes perpendicular to the direction indicated by the first suffix, in the direction indicated by the second suffix.

Then multiplying these equations respectively by u, v, w, integrating by parts, adding and putting

$$2E \text{ for } \rho\,(u^2+v^2+w^2)$$

and transposing, the rate of increase of kinetic energy per unit of volume is given by

$$\left(\frac{d}{dt}+u\frac{d}{dx}+v\frac{d}{dy}+w\frac{d}{dz}\right)E=-\left\{\begin{aligned}&\frac{d}{dx}(up_{xx})+\frac{d}{dy}(up_{yx})+\frac{d}{dz}(up_{zx})\\&+\frac{d}{dx}(vp_{xy})+\frac{d}{dy}(vp_{yy})+\frac{d}{dz}(vp_{zy})\\&+\frac{d}{dx}(wp_{xz})+\frac{d}{dy}(wp_{yz})+\frac{d}{dz}(wp_{zz})\end{aligned}\right\}$$

$$+\left\{\begin{aligned}&p_{xx}\frac{du}{dx}+p_{yx}\frac{du}{dy}+p_{zx}\frac{du}{dz}\\&+p_{xy}\frac{dv}{dx}+p_{yy}\frac{dv}{dy}+p_{zy}\frac{dv}{dz}\\&+p_{xz}\frac{dw}{dx}+p_{yz}\frac{dw}{dy}+p_{zz}\frac{dw}{dz}\end{aligned}\right\}\quad\ldots\ldots\ldots\ldots\ldots(3).$$

The left member of this equation expresses the *rate of increase* in the kinetic energy of the fluid per unit of volume at a point moving with the fluid.

The first term on the right expresses the *rate at which work* is being done by the surrounding fluid per unit of volume at a point.

The second term on the right therefore, by the law of conservation of energy, expresses the difference between the rate of increase of kinetic energy and the rate at which work is being done by the stresses. This difference has, so far as I am aware, in the absence of other forces, or any changes of potential energy, been equated to the rate at which heat is being converted into energy of motion, Sir Gabriel Stokes having first indicated this* as resulting from the law of conservation of energy then just established by Joule.

7. This conclusion, that the second term on the right of (3) expresses the rate at which heat is being converted, as it is usually accepted, may be correct enough, but there is a consequence of adopting this conclusion which enters largely into the method of reasoning in this paper, but which, so far as I know, has not previously received any definite notice.

* *Cambridge Phil. Trans.*, vol. IX. p. 57.

The Component Velocities in the Equations of Viscous Fluids.

In no case, that I am aware of, has any very strict definition of u, v, w, as they occur in the equations of motion, been attempted. They are usually defined as the velocities of a particle at a point (x, y, z) of the fluid, which may mean that they are the actual component-velocities of the point in the matter passing at the instant, or that they are the mean-velocities of all the matter in some space enclosing the point, or which passes the point in an interval of time. If the first view is taken, then the right-hand member of the equation represents the rate of increase of kinetic energy, per unit of volume, in the matter at the point; and the integral of this expression over any finite space S, moving with the fluid, represents the total rate of increase of kinetic energy, including heat-motion, within that space; hence the difference between the rate at which work is done on the surface of S, and the rate at which kinetic energy is increasing can, by the law of conservation of energy, only represent the rate at which that part of the heat which does not consist in kinetic energy of matter is being produced, whence it follows:—

(*a*) *That the adoption of the conclusion that the second term in equation* (3) *expresses the rate at which heat is being converted, defines u, v, w, as not representing the component-velocities of points in the passing matter.*

Further, if it is understood that u, v, w, represent the mean velocities of the matter in some space, enclosing x, y, z, the point considered, or the mean-velocities at a point taken over a certain interval of time, so that $\Sigma(\rho u)$, $\Sigma(\rho v)$, $\Sigma(\rho w)$ may express the components of momentum, and $x\Sigma(\rho v) - y\Sigma(\rho u)$, &c., &c., may express the components of moments of momentum of the matter over which the mean is taken; there still remains the question as to what spaces and what intervals of time.

(*b*) *Hence the conclusion that the second term expresses the rate of conversion of heat, defines the spaces and intervals of time over which the mean-component-velocities must be taken, so that E may include all the energy of mean-motion, and exclude that of heat-motions.*

Equations Approximate only except in Three Particular Cases.

8. According to the reasoning of the last article, if the second term on the right of equation (3) expresses the rate at which heat is being converted into energy of mean-motion, either $\rho u, \rho v, \rho w$ express the mean components of momentum of the matter, taken at any instant over a space S_0 enclosing the point x, y, z, to which u, v, w refer, so that this point is the centre of gravity of the matter within S_0 and such that ρ represents the mean density of the matter within this space; or $\rho u, \rho v, \rho w$ represent the mean components of momentum taken at x, y, z over an interval of time τ, such that ρ is

the mean density over the time τ, and if t marks the instant to which u, v, w refer, and t' any other instant, $\Sigma[(t-t')\rho]$, in which ρ is the actual density, taken over the interval τ is zero. The equations, however, require, that so obtained, ρ, u, v, w shall be continuous functions of space and time, and it can be shown that this involves certain conditions between the distribution of the mean-motion and the dimensions of S_0 and τ.

Mean- and Relative-motions of Matter.

Whatever the motions of matter within a fixed space S may be at any instant, if the component-velocities at a point are expressed by u, v, w, the mean-component-velocities taken over S will be expressed by

$$\bar{u} = \frac{\Sigma(\rho u)}{\Sigma(\rho)}, \text{ \&c., \&c.} \quad \text{..........................(4).}$$

If then $\bar{u}$, $\bar{v}$, $\bar{w}$ are taken at each instant as the velocities of x, y, z, the *instantaneous centre of gravity of the matter within* S, the component momentum at the centre of gravity may be put

$$\rho u = \rho\bar{u} + \rho u' \quad \text{................................(5),}$$

where u' is the motion of the matter, relative to axes moving with the mean velocity, at the centre of gravity of the matter within S. Since a space S of definite size and shape may be taken about any point x, y, z in an indefinitely larger space, so that x, y, z is the centre of gravity of the matter within S, the motion in the larger space may be divided into two distinct systems of motion, of which $\bar{u}$, $\bar{v}$, $\bar{w}$ represent a mean-motion at each point and u', v', w' a motion at the same point relative to the mean-motion at the point.

If, however, $\bar{u}$, $\bar{v}$, $\bar{w}$ are to represent the real mean-motion, it is necessary that $\Sigma(\rho u')$, $\Sigma(\rho v')$, $\Sigma(\rho w')$ summed over the space S, taken about any point, shall be severally zero; and in order that this may be so, certain conditions must be fulfilled.

For taking x, y, z, for G, the centre of gravity of the matter within S, and x', y', z' for any other point within S, and putting a, b, c for the dimensions of S in directions x, y, z, measured from the point x, y, z; since $\bar{u}$, $\bar{v}$, $\bar{w}$ are continuous functions of x, y, z by shifting S so that the centre of gravity of the matter within it is at x', y', z', the value of $\bar{u}$ for this point is given by

$$\bar{u} = \bar{u}_G + (x'-x)\left(\frac{d\bar{u}}{dx}\right)_G + (y'-y)\left(\frac{d\bar{u}}{dy}\right)_G + (z'-z)\left(\frac{d\bar{u}}{dz}\right)_G + \tfrac{1}{2}(x'-x)^2\left(\frac{d^2\bar{u}}{dx^2}\right)_G$$
$$+ \text{\&c.} \quad \text{..................(6),}$$

where all the differential coefficients on the left refer to the point x, y, z; and in the same way for $\bar{v}$ and $\bar{w}$.

Subtracting the value of $\bar{u}$ thus obtained for the point x', y', z', from that of u at the same point, the difference is the value of u' at this point, whence summing these differences over the space S about G at x, y, z, since by definition when summed over the space S about G

$$\Sigma\left[\rho\left(u-\bar{u}_G\right)\right]=0 \text{ and } \Sigma\left[\rho\left(x'-x\right)\right]=0 \quad \ldots\ldots\ldots\ldots\ldots (7).$$

$$\left.\begin{aligned}
\Sigma(\rho u') = -\Big\{\tfrac{1}{2}\Sigma\left[\rho(x-x')^2\right]\left(\frac{d^2\bar{u}}{dx^2}\right)_G &+ \tfrac{1}{2}\Sigma\left[\rho(y-y')^2\right]\left(\frac{d^2\bar{u}}{dy^2}\right)_G \\
&+ \tfrac{1}{2}\Sigma\left[\rho(z-z')^2\right]\left(\frac{d^2\bar{u}}{dz^2}\right)_G + \&c.\Big\} \\
\text{That is} \qquad & \\
\frac{\Sigma(\rho u')}{\Sigma(\rho)} \text{ is } < -\left\{\frac{a^2}{2}\left(\frac{d^2\bar{u}}{dx^2}\right)_G + \frac{b^2}{2}\left(\frac{d^2\bar{u}}{dy^2}\right)_G \right. &\left. + \frac{c^2}{2}\left(\frac{d^2\bar{u}}{dz^2}\right)_G + \&c.\right\}
\end{aligned}\right\} \ldots\ldots (8\text{A}).$$

In the same way if Σ be taken over the interval of time τ including t; and for the instant t

$$\bar{u}=\frac{\Sigma(\rho u)}{\Sigma(\rho)}, \text{ and } \rho\bar{u}=\rho u+\rho u';$$

then since for any other instant t'

$$\bar{u}=\bar{u}_t+(t-t')\left(\frac{d\bar{u}}{dt}\right)_t+\tfrac{1}{2}(t-t')^2\left(\frac{d^2\bar{u}}{dt^2}\right)_t+\&c.,$$

where $\Sigma\left[\rho(t-t')\right]=0$, and $\Sigma\left[\rho(\bar{u}_t-u)\right]=0$.

It appears that

$$\left.\begin{aligned}
\Sigma(\rho u') &= -\Sigma\left[\tfrac{1}{2}\rho(t-t')^2\right]\frac{d^2u}{dt^2}+\&c. \\
\frac{\Sigma(\rho u')}{\Sigma(\rho)} &\text{ is } < -\tfrac{1}{2}\tau^2\left(\frac{d^2\bar{u}}{dt^2}\right)_t - \&c.
\end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots (8\text{B}).$$

From equations (8A) and (8B), and similar equations for $\Sigma(\rho v')$ and $\Sigma(\rho w')$, it appears that if

$$\Sigma(\rho u')=\Sigma(\rho v')=\Sigma(\rho w')=0,$$

where the summation extends both over the space S and the interval τ, all the terms on the right of equations (8A) and (8B) must be respectively and continuously zero, or, what is the same thing, all the differential coefficients of $\bar{u}$, $\bar{v}$, $\bar{w}$ with respect to x, y, z and t of the first order must be respectively constant.

This condition will be satisfied if the mean-motion is steady, or uniformly varying with the time, and is everywhere in the same direction, being subject to no variations in the direction of motion; for suppose the direction of motion to be that of x, then since the periodic motion passes through a complete period within the distance $2a$, $\Sigma(\rho u')$ will be zero within the space

$$2a \,.\, dy \,.\, dz,$$

however small $dy \,.\, dz$ may be, and since the only variations of the mean-motion are in directions y and z, in which b and c may be taken zero, and du/dt is everywhere constant, the conditions are perfectly satisfied.

The conditions are also satisfied if the mean-motion is that of uniform expansion or contraction, or is that of a rigid body.

These three cases, in which it may be noticed that variations of mean-motion are everywhere uniform in the direction of motion, and subject to steady variations in respect of time, are the only cases in which the conditions (8 A), (8 B), can be perfectly satisfied.

The conditions will, however, be approximately satisfied, when the variations of $\bar{u}$, $\bar{v}$, $\bar{w}$ of the first order are approximately constant over the space S.

In such case the right-hand members of equations (8 A), (8 B), are neglected, and it appears that the closeness of the approximations will be measured by the relative magnitude of such terms as

$$a\frac{d^2\bar{u}}{dx^2}, \text{ \&c., } \tau\frac{d^2\bar{u}}{dt^2} \text{ as compared with } \frac{d\bar{u}}{dx}, \ \frac{d\bar{u}}{dt}, \text{ \&c.}$$

Since frequent reference must be made to these relative values, and, as in periodic motion, the relative values of such terms are measured by the period (in space or time) as compared with a, b, c and τ, which are, in a sense, the periods of u', v', w', I shall use the term period in this sense, taking note of the fact that when the mean-motion is constant in the direction of motion, or varies uniformly in respect of time, it is not periodic, *i.e.*, its periods are infinite.

9. It is thus seen that the closeness of the approximation with which the motion of any system can be expressed as a varying mean-motion together with a relative-motion, which, when integrated over a space of which the dimensions are a, b, c, has no momentum, increases as the magnitude of the periods of $\bar{u}$, $\bar{v}$, $\bar{w}$ in comparison with the periods of u', v', w', and is measured by the ratio of the relative orders of magnitudes to which these periods belong.

Heat-motions in Matter are Approximately Relative to the Mean-motions.

The general experience that heat in no way affects the momentum of matter, shows that the heat-motions are relative to the mean-motions of matter taken over spaces of sensible size. But, as heat is by no means the only state of relative-motion of matter, if the heat-motions are relative to all mean-motions of matter, whatsoever their periods may be, it follows—that there must be some *discriminative cause* which prevents the existence of relative-motions of matter other than heat, except mean-motions with periods in time and space of greatly higher orders of magnitude than the corresponding periods of the heat-motions—otherwise, by equations (8A), (8B), heat-motions could not be to a high degree of approximation relative to all other motions, and we could not have to a high degree of approximation,

$$\left.\begin{array}{l} p_{xx}\dfrac{du}{dx}+p_{yx}\dfrac{du}{dy}+p_{zx}\dfrac{du}{dz} \\ p_{xy}\dfrac{dv}{dx}+p_{yy}\dfrac{dv}{dy}+p_{zy}\dfrac{dv}{dz} \\ p_{xz}\dfrac{dw}{dx}+p_{yz}\dfrac{dw}{dy}+p_{zz}\dfrac{dw}{dz} \end{array}\right\}=-\frac{d}{dt}(pH) \quad\text{..............} \quad (9),$$

where the expression on the right stands for the rate at which heat is converted into energy of mean-motion.

Transformation of Energy of Relative-mean-motion to Energy of Heat-motion.

10. The recognition of the existence of a *discriminative cause*, which prevents the existence of relative-mean-motions with periods of the same order of magnitude as heat-motions, proves the existence of another general action by which the energy of relative-mean-motion, of which the periods are of another and higher order of magnitude than those of the heat-motions, is *transformed* to energy of heat-motion.

For if relative-mean-motions cannot exist with periods approximating to those of heat, the conversion of energy of mean-motion into energy of heat, proved by Joule, cannot proceed by the gradual degradation of the periods of mean-motion until these periods coincide with those of heat, but must, in its final stages, at all events, be the result of some action which causes the energy of relative-mean-motion to be transformed into the energy of heat-motions, without intermediate existence in states of relative-motion, with intermediate and gradually diminishing periods.

That such change of energy of mean-motion to energy of heat may be properly called transformation, becomes apparent when it is remembered that neither mean-motion nor relative-motion has any separate existence, but are only abstract quantities, determined by the particular process of abstraction, and so changes in the actual-motion may, by the process of abstraction, cause transformation of the abstract energy of the one abstract-motion, to abstract energy of the other abstract-motion.

All such transformation must depend on the changes in the actual-motions, and so must depend on mechanical principles and the properties of matter, and hence the direct passage of energy of relative-mean-motion to energy of heat-motions is evidence of a general cause of the condition of actual-motion which results in transformation—which may be called *the cause of transformation.*

The Discriminative Cause, and the Cause of Transformation.

11. The only known characteristic of heat-motions, besides that of being relative to the mean-motion, already mentioned, is that the motions of matter which result from heat are an ultimate form of motion which does not alter so long as the mean-motion is uniform over the space, and so long as no change of state occurs in the matter. In respect of this characteristic, heat-motions are, so far as we know, unique, and it would appear that heat-motions are distinguished from the mean-motions by some ultimate properties of matter.

It does not, however, follow that the cause of transformation, or even the discriminative cause, are determined by these properties. Whether this is so or not can only be ascertained by experience. If either or both these causes depend solely on properties of matter which only affect the heat-motions, then no similar effect would result as between the variations of mean-mean-motion and relative-mean-motion, whatever might be the difference in magnitude of their respective periods. Whereas, if these causes depend on properties of matter which affect all modes of motion, distinctions in periods must exist between mean-mean-motion and relative-mean-motion, and transformation of energy take place from one to the other, as between the mean-motion and the heat-motions.

The mean-mean-motion cannot, however, under any circumstances stand to the relative-mean-motion in the same relation as the mean-motion stands to the heat-motions, because the heat-motions cannot be absent, and in addition to any transformation from mean-mean-motion to relative-mean-motion, there are transformations both from mean- and relative-mean-motion to heat-motions, which transformation may have important effects on both the transformation of energy from mean- to relative-mean-motion, and on the discriminative cause of distinction in their periods.

In spite of the confusing effect of the ever present heat-motions, it would, however, seem that evidence as to the character of the properties on which the cause of transformation and the discriminative cause depend, should be forthcoming as the result of observing the mean- and relative-mean-motions of matter.

12. To prove by experimental evidence that the effects of these properties of matter are confined to the heat-motions, would be to prove a negative; but if these properties are in any degree common to all modes of matter, then at first sight it must seem in the highest degree improbable that the effects of these causes on the mean- and relative-mean-motions

would be obscure, and only to be observed by delicate tests. For properties which can cause distinctions between the mean- and heat-motions of matter so fundamental and general, that from the time these motions were first recognized the distinction has been accepted as part of the order of nature, and has been so familiar to us that its cause has excited no curiosity, must, if they have any effect at all, cause effects which are general and important on the mean-motions of matter. It would thus seem that evidence of the general effects of such properties should be sought in those laws and phenomena known to us as the result of experience, but of which no rational explanation has hitherto been found; such as the law that the resistance of fluids moving between solid surfaces and of solids moving through fluids, in such a manner that the general-motion is not periodic, is as the square of the velocities, the evidence covered by the law of the universal tendency of all energy to dissipation, and the second law of thermodynamics.

13. In considering the first of the instances mentioned, it will be seen that the evidence it affords as to the general effect of the properties, on which depends transformation of energy from mean- to relative-motion, is very direct. For, since my experiments with colour bands have shown that when the resistance of fluids, in steady mean flow, varies with a power of the velocity higher than the first, the fluid is always in a state of sinuous motion, it appears that the prevalence of such resistance is evidence of the existence of a general action, by which energy of mean-mean-motion, with infinite periods, is directly transformed to the energy of relative-mean-motion, with finite periods, represented by the eddying motion, which renders the general mean-motion sinuous, by which transformation the state of eddying-motion is maintained, notwithstanding the continual transformation of its energy into heat-motions.

We have thus direct evidence that properties of matter which determine the cause of transformation, produce general and important effects which are not confined to the heat-motions.

In the same way, the experimental demonstration I was able to obtain, that relative-mean-motion in the form of eddies of finite periods, both as shown by colour bands and as shown by the law of resistances, cannot be maintained except under circumstances depending on the conditions which determine the superior limits to the velocity of the mean-mean-motion, of infinite periods, and the periods of the relative-mean-motion, as defined in the criterion

$$DU_m/\mu = K \qquad (10),$$

is not only a direct experimental proof of the existence of a discriminative cause which prevents the maintenance of periodic mean-motion except with periods greatly in excess of the periods of the heat-motions, but also indicates that the discriminative cause depends on properties of matter which affect the mean-motions as well as the heat-motions.

Expressions for the Rate of Transformation and the Discriminative Cause.

14. It has already been shown (Art. 8) that the equations of motion approximate to a true expression of the relations between the mean-motions and stresses, when the ratio of the periods of mean-motions to the periods of the heat-motions approximates to infinity. Hence it follows that these equations must of necessity include whatever mechanical or kinematical principles are involved in the transformation of energy of mean-mean-motion to energy of relative-mean-motion. It has also been shown that the properties of matter, on which depends the transformation of energy of varying mean-motion to relative-motion, are common to the relative-mean-motion as well as to the heat-motion. Hence, if the equations of motion are applied to a condition in which the mean-motion consists of two components, the one component being a mean-mean-motion, as obtained by integrating the mean-motion over spaces S_1 taken about the point x, y, z, as centre of gravity, and the other component being a relative-mean-motion, of which the mean components of momentum taken over the space S_1 everywhere vanish, it follows:—

(1) *That the resulting equations of motion must contain an expression for the rate of transformation from energy of mean-mean-motion to energy of relative-mean-motion, as well as the expressions for the transformation of the respective energies of mean- and relative-mean-motion to energy of heat-motion.*

(2) *That, when integrated over a complete system these equations must show that the possibility of the maintenance of the energy of relative-mean-motion depends, whatsoever may be the conditions, on the possible order of magnitudes of the periods of the relative-mean-motion, as compared with the periods of the heat-motions.*

The Equations of Mean- and Relative-mean-motion.

15. These last conclusions, besides bringing the general results of the previous argument to the test point, suggest the manner of adaptation of the equations of motion, by which the test may be applied.

Put $$u = \bar{u} + u', \quad v = \bar{v} + v', \quad w = \bar{w} + w' \text{.....................(11)},$$

where $$\bar{u} = \frac{\Sigma(\rho u)}{\Sigma(\rho)}, \text{ \&c., \&c. ....(12)},$$

the summation extending over the space S_1 of which the centre of gravity is at the point x, y, z. Then since u, v, w are continuous functions of x, y, z, therefore $\bar{u}$, $\bar{v}$, $\bar{w}$, and u', v', w', are continuous functions of x, y, z. And as ρ is assumed constant, the equations of continuity for the two systems of motion are:

$$\frac{d\bar{u}}{dx}+\frac{d\bar{v}}{dy}+\frac{d\bar{w}}{dz}=0 \quad\text{and}\quad \frac{du'}{dx}+\frac{dv'}{dy}+\frac{dw'}{dz}=0 \quad\text{.............(13)};$$

also both systems of motions must satisfy the boundary conditions, whatever they may be.

Further putting $\bar{p}_{xx}$, &c., for the mean values of the stresses taken over the space S_1 and

$$p'_{xx}=p_{xx}-\bar{p}_{xx} \quad\text{..............................(14)},$$

and defining S_1 to be such that the space variations of $\bar{u}$, $\bar{v}$, $\bar{w}$ are approximately constant over this space, we have, putting $\overline{u'u'}$, &c., for the mean values of the squares and products of the components of relative-mean-motion, for the equations of mean-mean-motion,

$$\left.\begin{aligned} \rho\frac{d\bar{u}}{dt} &= -\left\{\frac{d}{dx}\left(\bar{p}_{xx}+\rho\overline{uu}+\rho\overline{u'u'}\right)\right.\\ &\qquad +\frac{d}{dy}\left(\bar{p}_{yx}+\rho\overline{uv}+\rho\overline{u'v'}\right)\\ &\qquad \left.+\frac{d}{dz}\left(\bar{p}_{zx}+\rho\overline{uw}+\rho\overline{u'w'}\right)\right\}\\ \text{\&c.} &= \qquad \text{\&c.}\\ \text{\&c.} &= \qquad \text{\&c.} \end{aligned}\right\} \text{..................(15)},$$

which equations are approximately true at every point in the same sense as that in which the equations (1) of mean-motion are true.

Subtracting these equations of mean-mean-motion from the equations of mean-motion, we have

$$\rho\frac{du'}{dt}=-\left\{\begin{aligned} &\frac{d}{dx}\left\{p'_{xx}+\rho\left(\bar{u}u'+u'\bar{u}\right)+\rho\left(u'u'-\overline{u'u'}\right)\right\}\\ +&\frac{d}{dy}\left\{p'_{yx}+\rho\left(\bar{u}v'+u'\bar{v}\right)+\rho\left(u'v'-\overline{u'v'}\right)\right\}\\ +&\frac{d}{dz}\left\{p'_{zx}+\rho\left(\bar{u}w'+u'\bar{w}\right)+\rho\left(u'w'-\overline{u'w'}\right)\right\} \end{aligned}\right\} \text{\&c., \&c....(16)},$$

which are the equations of momentum of relative-mean-motion at each point.

Again, multiplying the equations of mean-mean-motion by $\bar{u}$, $\bar{v}$, $\bar{w}$ respectively, adding and putting $2\bar{E}=\rho\left(\bar{u}^2+\bar{v}^2+\bar{w}^2\right)$, we obtain

$$\left(\frac{d}{dt}+\bar{u}\frac{d}{dx}+\bar{v}\frac{d}{dy}+\bar{w}\frac{d}{dz}\right)\bar{E}$$

$$= -\left\{\begin{array}{l} \frac{d}{dx}[\bar{u}(\bar{p}_{xx}+\rho\overline{u'u'})]+\frac{d}{dy}[\bar{u}(\bar{p}_{yx}+\rho\overline{u'v'})]+\frac{d}{dz}[\bar{u}(\bar{p}_{zx}+\rho\overline{u'w'})] \\ +\frac{d}{dx}[\bar{v}(\bar{p}_{xy}+\rho\overline{v'u'})]+\frac{d}{dy}[\bar{v}(\bar{p}_{yy}+\rho\overline{v'v'})]+\frac{d}{dz}[\bar{v}(\bar{p}_{zy}+\rho\overline{v'w'})] \\ +\frac{d}{dx}[\bar{w}(\bar{p}_{xz}+\rho\overline{w'u'})]+\frac{d}{dy}[\bar{w}(\bar{p}_{yz}+\rho\overline{w'v'})]+\frac{d}{dz}[\bar{w}(\bar{p}_{zz}+\rho\overline{w'w'})] \end{array}\right\}$$

$$+\left\{\begin{array}{l} \bar{p}_{xx}\frac{d\bar{u}}{dx}+\bar{p}_{yx}\frac{d\bar{u}}{dy}+\bar{p}_{zx}\frac{d\bar{u}}{dz} \\ +\bar{p}_{xy}\frac{d\bar{v}}{dx}+\bar{p}_{yy}\frac{d\bar{v}}{dy}+\bar{p}_{zy}\frac{d\bar{v}}{dz} \\ +\bar{p}_{xz}\frac{d\bar{w}}{dx}+\bar{p}_{yz}\frac{d\bar{w}}{dy}+\bar{p}_{zz}\frac{d\bar{w}}{dz} \end{array}\right\}+\rho\left\{\begin{array}{l} \overline{u'u'}\frac{d\bar{u}}{dx}+\overline{u'v'}\frac{d\bar{u}}{dy}+\overline{u'w'}\frac{d\bar{u}}{dz} \\ +\overline{v'u'}\frac{d\bar{v}}{dx}+\overline{v'v'}\frac{d\bar{v}}{dy}+\overline{v'w'}\frac{d\bar{v}}{dz} \\ +\overline{w'u'}\frac{d\bar{w}}{dx}+\overline{w'v'}\frac{d\bar{w}}{dy}+\overline{w'w'}\frac{d\bar{w}}{dz} \end{array}\right\} \dots(17),$$

which is the approximate equation of energy of mean-mean-motion in the same sense as the equation (3) of energy of mean-motion is approximate.

In a similar manner multiplying the equations (16) for the momentum of relative-mean-motion respectively by u', v', w', and adding, the result would be the equation for energy of relative-mean-motion at a point, but this would include terms of which the mean values taken over the space S_1 are zero, and, since all corresponding terms in the energy of heat are excluded, by summation over the space S_0 in the expression for the rate at which mean-motion is transformed into heat, there is no reason to include them for the space S_1; so that, omitting all such terms and putting

$$2\bar{E}'=\rho\,(\overline{u'^2}+\overline{v'^2}+\overline{w'^2}) \dots\dots\dots\dots(18),$$

we obtain

$$\left(\frac{d}{dt}+\bar{u}\frac{d}{dx}+\bar{v}\frac{d}{dy}+\bar{w}\frac{d}{dz}\right)\bar{E}'$$

$$= -\left\{\begin{array}{l} \frac{d}{dx}[u'(p'_{xx}+\rho u'u')]+\frac{d}{dy}[u'(p'_{yx}+\rho u'v')]+\frac{d}{dz}[u'(p'_{zx}+\rho u'w')] \\ +\frac{d}{dx}[v'(p'_{yx}+\rho v'u')]+\frac{d}{dy}[v'(p'_{yy}+\rho v'v')]+\frac{d}{dz}[v'(p'_{zy}+\rho v'w')] \\ +\frac{d}{dx}[w'(p'_{xz}+\rho w'u')]+\frac{d}{dy}[w'(p'_{yz}+\rho w'v')]+\frac{d}{dz}[w'(p'_{zz}+\rho w'w')] \end{array}\right\}$$

$$+\left\{\begin{array}{l} p'_{xx}\frac{du'}{dx}+p'_{yx}\frac{du'}{dy}+p'_{zx}\frac{du'}{dz} \\ +p'_{xy}\frac{dv'}{dx}+p'_{yy}\frac{dv'}{dy}+p'_{zy}\frac{dv'}{dz} \\ +p'_{xz}\frac{dw'}{dx}+p'_{yz}\frac{dw'}{dy}+p'_{zz}\frac{dw'}{dz} \end{array}\right\}-\left\{\begin{array}{l} \rho\overline{u'u'}\frac{d\bar{u}}{dx}+\rho\overline{u'v'}\frac{d\bar{u}}{dy}+\rho\overline{u'w'}\frac{d\bar{u}}{dz} \\ +\rho\overline{v'u'}\frac{d\bar{v}}{dx}+\rho\overline{v'v'}\frac{d\bar{v}}{dy}+\rho\overline{v'w'}\frac{d\bar{v}}{dz} \\ +\rho\overline{w'u'}\frac{d\bar{w}}{dx}+\rho\overline{w'v'}\frac{d\bar{w}}{dy}+\rho\overline{w'w'}\frac{d\bar{w}}{dz} \end{array}\right\}$$

$$\dots\dots\dots\dots(19),$$

where only the mean values, over the space S_1, of the expressions in the right member are taken into account.

This is the equation for the mean rate, over the space S_1, of change in the energy of relative-mean-motion per unit of volume.

It may be noticed that the rate of change in the energy of mean-mean-motion, together with the mean rate of change in the energy of relative-mean-motion, must be the total mean-rate of change in the energy of mean-motion, and that by adding the equations (17) and (19) the result is the same as is obtained from the equation (3) of energy of mean-motion by omitting all terms which have no mean value as summed over the space S_1.

The Expressions for Transformation of Energy from Mean-mean-motion to Relative-mean-motion.

16. When equations (17) and (19) are added together, the only expressions that do not appear in the equation of mean-energy of mean-motion are the last terms on the right of each of the equations, which are identical in form and opposite in sign.

These terms, which thus represent no change in the total energy of mean-motion, can only represent a transformation from energy of mean-mean-motion to energy of relative-mean-motion. And as they are the only expressions which do not form part of the general expression for the rate of change of the mean energy of mean-motion, they represent the total exchange of energy between the mean-mean-motion and the relative-mean-motion.

It is also seen that the action, of which these terms express the effect, is purely kinematical, depending simply on the instantaneous characters of the mean- and relative-mean-motion, whatever may be the properties of the matter involved, or the mechanical actions which have taken part in determining these characters. The terms, therefore, express the entire result of transformation from energy of mean-mean-motion to energy of relative-mean-motion, and of nothing but the transformation. Their existence thus completely verifies the first of the general conclusions in Art. 14.

The term last but one in the right member of the equation (17) for energy of mean-mean-motion, expresses the rate of transformation of energy of heat-motions to that of energy of mean-mean-motion, and is entirely independent of the relative-mean-motion.

In the same way, the term, last but one on the right of the equation (19) for energy of relative-mean-motion, expresses the rate of transformation from energy of heat-motions to energy of relative-mean-motion, and is quite independent of the mean-mean-motion.

17. In both equations (17) and (19) the first terms on the right express the rates at which the respective energies of mean- and relative-mean-motion are increasing on account of work done by the stresses on the mean- and relative-motions respectively, and by the additions of momentum caused by convections of relative-mean-motion by relative-mean-motion to the mean- and relative-mean-motions respectively.

It may also be noticed that while the first term on the right, in the equation (19) of energy of relative-mean-motion, is independent of mean-mean-motion, the corresponding term in equation (17) for mean-mean-motion is not independent of relative-mean-motion.

A Discriminating Equation.

18. In integrating the equations over a space moving with the mean-mean-motion of the fluid, the first terms on the right may be expressed as surface integrals, which integrals respectively express the rates at which work is being done on, and energy is being received across the surface, by the mean-mean-motion, and by the relative-mean-motion.

If the space over which the integration extends includes the whole system, or such part that the total energy conveyed across the surface by the relative-mean-motion is zero, then the rate of change in the total energy of relative-mean-motion within the space, is the difference of the integral, over the space, of the rate of increase of this energy by transformation from energy of mean-mean-motion, less the integral rate at which energy of relative-mean-motion is being converted into heat, or, integrating equation (19),

$$\iiint\left(\frac{d}{dt}+\bar{u}\frac{d}{dx}+\bar{v}\frac{d}{dy}+\bar{w}\frac{d}{dz}\right)\bar{E}'dxdydz$$

$$=-\iiint\left\{\begin{aligned}&\rho\overline{u'u'}\,\frac{d\bar{u}}{dx}+\rho\overline{u'v'}\,\frac{d\bar{u}}{dy}+\rho\overline{u'w'}\,\frac{d\bar{u}}{dz}\\&+\rho\overline{v'u'}\,\frac{d\bar{v}}{dx}+\rho\overline{v'v'}\,\frac{d\bar{v}}{dy}+\rho\overline{v'w'}\,\frac{d\bar{v}}{dz}\\&+\rho\overline{w'u'}\,\frac{d\bar{w}}{dx}+\rho\overline{w'v'}\,\frac{d\bar{w}}{dy}+\rho\overline{w'w'}\,\frac{d\bar{w}}{dz}\end{aligned}\right\}dxdydz$$

$$+\iiint\left\{\begin{aligned}&\overline{p'_{xx}\frac{du'}{dx}+p'_{yx}\frac{du'}{dy}+p'_{zx}\frac{du'}{dz}}\\&\overline{p'_{xy}\frac{dv'}{dx}+p'_{yy}\frac{dv'}{dy}+p'_{zy}\frac{dv'}{dz}}\\&\overline{p'_{xz}\frac{dw'}{dx}+p'_{yz}\frac{dw'}{dy}+p'_{zz}\frac{dw'}{dz}}\end{aligned}\right\}dxdydz \;\ldots\ldots\ldots\ldots(20).$$

This equation expresses the fundamental relations:—

(1) *That the only integral effect of the mean-mean-motion on the relative-mean-motion is the integral of the rate of transformation from energy of mean-mean-motion to energy of relative-mean-motion.*

(2) *That, unless relative energy is altered by actions across the surface within which the integration extends, the integral energy of relative-mean-motion will be increasing, or diminishing, according as the integral rate of transformation from mean-mean-motion to relative-mean-motion is greater, or less than, the rate of conversion of the energy of relative-mean-motion into heat.*

19. For p'_{xx}, &c., are substituted their values as determined according to the theory of viscosity, the approximate truth of which has been verified, as already explained.

Putting

$$\left.\begin{aligned} p'_{xx} &= p + \tfrac{2}{3}\mu\left(\frac{du'}{dx} + \frac{dv'}{dy} + \frac{dw'}{dz}\right) - 2\mu\frac{du}{dx}, \text{ \&c., \&c.} \\ p'_{yx} &= -\mu\left(\frac{du'}{dy} + \frac{dv'}{dx}\right) \text{ \&c., \&c.} \end{aligned}\right\} \quad \text{.........(21)},$$

we have, substituting in the last term of equation (20), as the expression for the rate of conversion of energy of relative-mean-motion into heat,

$$\begin{aligned} -\iiint \frac{d}{dt}(pH)\,dx\,dy\,dz = \iiint \Bigg[p\left(\frac{du'}{dx} + \frac{dv'}{dy} + \frac{dw'}{dz}\right) \\ -\mu\left\{-\tfrac{2}{3}\left(\frac{du'}{dx} + \frac{dv'}{dy} + \frac{dw'}{dz}\right)^2 + 2\left[\left(\frac{du'}{dx}\right)^2 + \left(\frac{dv'}{dy}\right)^2 + \left(\frac{dw'}{dz}\right)^2\right]\right. \\ \left. + \left(\frac{dw'}{dy} + \frac{dv'}{dz}\right)^2 + \left(\frac{du'}{dz} + \frac{dw'}{dx}\right)^2 + \left(\frac{dv'}{dx} + \frac{du'}{dy}\right)^2\right\}\Bigg] dx\,dy\,dz \text{ ......(22)}, \end{aligned}$$

in which μ is a function of temperature only; or since ρ is here considered as constant,

$$\begin{aligned} -\iiint \frac{d}{dt}(pH')\,dx\,dy\,dz = -\mu \iiint \left\{2\left[\left(\frac{du'}{dx}\right)^2 + \left(\frac{dv'}{dy}\right)^2 + \left(\frac{dw'}{dz}\right)^2\right] + \left(\frac{dw'}{dy} + \frac{dv'}{dx}\right)^2\right. \\ \left. + \left(\frac{du'}{dz} + \frac{dw'}{dx}\right)^2 + \left(\frac{dv'}{dx} + \frac{du'}{dy}\right)^2\right\} dx\,dy\,dz \text{...(23)}, \end{aligned}$$

whence substituting for the last term in equation (20) we have, if the energy of relative-mean-motion is maintained, neither increasing nor diminishing,

$$-\rho \iiint \left\{\begin{aligned} &\overline{u'u'}\frac{d\bar{u}}{dx} + \overline{u'v'}\frac{d\bar{u}}{dy} + \overline{u'w'}\frac{d\bar{u}}{dz} \\ &+ \overline{v'u'}\frac{d\bar{v}}{dx} + \overline{v'v'}\frac{d\bar{v}}{dy} + \overline{v'w'}\frac{d\bar{v}}{dz} \\ &+ \overline{w'u'}\frac{d\bar{w}}{dx} + \overline{w'v'}\frac{d\bar{w}}{dy} + \overline{w'w'}\frac{d\bar{w}}{dz} \end{aligned}\right\} dx\,dy\,dz$$

$$-\mu\iiint\left\{\begin{aligned}&2\left[\left(\frac{du'}{dx}\right)^2+\left(\frac{dv'}{dy}\right)^2+\left(\frac{dw'}{dz}\right)^2\right]\\&\quad+\left(\frac{dw'}{dy}+\frac{dv'}{dx}\right)^2+\left(\frac{du'}{dz}+\frac{dw'}{dx}\right)^2\\&\quad+\left(\frac{dv'}{dx}+\frac{du'}{dy}\right)^2\end{aligned}\right\}dx\,dy\,dz=0\ \dots(24),$$

which is a discriminating equation as to the conditions under which relative-mean-motion can be sustained.

20. Since this equation is homogeneous in respect to the component velocities of the relative-mean-motion, it at once appears that it is independent of the energy of relative-mean-motion divided by the ρ. So that if μ/ρ is constant, the condition it expresses depends only on the relation between variations of the mean-mean-motion and the directional, or angular, distribution of the relative-mean-motion, and on the squares and products of the space periods of the relative-mean-motion.

And since the second term expressing the rate of conversion of heat into energy of relative-mean-motion is always negative, it is seen at once that, whatsoever may be the distribution and angular distribution of the relative-mean-motion and the variations of the mean-mean-motion, this equation must give an inferior limit for the rates of variation of the components of mean-mean-motion, in terms of the limits to the periods of relative-mean-motion, and μ/ρ, within which the maintenance of relative-mean-motion is impossible. And that, so long as the limits to the periods of relative-mean-motion are not infinite, this inferior limit to the rates of variation of the mean-mean-motion will be greater than zero.

Thus the second conclusion of Art. 14, and the whole of the previous argument is verified, and the properties of matter which prevent the maintenance of mean-motion, with periods of the same order of magnitude as those of the heat-motion, are shown to be amongst those properties of matter which are included in the equations of motion of which the truth has been verified by experience.

The Cause of Transformation.

21. The transformation function, which appears in the equations of mean-energy of mean- and relative-mean-motion, does not indicate the cause of transformation, but only expresses a kinematical principle as to the effect of the variations of mean-mean-motion, and the distribution of relative-mean-motion. In order to determine the properties of matter and the mechanical principles on which the effect of the variations of the mean-mean-motion on the distribution and angular distribution of relative-mean-motion depends, it is necessary to go back to the equations (16) of relative-

momentum at a point; and even then the cause is only to be found by considering the effects of the actions which these equations express in detail. The determination of this cause, though it in no way affects the proofs of the existence of the criterion as deduced from the equations, may be the means of explaining what has been hitherto obscure in the connection between thermodynamics and the principles of mechanics. That such may be the case, is suggested by the recognition of the separate equations of mean- and relative-mean-motion of matter.

The Equation of Energy of Relative-mean-motion and the Equation of Thermodynamics.

22. On consideration, it will at once be seen that there is more than an accidental correspondence between the equations of energy of mean- and relative-mean-motion respectively, and the respective equations of energy of mean-motion and of heat in thermodynamics.

If instead of including only the effects of the heat-motion on the mean-momentum, as expressed by p_{xx}, &c., the effects of relative-mean-motion are also included by putting p_{xx} for $\bar{p}_{xx} + \rho\overline{u'u'}$, &c., and p_{yz} for $\bar{p}_{yz} + \rho\overline{w'v'}$, &c., in equations (15) and (17), the equations (15) of mean-mean-motion become identical in form with the equations (1) of mean-motion, and the equation (17) of energy of mean-mean-motion becomes identical in form with the equation (3) of energy of mean-motion.

These equations, obtained from (15) and (17), being equally true with equations (1) and (3), the mean-mean-motion in the former being taken over the space S_1 instead of S_0 as in the latter, then, instead of equation (9), we should have for the value of the last term—

$$p_{xx}\frac{d\bar{u}}{dx} + \&\text{c.}, = -\frac{d(pH)}{dt} + \rho\overline{u'u'}\frac{d\bar{u}}{dx} + \&\text{c.} \quad \ldots\ldots\ldots\ldots\ldots(25),$$

in which the right member expresses the rate at which heat is converted into energy of mean-mean-motion, together with the rate at which energy of relative-mean-motion is transformed into energy of mean-mean-motion; while equation (19) shows whence the transformed energy is derived.

The similarity of the parts taken by the transformation of mean-mean-motion into relative-mean-motion, and the conversion of mean-motion into heat, indicates that these parts are identical in form; or that the conversion of mean-motion into heat is the result of transformation, and is expressible by a transformation function similar in form to that for relative-mean-motion, but in which the components of relative-motion are the components of the heat-motions, and the density is the actual density at each point. Whence it would appear that the general equations, of which equations (19) and (16) are respectively the adaptations to the special condition of uniform density, must, by indicating the properties of matter involved, afford mechanical explanations of the law of universal dissipation of energy and of the second law of thermodynamics.

The proof of the existence of a criterion, as obtained from the equations, is quite independent of the properties and mechanical principles on which the effect of the variations of mean-mean-motion on the distribution of relative-mean-motion depends. And as the study of these properties and principles requires the inclusion of conditions which are not included in the equations of mean-motion of incompressible fluid, it does not come within the purpose of this paper. It is therefore reserved for separate investigation by a more general method.

The Criterion of Steady Mean-motion.

23. As already pointed out, it appears from the discriminating equation that the possibility of the maintenance of a state of relative-mean-motion depends on μ/ρ, the variation of mean-mean-motion, and the periods of the relative-mean-motion.

Thus, if the mean-mean-motion is in direction x only, and varies in direction y only, if u', v', w' are periodic in directions x, y, z, a being the largest period in space, so that their integrals over a distance a in direction x are zero, and if the co-efficients of all the periodic factors are α, then putting

$$\pm \frac{d\bar{u}}{dy} = C_1^2,$$

and taking the integrals, over the space a^3, of the 18 squares and products in the last term on the left of the discriminating equation (24) to be

$$-18\mu C_2 \left(\frac{2\pi}{a}\right)^2 \alpha^2 a^3,$$

the integral of the first term over the same space cannot be greater than

$$\rho C_2 \alpha^2 C_1^2 a^3.$$

Then, by the discriminating equation, if the mean-energy of relative-mean-motion is to be maintained,

$$\rho C_1^2 \text{ is greater than } 700 \,.\, \frac{\mu}{a^2},$$

or

$$\frac{\rho a^2}{\mu} \sqrt{\left(\frac{d\bar{u}}{dy}\right)^2} = 700 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(26)$$

is a condition under which relative-mean-motion cannot be maintained in a fluid, of which the mean-mean-motion is constant in the direction of mean-mean-motion, and subject to a uniform variation at right angles to the direction of mean-mean-motion. It is not the actual limit, to obtain which it would be necessary to determine the actual forms of the periodic function for u', v', w', which would satisfy the equations of motion (15), (16), as well as the equation of continuity (13), and to do this the functions would be of the form

$$\Sigma \left[A_r \cos\left\{r\left(nt + \frac{2\pi}{a}x\right)\right\}\right],$$

where r has the values 1, 2, 3, &c. It may be shown, however, that the retention of the terms in the periodic series in which r is greater than unity would increase the numerical value of the limit.

24. It thus appears that the existence of the condition (26) within which no relative-mean-motion, completely periodic in the distance a, can be maintained, is a proof of the existence, for the same variation of mean-mean-motion, of an actual limit of which the numerical value is between 700 and infinity.

In viscous fluids, experience shows that the further kinematical conditions imposed by the equations of motion do not prevent such relative-mean-motion. Hence for such fluids equation (26) proves that the actual limit, which discriminates between the possibility and impossibility of relative-mean-motion completely periodic in a space a, is greater than 700.

Putting equation (26) in the form

$$\sqrt{\left(\frac{d\bar{u}}{dy}\right)^2} = 700\,\frac{\mu}{\rho a^2},$$

it at once appears that this condition does not furnish a criterion as to the possibility of the maintenance of relative-mean-motion, irrespective of its periods, for a certain condition of variation of mean-mean-motion. For by taking a^2 large enough, such relative-mean-motion would be rendered possible whatever might be the variation of the mean-mean-motion.

The existence of a criterion is thus seen to depend on the existence of certain restrictions to the value of the periods of relative-mean-motion—on the existence of conditions which impose superior limits on the values of a.

Such limits to the maximum values of a may arise from various causes. If $d\bar{u}/dy$ is periodic, the period would impose such a limit, but the only restrictions which it is my purpose to consider in this paper, are those which arise from the solid surfaces between which the fluid flows. These restrictions are of two kinds—restrictions to the motions normal to the surfaces, and restrictions tangential to the surfaces—the former are easily defined, the latter depend for their definition on the evidence to be obtained from experiments such as those of Poiseuille, and I shall proceed to show that these restrictions impose a limit to the value of a, which is proportional to D, the dimension between the surfaces. In which case, if

$$\sqrt{\overline{\left(\frac{du}{dy}\right)^2}} = \frac{U}{D},$$

equation (26) affords a proof of the existence of a criterion

$$\rho \cdot \frac{DU}{\mu} = K \quad \text{.................................(27)}$$

of the conditions of mean-mean-motion under which relative or sinuous-motion can continuously exist in the case of a viscous fluid between two continuous surfaces perpendicular to the direction y, one of which is maintained at rest, and the other in uniform tangential-motion in the direction x with velocity U.

SECTION III.

The Criterion of the Conditions under which Relative-mean-motion cannot be maintained in the case of Incompressible Fluid in Uniform Symmetrical Mean-flow between Parallel Solid Surfaces.—Expression for the Resistance.

25. The only conditions, under which definite experimental evidence as to the value of the criterion has as yet been obtained, are those of steady flow through a straight round tube of uniform bore; and for this reason it would seem desirable to choose for theoretical application the case of a round tube. But inasmuch as the application of the theory is only carried to the point of affording a proof of the existence of an inferior limit to the value of the criterion, which shall be greater than a certain quantity determined by the density and viscosity of the fluid and the conditions of flow, and as the necessary expressions for the round tube are much more complex than those for parallel plane surfaces, the conditions here considered are those defined by such surfaces.

Case I. Conditions.

26. The fluid is of constant density ρ and viscosity μ, and is caused to flow, by a uniform variation of pressure $d\bar{p}/dx$, in direction x between parallel surfaces, given by

$$y = -b_0, \quad y = b_0 \quad \text{..............................(28)},$$

the surfaces being of indefinite extent in directions z and x.

The Boundary Conditions.

(1) There can be no motion normal to the solid surfaces, therefore

$$v = 0 \text{ when } y = \pm b_0 \text{ ....(29).}$$

(2) That there shall be no tangential motion at the surface, therefore

$$u = w = 0 \text{ when } y = \pm b_0 \text{ ....(30);}$$

whence by equation (21), putting u for u', $p_{yx} = -\mu du/dy$.

By the equation of continuity $du/dx + dv/dy + dw/dz = 0$, therefore at the boundaries we have the further conditions, that when $y = \pm b_0$,

$$du/dx = dv/dy = dw/dz = 0 \text{ ....(31).}$$

Singular Solution.

27. If the mean-motion is everywhere in direction x, then, by the equation of continuity, it is constant in this direction, and as shown (Art. 8) the periods of mean-motion are infinite, and the equations (1), (3), and (9) are strictly true. Hence if

$$\bar{v} = \bar{w} = u' = v' = w' = 0 \text{ ....(32),}$$

we have conditions under which a singular solution of the equations, applied to this case, is possible whatsoever may be the value of b_0, dp/dx, ρ and μ.

Substituting for p_{xx}, p_{yz}, &c., in equations (1) from equations (21), and substituting u for u', &c., these become

$$\rho \frac{du}{dt} = -\frac{dp}{dx} + \mu \left(\frac{d^2u}{dy^2} + \frac{d^2u}{dz^2}\right) \text{..........................(33).}$$

This equation does not admit of solution from a state of rest*; but assuming a condition of steady motion such that du/dt is everywhere zero, and dp/dx constant, the solution of

* In a paper on the "Equations of Motion and the Boundary Conditions of Viscous Fluid," read before Section A at the meeting of the B. A., 1883, I pointed out the significance of this disability to be integrated, as indicating the necessity of the retention of terms of higher orders to complete the equations, and advanced certain confirmatory evidence as deduced from the theory of gases. The paper was not published, as I hoped to be able to obtain evidence of a more definite character, such as that which is now adduced in Articles 7 and 8 of this paper, which shows that the equations are incomplete, except for steady motion, and that to render them integrable from rest the terms of higher orders must be retained, and thus confirms the argument I advanced, and completely explains the anomaly. (See Paper 46, page 132.)

$$\left.\begin{aligned} &\frac{\mu}{\rho}\left(\frac{d^2u}{dy^2}+\frac{d^2u}{dz^2}\right)-\frac{1}{\rho}\frac{dp}{dx}=0, \\ \text{if}\qquad &u=du/dz=0 \text{ when } y=\pm b_0, \\ \text{is}\qquad &u=\frac{1}{\mu}\frac{dp}{dx}\frac{y^2-b_0^2}{2} \end{aligned}\right\}\qquad (34).$$

This is a possible condition of steady motion, in which the periods of u, according to Art. 8, are infinite; so that the equations for mean-motion as affected by heat-motion, by Art. 8, are exact, whatever may be the values of

$$u,\ b_0,\ \rho,\ \mu, \text{ and } dp/dx.$$

The last of equations (34) is thus seen to be a singular solution of the equations (15) for steady mean-flow, or steady mean-mean-motion, when u', v', w', p', &c., have severally the values zero, and so the equations (16) of relative-mean-motion are identically satisfied.

In order to distinguish the singular values of u, I put

$$\left.\begin{aligned} &u=U, \quad \int_{-b}^{b} u\,dy = 2b_0U_m; \\ \text{whence}\qquad &\frac{dp}{dx}=-\frac{3\mu}{b_0^2}U_m, \quad U=\frac{3}{2}U_m\frac{b_0^2-y^2}{b_0^2} \end{aligned}\right\}\qquad (35).$$

According to the equations, such a singular solution is always possible where the conditions can be realized, but the manner in which this solution of the equation (1) of mean-motion is obtained affords no indication as to whether or not it is the only solution—as to whether or not the conditions can be realized. This can only be ascertained either by comparing the results as given by such solutions with the results obtained by experiment, or by observing the manner of motion of the fluid, as in my experiments with colour bands.

The fact that these conditions are realized, under certain circumstances, has afforded the only means of verifying the truth of the assumptions as to the boundary conditions, that there shall be no slipping, and as to μ being independent of the variations of mean-motion.

Verification of the Assumptions in the Equation of Viscous Fluid.

28. As applied to the conditions of Poiseuille's experiments and similar experiments made since, the results obtained from the theory are found to agree throughout the entire range so long as u', v', w' are zero, showing that if there were any slipping it must have been less than the thousandth part of the mean-flow, although the tangential force at the boundary was 0·2 gr.

per square centimetre, or over 6 lbs. per square foot, the mean flow 376 millims. (1·23 feet) per second, and

$$d\bar{u}/dr = 215{,}000,$$

the diameter of this tube being 0·014 millim., the length 1·25 millims., and the head 30 inches of mercury.

Considering that the skin resistance of a steamer going at 25 knots is not 6 lbs. per square foot, it appears that the assumptions, as to the boundary conditions and the constancy of μ, have been verified under more exigent circumstances, both as regards tangential resistance and rate of variation of tangential stress, than occur in anything but exceptional cases.

Evidence that other Solutions are possible.

29. The fact that steady mean-motion is almost confined to capillary tubes, and that in larger tubes, except when the motion is almost insensibly slow, the mean-motion is sinuous and full of eddies, is abundant evidence of the possibility, under certain conditions, of solutions other than the singular solutions.

In such solutions u', v', w' have values, which are maintained, not as a system of steady periodic motion, but such as has a steady effect on the mean-flow through the tube; and equations (1) are only approximately true.

The Application of the Equations of the Mean- and Relative-mean-motion.

30. Since the components of mean-mean-motion in directions y and z are zero, and the mean flow is steady,

$$\bar{v} = 0, \quad \bar{w} = 0, \quad d\bar{u}/dt = 0, \quad d\bar{u}/dx = 0 \qquad \text{(36)},$$

and as the mean values of functions of u', v', w' are constant in the direction of flow,

$$\frac{d\,(\overline{u'u'})}{dx} = 0, \quad \frac{d\,(\overline{v'u'})}{dx} = 0, \quad \frac{d\,(\overline{w'u'})}{dx} = 0, \text{ \&c.} \qquad \text{(37)}.$$

By equations (21) and (37) the equations (15) of mean-motion become

$$\left.\begin{aligned}
\rho\frac{d\bar{u}}{dt} &= -\frac{dp}{dx} + \mu\left(\frac{d^2\bar{u}}{dy^2} + \frac{d^2\bar{u}}{dz^2}\right) - \rho\left\{\frac{d}{dy}(\overline{u'v'}) + \frac{d}{dz}(\overline{u'w'})\right\}\\
\rho\frac{d\bar{v}}{dt} &= -\frac{dp}{dy} \qquad\qquad\qquad\qquad - \rho\left\{\frac{d}{dy}(\overline{v'v'}) + \frac{d}{dz}(\overline{v'w'})\right\}\\
\rho\frac{d\bar{w}}{dt} &= -\frac{dp}{dz} \qquad\qquad\qquad\qquad - \rho\left\{\frac{d}{dy}(\overline{w'v'}) + \frac{d}{dz}(\overline{w'w'})\right\}
\end{aligned}\right\} \qquad \text{(38)}.$$

The equation of energy of mean-mean-motion (17) becomes

$$\left.\begin{aligned}\frac{d(\bar{E})}{dt} = -\bar{u}\frac{d\bar{p}}{dx} + \mu\left\{\frac{d}{dy}\left(\bar{u}\frac{d\bar{u}}{dy}\right) + \frac{d}{dz}\left(\bar{u}\frac{d\bar{u}}{dz}\right)\right\}\\ -\rho\left\{\frac{d}{dy}(\bar{u}\,\overline{u'v'}) + \frac{d}{dz}(\bar{u}\,\overline{u'w'})\right\} - \mu\left\{\left(\frac{d\bar{u}}{dy}\right)^2 + \left(\frac{d\bar{u}}{dz}\right)^2\right\}\\ +\rho\left\{\overline{u'v'}\frac{d\bar{u}}{dy} + \overline{u'w'}\frac{d\bar{u}}{dz}\right\}\end{aligned}\right\} \quad \ldots\ldots(39).$$

Similarly the equation of mean-energy of relative-mean-motion (19) becomes

$$\begin{aligned}\frac{d\bar{E}'}{dt} = &-\frac{d}{dy}\left[u'(p'_{yx} + \rho\overline{u'v'}) + v'(p'_{yy} + \rho\overline{v'v'}) + w'(p'_{yz} + \rho\overline{w'v'})\right]\\ &-\frac{d}{dz}\left[u'(p'_{zx} + \rho\overline{u'w'}) + v'(p'_{zy} + \rho\overline{v'w'}) + w'(p_{zz} + \rho\overline{w'w'})\right]\\ &-\mu\left[2\left\{\left(\frac{du'}{dx}\right)^2 + \left(\frac{dv'}{dy}\right)^2 + \left(\frac{dw'}{dz}\right)^2\right\} + \left(\frac{dw'}{dy} + \frac{dv'}{dz}\right)^2\right.\\ &\qquad\left. + \left(\frac{du'}{dz} + \frac{dw'}{dx}\right)^2 + \left(\frac{dv'}{dx} + \frac{du'}{dz}\right)^2\right]\\ &-\rho\left\{\overline{u'v'}\frac{d\bar{u}}{dy} + \overline{u'w'}\frac{d\bar{u}}{dz}\right\} \quad \ldots\ldots(40).\end{aligned}$$

Integrating in directions y and z between the boundaries and taking note of the boundary conditions by which u, u', v', w' vanish at the boundaries together with the integrals, in direction z, of

$$\frac{d}{dz}\left(\bar{u}\frac{d\bar{u}}{dz}\right),\quad \frac{d}{dz}\left[\bar{u}'\,\rho\overline{u'w'})\right],\quad \frac{d}{dz}\left[u'(p_{zx} + \rho\overline{u'w'})\right], \text{ \&c.},$$

the integral equation of energy of mean-mean-motion becomes

$$\begin{aligned}\iint\frac{d\bar{E}}{dt}dy\,dz = -\iint\Bigg[\bar{u}\frac{dp}{dx} + \mu\left\{\left(\frac{d\bar{u}}{dy}\right)^2 + \left(\frac{d\bar{u}}{dz}\right)^2\right\}\\ -\rho\left\{\overline{u'v'}\frac{d\bar{u}}{dy} + \overline{u'w'}\frac{d\bar{u}}{dz}\right\}\Bigg]dy\,dz \quad \ldots\ldots(41).\end{aligned}$$

The integral equation of energy of relative-mean-motion becomes

$$\begin{aligned}\iint\frac{d\bar{E}'}{dt}dy\,dz = &-\iint\left[\rho\left\{\overline{u'v'}\frac{d\bar{u}}{dy} + \overline{u'w'}\frac{d\bar{u}}{dz}\right\}\right]dy\,dz\\ &-\mu\iint\Bigg[2\left(\frac{du'}{dx}\right)^2 + \left(\frac{dv'}{dy}\right)^2 + \left(\frac{dw'}{dz}\right)^2\\ &+\left(\frac{dw'}{dy} + \frac{dv'}{dz}\right)^2 + \left(\frac{du'}{dz} + \frac{dw'}{dx}\right)^2 + \left(\frac{dv'}{dx} + \frac{du'}{dy}\right)^2\Bigg]dy\,dz \quad \ldots\ldots(42).\end{aligned}$$

If the mean-mean-motion is steady it appears from equation (41) that

$$-\iint \bar{u}\,\frac{\overline{dp}}{dx}\,dy\,dz,$$

the work done on the mean-mean-motion $\bar{u}$, per unit of length of the tube, by the constant variation of pressure, is in part transformed into energy of relative-mean-motion at a rate expressed by the transformation function:

$$-\iint \rho\left(\overline{u'v'}\,\frac{\overline{du}}{dy} + \overline{u'w'}\,\frac{\overline{dw}}{dy}\right)dy\,dz,$$

and in part transformed into heat at the rate:

$$\mu\iint\left[\left(\frac{\overline{du}}{dy}\right)^2 + \left(\frac{\overline{du}}{dz}\right)^2\right]dy\,dz.$$

While the equation (42) for the integral energy of relative-mean-motion shows that the only energy received by the relative-mean-motion is that transformed from mean-mean-motion, and the only energy lost by relative-mean-motion is that converted into heat by the relative-mean-motion at the rate expressed by the last term.

And hence if the integral of E' is maintained constant, the rate of transformation from energy of mean-mean-motion must be equal to the rate at which energy of relative-mean-motion is converted into heat, and the discriminating equation becomes

$$\iint \rho\left(\overline{u'v'}\,\frac{\overline{du}}{dy} + \overline{u'w'}\,\frac{\overline{du}}{dz}\right)dy\,dz = -\mu\iint\left[2\left\{\left(\frac{du'}{dx}\right)^2 + \left(\frac{dv'}{dy}\right)^2 + \left(\frac{dw'}{dz}\right)^2\right\}\right.$$
$$\left. + \left(\frac{dw'}{dy} + \frac{dv'}{dz}\right)^2 + \left(\frac{du'}{dz} + \frac{dw'}{dx}\right)^2 + \left(\frac{dv'}{dx} + \frac{du'}{dy}\right)^2\right]dy\,dz \ldots\ldots(43).$$

The Conditions to be Satisfied by $\bar{u}$ and u', v', w'.

31. If the mean-mean-motion is steady $\bar{u}$ must satisfy :—

(1) The boundary conditions

$$\bar{u} = 0 \text{ when } y = \pm b_0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(44);$$

(2) The equation of continuity

$$d\bar{u}/dx = 0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(45);$$

(3) The first of the equations of motion (38)

$$\frac{\overline{dp}}{dx} = \mu\left(\frac{d^2\bar{u}}{dy^2} + \frac{d^2\bar{u}}{dz^2}\right) - \rho\left\{\frac{d}{dy}(\overline{u'v'}) + \frac{d}{dz}(\overline{u'w'})\right\}\ldots\ldots\ldots\ldots(46);$$

or putting $$\bar{u} = U + \bar{u} - U,$$

and $$\frac{dp}{dx} = \mu \frac{d^2 U}{dy^2} \text{ as in the singular solution,}$$

equation (46) becomes

$$\mu \left(\frac{d^2(\bar{u} - U)}{dy^2} + \frac{d^2(\bar{u} - U)}{dz^2} \right) = \rho \left\{ \frac{d}{dy} (\overline{u'v'}) + \frac{d}{dz} (\overline{u'w'}) \right\} \ldots\ldots(47);$$

(4) The integral of (47) over the section of which the left member is zero, and

$$\text{the mean value of } \mu d\bar{u}/dy = \mu dU/dy \text{ when } y = \pm b_0 \ldots\ldots(48).$$

From the condition (3) it follows that if $\bar{u}$ is to be symmetrical with respect to the boundary surfaces, the relative-mean-motion must extend throughout the tube, so that

$$\int_{-\infty}^{\infty} \left[\frac{d}{dy} (\overline{u'v'}) + \frac{d}{dz} (\overline{u'w'}) \right] dz \text{ is a function of } y^2 \ldots\ldots\ldots(49).$$

And as this condition is necessary, in order that the equations (38) of mean-mean-motion and the equations (16) of relative-mean-motion may be satisfied for steady mean-motion, it is assumed as one of the conditions for which the criterion is sought.

The components of relative-mean-motion must satisfy the periodic conditions as expressed in equations (12), which become, putting $2c$ for the limit in direction z,

(1)
$$\left. \begin{array}{l} \int_0^a u' dx = \int_0^a v' dx = \int_0^a w' dx = 0 \\ \int_{-b_0}^{b_0} \int_{-c}^{c} u' dy dz = 0 \end{array} \right\} \ldots\ldots\ldots\ldots\ldots\ldots(50).$$

(2) The equation of continuity

$$du'/dx + dv'/dy + dw'/dz = 0.$$

(3) The boundary conditions which with the equation of continuity give

$$u' = v' = w' = du'/dx = dv'/dy = dw'/dz = 0 \text{ when } y = \pm b_0 \ldots\ldots(51).$$

(4) The condition imposed by symmetrical mean-motion

$$\int_{-c}^{c} \left[\frac{d}{dy} (\overline{u'v'}) + \frac{d}{dz} (\overline{u'w'}) \right] dz = 2c \,.\, f(y^2) \ \ldots\ldots\ldots\ldots(52).$$

These conditions (1 to 4) must be satisfied, if the effect on $\bar{u}$ is to be symmetrical however arbitrarily u', v', w' may be superimposed on the mean-motion which results from a singular solution.

(5) If the mean-motion is to remain steady u', v', w' must also satisfy the kinematical conditions obtained by eliminating $\bar{p}$ from the equations of mean-mean-motion (38) and those obtained by eliminating p' from the equations of relative-mean-motion (16).

Conditions (1 to 4) determine an inferior Limit to the Criterion.

32. The determination of the kinematic conditions (5) is, however, practically impossible; but if they are satisfied, u', v', w' must satisfy the more general conditions imposed by the discriminating equation. From which it appears that when u', v', w' are such as satisfy the conditions (1 to 4), however small their values relative to u may be, if they be such that the rate of conversion of energy of relative-mean-motion into heat is greater than the rate of transformation of energy of mean-mean-motion into relative-mean-motion, the energy of relative-mean-motion must be diminishing. Whence, when u', v', w' are taken such periodic functions of x, y, z, as under conditions (1 to 4) render the value of the transformation function relative to the value of the conversion function a maximum, if this ratio is less than unity, the maintenance of any relative-mean-motion is impossible. And whatever further restrictions might be imposed by the kinematical conditions, the existence of an inferior limit to the criterion is proved.

Expressions for the Components of possible Relative-mean-motion.

33. To satisfy the first three of the equations (50) the expressions for u', v', w', must be continuous periodic functions of x, with a maximum periodic distance a, such as satisfy the conditions of continuity.

Putting

$$l = 2\pi/a; \text{ and } n \text{ for any number from 1 to } \infty,$$

and

$$\left.\begin{aligned} u' &= \Sigma_0^\infty \left\{\left(\frac{d\alpha_n}{dy} + \frac{d\gamma_n}{dz}\right)\cos(nlx) + \left(\frac{d\beta_n}{dy} + \frac{d\delta_n}{dz}\right)\sin(nlx)\right\} \\ v' &= \Sigma_0^\infty \{nl\alpha_n \sin(nlx) - nl\beta_n \cos(nlx)\} \\ w' &= \Sigma_0^\infty \{nl\gamma_n \sin(nlx) - nl\delta_n \cos(nlx)\} \end{aligned}\right\} \quad \text{......(53)},$$

u', v', w' satisfy the equation of continuity. And, if

$$\left.\begin{aligned} &\alpha = \beta = \gamma = \delta = d\alpha/dy = d\beta/dy = d\gamma/dz = d\delta/dz = 0 \text{ when } y = \pm b_0 \\ &\text{and } \alpha\beta,\ \alpha\gamma,\ \alpha\delta \text{ are all functions of } y^2 \text{ only,} \end{aligned}\right\} \text{......(54)},$$

it would seem that the expressions are the most general possible for the components of relative-mean-motion.

Cylindrical-relative-motion.

34. If the relative-mean-motion, like the mean-mean-motion, is restricted to motion parallel to the plane of xy,

$$\gamma = \delta = w' = 0, \text{ everywhere} \quad \ldots\ldots\ldots\ldots\ldots\ldots(55),$$

and the equations (53) express the most general forms for u', v' in case of such cylindrical disturbance.

Such a restriction is perfectly arbitrary, and having regard to the kinematical restrictions, over and above those contained in the discriminating equation, would entirely change the character of the problem. But as no account of these extra kinematical restrictions is taken in determining the limit to the criterion, and as it appears from trial that the value found for this limit is essentially the same, whether the relative-mean-motion is general or cylindrical, I only give here the considerably simpler analyses for the cylindrical motion.

The functions of Transformation of Energy and Conversion to Heat for Cylindrical Motion.

35. Putting $\dfrac{d}{dt}({}_pH')$ for the rate at which energy of relative-mean-motion is converted to heat per unit of volume, expressed in the right-hand member of the discriminating equation (43),

$$\iiint \frac{d}{dt}({}_pH')\, dx\,dy\,dz$$
$$= \mu \iiint \left[2\left\{ \left(\frac{du'}{dx}\right)^2 + \left(\frac{dv'}{dy}\right)^2 \right\} + \left(\frac{du'}{dy}\right)^2 + \left(\frac{dv'}{dx}\right)^2 + 2\frac{du'}{dy}\frac{dv'}{dx} \right] dx\,dy\,dz \ldots\ldots(56).$$

Then substituting for the values of u', v', w' from equations (53), and integrating in direction x over $2\pi/l$, and omitting terms the integral of which, in direction y, vanishes by the boundary conditions,

$$\iint \frac{d}{dt}({}_pH')\, dy\,dz = \frac{\mu}{2}\iint \Sigma \left\{ (nl)^4 (\alpha_n^2 + \beta_n^2) + 2(nl)^2 \left[\left(\frac{d\alpha_n}{dy}\right)^2 + \left(\frac{d\beta_n}{dy}\right)^2 \right] \right.$$
$$\left. + \left(\frac{d^2\alpha_n}{dy^2}\right)^2 + \left(\frac{d^2\beta_n}{dy^2}\right)^2 \right\} dy\,dz \ldots\ldots(57).$$

In a similar manner, substituting for u', v', integrating, and omitting terms which vanish on integration, the rate of transformation of energy from mean-mean-motion, as expressed by the left member in the discriminating equation (43), becomes

$$\iint \rho \overline{u'v'} \frac{d\bar{u}}{dy} dy dz = \tfrac{1}{2} \iint \Sigma \left[nl \left(\alpha_n \frac{d\beta_n}{dy} - \beta_n \frac{d\alpha_n}{dy} \right) \frac{d\bar{u}}{dy} \right] dy dz \text{...(58)}.$$

And, since by Art. 31, conditions (3) equation (47),

$$\mu \frac{d^2}{dy^2} (\bar{u} - U) = \rho \frac{d}{dy} (\overline{u'v'}) \text{..........................(59)},$$

integrating and remembering the boundary conditions,

$$\mu \frac{d}{dy} (\bar{u} - U) = \rho \overline{u'v'}, \quad \mu (\bar{u} - U) = \rho \int_{-b_0}^{y} \overline{u'v'} dy \text{.........(60)}.$$

And since at the boundary $\bar{u} - U$ is zero,

$$\rho \int_{-b_0}^{b_0} (u'v') dy = 0 \text{ ....(61)}.$$

Whence, putting $U + \bar{u} - U$ for $\bar{u}$ in the right member of equation (58), substituting for $\bar{u} - U$ from (60), integrating by parts, and remembering that

$$\frac{d^2U}{dy^2} = -3 \frac{U_m}{b_0^2}, \text{ which is constant ....(62)},$$

also that

$$u'v' = \tfrac{1}{2} \Sigma \left\{ nl \left(\alpha_n \frac{d\beta_n}{dy} - \beta_n \frac{d\alpha_n}{dy} \right) \right\} \text{ ....(63)},$$

we have for the transformation function:

$$\int_{-b_0}^{b_0} \left(\rho \overline{u'v'} \frac{du}{dy} \right) dy = \Sigma \left[\tfrac{3}{2} \rho \frac{U_m}{b_0^2} \int_{-b_0}^{b_0} dy \int_{-b_0}^{y} nl \left(\alpha_n \frac{d\beta_n}{dy} - \beta_n \frac{d\alpha_n}{dy} \right) dy \right.$$

$$\left. + \frac{\rho^2}{4\mu} \int_{-b_0}^{b} (nl)^2 \left(\alpha_n \frac{d\beta_n}{dy} - \beta_n \frac{d\alpha_n}{dy} \right)^2 dy \right] \text{......(64)}.$$

If u', v' are indefinitely small, the last term, which is of the fourth degree, may be neglected.

Substituting in the discriminating equation (43) this may be put in the form

$$\frac{2\rho b_0 U_m}{\mu} = \frac{2b_0^3 \int_{-b_0}^{b_0} \Sigma \left\{ n^4 l^4 (\alpha_n^2 + \beta_n^2) + 2n^2 l^2 \left[\left(\frac{d\alpha_n}{dy} \right)^2 + \left(\frac{d\beta_n}{dy} \right)^2 \right] + \left(\frac{d^2\alpha_n}{dy^2} \right)^2 + \left(\frac{d^2\beta_n}{dy^2} \right)^2 \right\} dy}{3 \int_{-b_0}^{b_0} dy \int_{-b_0}^{y} \Sigma \left\{ nl \left(\beta_n \frac{d\alpha_n}{dy} - \alpha_n \frac{d\beta_n}{dy} \right) \right\} dy}$$

......(65).

Limits to the Periods.

36. As functions of y, the variations of α_n, β_n are subject to the restrictions imposed by the boundary conditions, and in consequence their periodic distances are subject to superior limits determined by $2b_0$, the distance between the fixed surfaces.

In direction x, however, there is no such direct connection between the value of b_0 and the limits to the periodic distance, as expressed by $2\pi/nl$. Such limits necessarily exist, and are related to the limits of α_n and β_n in consequence of the kinematical conditions necessary to satisfy the equations of motion for steady mean-mean-motion; these relations, however, cannot be exactly determined without obtaining a general solution of the equations.

But from the form of the discriminating equation (43) it appears that no such exact determination is necessary in order to prove the inferior limit to the criterion.

The boundaries impose the same limits on α_n, β_n whatever may be the value of nl; so that if the values of α_n, β_n be determined so that the value of

$$\frac{2\rho b_0 U_m}{\mu} \text{ is a minimum}$$

for every value of nl, the value of rl, which renders this minimum a minimum-minimum may then be determined, and so a limit found to which the value of the complete expression approaches, as the series in both numerator and denominator become more convergent for values of nl differing in both directions from rl.

Putting l, α, β for rl, α_r, β_r respectively, and putting for the limiting value to be found for the criterion

$$K_1 = \frac{2\rho b_0 U_m}{\mu} \qquad \text{.................................(66)}$$

$$\frac{3}{2}K_1 = b_0^3 \frac{\int_{-b_0}^{b_0}\left\{l^4(\alpha^2+\beta^2) + 2l^2\left[\left(\frac{d\alpha}{dy}\right)^2 + \left(\frac{d\beta}{dy}\right)^2\right] + \left(\frac{d^2\alpha}{dy}\right)^2 + \left(\frac{d^2\beta}{dy}\right)^2\right\}dy}{l\int_{-b_0}^{b} dy \int_{-b_0}^{y}\left(\beta\frac{d\alpha}{dy} - \alpha\frac{d\beta}{dy}\right)dy} \quad \text{...(67)}$$

where α and β are such functions of y that K_1 is a minimum whatever the value of l, and l is so determined as to render K_1 a minimum-minimum.

Having regard to the boundary conditions, &c., and omitting all possible terms which increase the numerator without affecting the denominator, the most general form appears to be

$$\alpha = \Sigma_1^\infty [a_{2r-1} \sin (2r-1) p], \qquad \beta = \Sigma_1^\infty [b_{2r} \sin 2rp], \qquad \text{.........(68)}$$

where $\qquad p = \pi y / 2b_1.$

To satisfy the boundary conditions [illegible]

Since $\alpha = 0$, when $p = \pm \frac{1}{2}\pi$,

$$\Sigma_1^\infty (a_{4r-3} - a_{4r-1}) = 0$$

and since $d\beta/dy = 0$, when $p = \pm \frac{1}{2}\pi$,

$$\Sigma_1^\infty [-(4r-2) b_{4r-2} + 4r\, b_{4r}] = 0 \qquad \text{.........(69)}.$$

From the form of K_1 it is clear that every term in the series for α and β increases the value of K_1 and to an extent depending on the value of r. K_1 will therefore be a minimum, when

$$\alpha = a_1 \sin p + a_3 \sin 3p, \qquad \beta = b_2 \sin 2p + b_4 \sin 4p \qquad \text{.........(70)},$$

which satisfy the boundary conditions if

$$a_3 = a_1, \qquad b_4 = [illegible] b_2 \qquad \text{.........(71)}.$$

Therefore we have, as the values of α and β which render K_1 a minimum for any value of r,

$$\alpha / a_1 = \sin p + \sin 3p, \qquad \beta / b_2 = \sin 2p - \tfrac{1}{2} \sin 4p.$$

And

$$\frac{2b_1}{\pi a_1} \frac{d\alpha}{dy} = \cos p + 3 \cos 3p, \qquad \frac{2b_1}{\pi b_2} \frac{d\beta}{dy} = 2 \cos 2p - 2 \cos 4p \qquad \text{.........(72)}.$$

$$\frac{2b_1}{\pi a_1 b_2} \left(\alpha \frac{d\beta}{dy} - \beta \frac{d\alpha}{dy} \right) = \frac{1}{4} (-3 \sin p - 3 \sin 3p - \sin 5p - \sin 7p)$$

and integrating twice

$$l \int_{-b_1}^{b_1} dy \int_{-b_1}^{y} \left(\beta \frac{d\alpha}{dy} - \alpha \frac{d\beta}{dy} \right) dy = -1{\cdot}325\, l \frac{2b_1}{\pi} a_1 b_2 \qquad \text{.........(73)}.$$

Putting $\qquad \dfrac{\pi}{2b_1} L$ for l,

the denominator of $\frac{3}{2} K_1$, equation (67), becomes

$$-1{\cdot}325\, L a_1 b_2.$$

In a similar manner the numerator is found to be

$$b_0^4\left(\frac{\pi}{2b_0}\right)^4\{L^4(2a_1^2+1\cdot25b_2^2)+2L^2(10a_1^2+8b_2^2)+82a_1^2+80b_2^2\},$$

and as the coefficients of a_1 and b_2 are nearly equal in the numerator, no sensible error will be introduced by putting

$$b_2=-a_1,$$

then

$$\frac{3}{2}K_1=\frac{L^4+2\times5\cdot53L^2+50}{0\cdot408L}\left(\frac{\pi}{2}\right)^4 \quad\text{..................(74)}$$

which is a minimum if

$$L=1\cdot62 \quad\text{.................................(75)}$$

and

$$K_1=517 \quad\text{.................................(76).}$$

Hence, for a flat tube of unlimited breadth, the criterion

$$\rho 2b_0U_m/\mu \text{ is greater than } 517 \quad\text{.....................(77).}$$

37. This value must be less than that of the criterion for similar circumstances. How much less it is impossible to determine theoretically without effecting a general solution of the equations; and, as far as I am aware, no experiments have been made in a flat tube. Nor can the experimental value 1900, which I obtained for the round tube, be taken as indicative of the value for a flat tube, except that, both theoretically and practically, the critical value of U_m is found to vary inversely as the hydraulic mean depth, which would indicate that, as the hydraulic mean depth in a flat tube is double that for a round tube, the criterion would be half the value, in which case the limit found for K_1 would be about $0\cdot61K$. This is sufficient to show that the absolute theoretical limit found is of the same order of magnitude as the experimental value; so that the latter verifies the theory, which, in its turn, affords an explanation of the observed facts.

The State of Steady Mean-motion above the Critical Value.

38. In order to arrive at the limit for the criterion it has been necessary to consider the smallest values of u', v', w', and the terms in the discriminating equation of the fourth degree have been neglected. This, however, is only necessary for the limit, and, preserving these higher terms, the discriminating equation affords an expression for the resistance in the case of steady mean-mean-motion.

The complete value of the function of transformation as given in equation (64) is

$$\int_{-b_0}^{b_0}\left(\overline{\rho u'v'}\frac{d\bar{u}}{dy}\right)=\Sigma\left[\rho\frac{3U_m}{2b_0^2}\int_{-b_0}^{b_0}dy\int_{-b_0}^{y}nl\left(\alpha_n\frac{d\beta_n}{dy}-\beta_n\frac{d\alpha_n}{dz}\right)dy\right.$$

$$\left.+\frac{\rho^2}{4\mu}\int_{-b_0}^{b_0}(nl)^2\left(\alpha_n\frac{d\beta_n}{dy}-\beta\frac{d\alpha}{dy}\right)^2dy\right] \quad\text{......(77}a\text{).}$$

Whence putting $U+\bar{u}-U$, for $\bar{u}$ in the left member of equation (77), and integrating by parts, remembering the conditions, this member becomes

$$\frac{3U_m}{b_0^2}\int_{-b_0}^{b_0}dy\int_{-b_0}^{y}\rho\overline{u'v'}dy+\frac{\rho^2}{\mu}\int_{-b_0}^{b_0}(u'v')^2dy \quad\text{..............(78),}$$

in which the first term corresponds with the first term in the right member of equation (64), which was all that was retained for the criterion, and the second term corresponds with the second term in equation (64), which was neglected.

Since by equation (35)

$$\frac{3U_m}{b_0^2}=-\frac{1}{\mu}\frac{dp}{dx} \quad\text{..............................(78}a\text{),}$$

we have, substituting in the discriminating equation (43), either

$$-\frac{2}{3}\rho\frac{b_0^3}{\mu^2}\frac{dp}{dx}=\frac{2b_0^3}{3}\left.\frac{\left(\int\frac{d/dt\,(pH')\,dy}{\mu}+\frac{\rho^2}{\mu^2}\int_{-b_0}^{b_0}(\overline{u'v'})^2\,dy\right)}{-\int_{-b_0}^{b_0}dy\int_{-b_0}^{y}\overline{u'v'}\,dy}\right\} \quad\text{......(79),}$$

or

$$\mu\frac{d^2\bar{u}}{dy^2}-\frac{dp}{dx}=0 \quad\text{..............................(80).}$$

Therefore, as long as $$\frac{2}{3}\rho\frac{b_0^3}{\mu^2}\frac{dp}{dx}$$

is of constant value, there is dynamical similarity under geometrically similar circumstances.

The equation (79) shows that,

when $-\frac{2}{3}\rho\frac{b_0^3}{\mu^2}\frac{dp}{dx}$ is greater than K,

$\overline{u'v'}$ must be finite, and such that the last term in the numerator limits the rate of transformation, and thus prevents further increase of $\overline{u'v'}$.

The last term in the numerator of equation (79) is of the order and degree

$$\rho^2 L^4 \alpha^4/\mu^2 \text{ as compared with } L^4\alpha^2,$$

the order and degree of $\frac{1}{\mu}\frac{d}{dt}(pH')$ the first term in the numerator.

It is thus easy to see how the limit comes in. It is also seen from equation (79) that, above the critical value, the law of resistance is very complex and difficult of interpretation, except in so far as showing that the resistance varies as a power of the velocity higher than the first.

63.

EXPERIMENTS SHOWING THE BOILING OF WATER IN AN OPEN TUBE AT ORDINARY TEMPERATURES.

(*Exhibited before Section A, Brit. Assoc.*, 1894, *at Oxford.*)

AMONG the many phenomena, the secrets of which have been preserved by the deadening influence of familiarity on curiosity, there is perhaps none more remarkable than that of the 'singing of the kettle on the hob,' which has many times been the subject of sentiment and verse but not, it would seem, hitherto a subject of physical study which like the study of the rainbow might afford evidence as to the conditions under which we exist.

That the cheering evidence of the readiness of the social gathering is not the only evidence to be obtained from the song of the kettle will in the first place be demonstrated in these experiments. Thus, having analyzed by experiment the physical causes of this sound and its variations, the purpose of the experiments is to demonstrate the relation which exists between sounds in the kettle and sounds produced by the motion of water, or any liquid, under certain common conditions. And, in the third place, to demonstrate the general fact that liquids flowing between fixed boundaries emit no sound as long as they continuously occupy the space between the boundaries, and thence to demonstrate that when such sound occurs it is evidence of the boiling of the water.

If we place a kettle on the top of a fire, the first evidence of action is that of a somewhat feeble and intermittent hissing sound which at first increases and becomes continuous and then again subsides as the temperature increases.

This is followed by a much more definite and harsher sound which comes on suddenly, somewhat increases in volume, then suddenly softens and is immediately followed by the exit of steam showing that the water is boiling.

If a glass flask is substituted for the opaque kettle the causes of the sound and its variations become apparent.

The water in the flask is under the pressure of the atmosphere at its upper surface, which pressure is increased at points below the surface by the water above; so that the boiling-point at the bottom of the kettle is somewhat above that higher up.

The water receives its heat from the fire below by conduction through the metal, or glass, and the water between the bottom and the point considered.

The conduction through water is very slow; so that the water in immediate contact with the hot surface at the bottom becomes much hotter than the water immediately above. Water expands with heat. Hence this hot layer on the bottom is in unstable equilibrium, and vertical convection currents are set up which carry the hot water from the bottom into the colder water above. Owing however to the eddying motion which is a consequence of the resistance offered to the ascending currents by the water above, these currents do not follow a straight course but, somewhat rapidly, interweave, as thin sheets, with the surrounding water; so that the heat is soon diffused through the flask, leaving very little variation of temperature except close to the bottom of the flask or kettle. These convection currents are most vigorous soon after the kettle is put on the fire, when there is the greatest difference of temperature between the water on the bottom and the water above. In this condition however there is no sound, since the vigour of the currents, owing to the greater density of the water above, carry away the water, heated on the bottom, before it has reached a sufficient temperature. Then as the water above acquires heat through the agency of these currents, these currents diminish in vigour but still continue.

When a certain temperature, about 174° F., at the upper surface is reached (which depends on the amount of air occluded in the water) bubbles begin to collect on the surface at the bottom of the flask and then to rise in increasing numbers. These bubbles do not vanish but rise to the surface, increasing in size as they ascend. They are a consequence of the tension of the occluded air added to that of the vapour.

When a bubble first appears there is a sharp but slight click and these clicks, as they become numerous, constitute the preliminary hiss, which nearly subsides before the temperature reaches 200° F.

At about 10° below the boiling-point the harsh hiss comes on suddenly and, in the glass flask, it may be observed that, simultaneous with this sound, there appear again bubbles on the bottom of the flask, which bubbles grow on the bottom gradually until they leave the surface, and start to rise, when unlike the previous bubbles of air they suddenly collapse with a sharp click, which being rapidly repeated causes the harsh hiss. The reason of the collapse of these bubbles is that they are bubbles of steam at the temperature of the boiling-point at the bottom of the flask, formed between the surface of the glass, or metal, and held down by capillary action until they are large enough to break away and ascend, when their ascension brings the steam into contact with the colder water above, when, being free from air, their collapse is sudden and sonorous.

As the temperature still further increases and the difference of temperature between the water at the bottom and that which is above diminishes, the bubbles rise higher and higher before condensing but still collapse suddenly, until the bubbles rise to the surface, when the water boils and the sharp sound subsides as suddenly as it came on.

This analysis of the sound phenomenon of the kettle, which owing to our familiarity with it, has hitherto attracted but little notice, throws very definite light on a fact of the greatest importance to physics, which it would appear has met with partial recognition only.

The question as to whether the motion of continuous liquid between solid boundaries with which it is everywhere in contact can produce sound, as a consequence of the motion, has not I believe hitherto received any definite answer.

The general association of sound with running water has doubtless obscured the subject, although for the most part where it occurs the source of such sound may be easily traced to the variation of the positions of the surface of the water, and particularly where the surface is discontinuous or intermittent.

But, apart from such sources of sound, it is a matter of familiar observation that the flow of water through pipes under great pressure, as when, in the water supply of a town, the water is brought from below the surface of a reservoir on a continuous slope into houses or mains several hundred feet below the reservoir, and is generally attended with a hissing noise; and of this I believe no explanation has hitherto been given. Nor have I ever heard anyone suggest that there is any connection between the singing of the kettle and the hiss which almost invariably attends the opening of a tap in a pipe under considerable pressure as in a town's service. Yet when observed the hiss of the pipe closely resembles the harshest sound of the kettle.

It is now some years since I was led, as the result of hydrodynamical analysis applied to a fluid having the physical properties of water, to the conclusion that both these sources of sound have the same origin.

In hydrodynamics it is customary to consider the physical properties of the fluid as consisting of incompressibility and perfect fluidity only, no account being taken of internal cohesion or of adhesion to solid surfaces, as between water and glass, and still less of any vapour tension in spaces not occupied by the water.

With these limited properties the hydrodynamical problem only admits of solution when the circumstances are such that the pressure is everywhere positive, so that there could be no possibility of disruption of the fluid.

The case however is entirely changed when we recognise that the water has cohesion, depending on its freedom from occluded air as well as viscosity, and that where the water is discontinuous the spaces are filled with vapour at a tension corresponding to the temperature.

It has long been known, as shown by Bernoulli, that when water flows along a contracting channel which it completely occupies, the pressure falls approximately according to the law that the sum of the intensity of pressure p and the product of the density of mass multiplied by the half of the *vis viva* is constant, or

$$p + \rho v^2 = \text{a constant}.$$

Thus if water flows from below the surface of a reservoir, of unlimited dimensions, through a conical tube, the small end of which is in connection with the receiver of an air-pump from which all air has been removed, the small end of the pipe being at the level of the surface of the reservoir, supposing that there is no vapour tension, and the pressure of the atmosphere 14·70 lbs. on the square inch, the water enters the receiver with a velocity of 46·5 feet per second.

If however the temperature of the water is 59° F. the vapour tension is 0·241 lbs. per sq. inch; so that on entering the receiver the water would boil, and if the water and vapour were continually removed the experiment might be continued indefinitely—the water enters in the receiver at 59° F. and boiling, so as to maintain the vapour tension something less than 0·241 lbs. per square inch.

In this case we have a continuous stream of water boiling at the ordinary temperature 59° F. But this cannot be said to be boiling in an open tube. And it is important to notice that although the water at the neck entering the receiver would be boiling, the temperature of the receiver would be maintained at, or about, 59°, so that there would be no condensation of bubbles in the stream of water, and hence the only hissing sound would be that resulting from the disruption of the water as in the preliminary hiss in the kettle when the air bubbles are coming off.

If instead of withdrawing the water and vapour by means of the air-pump we can by taking off the receiver and connecting the small end of the conical tube, so far contracting in the direction of flow, with a similar conical tube the other way about, *i.e.*, expanding in the direction of flow and discharging into a reservoir at a lower level than that of the supply, make such arrangements that the momentum of the water entering the diverging pipe at the minimum section would be sufficient to sweep out the water and bubbles of vapour and air which had been formed in the contracting tube, and secure in the expanding pipe a law of pressure and velocity somewhat similar to that of the contraction:

$$p + \rho v^2 = \text{a constant}.$$

Then as the bubbles of air and vapour in the stream would be carried with great velocity from the low pressure at the neck, where they formed, into the higher pressure in the wider portion of the expanding tube; so that the pressure being greater than the vapour tension, condensation would ensue and the bubbles would collapse, producing the hiss of the kettle before boiling, and in this case we should have water boiling in an open tube. Although certain conditions are necessary a simple experiment shows that these may be realized.

Take a glass tube, say, half-an-inch internal diameter and six inches long, and draw it down in the middle so as to form a restriction with easy gradual curves so that the inside diameter in the middle is something less than the tenth of an inch, leaving the parallel ends of the tube something like $2\frac{1}{2}$ inches each. And then connect one of these parallel ends by flexible hose to a water main which is controlled by a tap. Then, on first opening the tap, the water entering from the main at A will fill the tube as far as the restriction, and pass through the restriction, but it will not, in the first instance, of necessity fill the tube on the far side of the restriction. If the water is turned on very slowly and the open end of the tube is inclined upwards, then the water will accumulate and fill the tube, displacing the air. But if the water is turned on sharply so that when it reaches the neck it has a velocity of 40 or 50 feet a second, the water after passing the minimum section will preserve its velocity and shoot out as a jet from a squirt, not touching the sides of the glass, while if the open end of the tube be held downwards the water, whatever the velocity, will, after passing the restriction, run out of the tube without filling it.

In neither of these cases is there any hiss or sound except such as is caused by the free jet passing through the air.

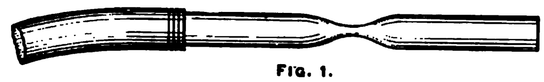


FIG. 1.

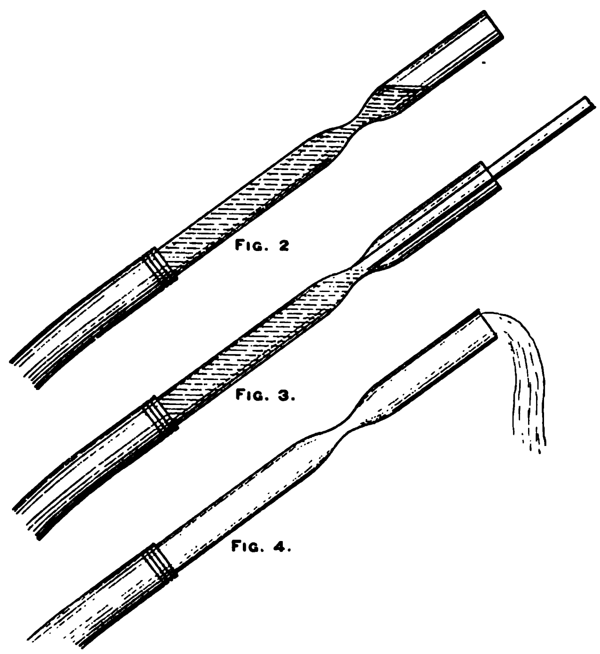


FIG. 2

FIG. 3.

FIG. 4.

But on holding the open end of the tube upwards and quietly filling both limbs of the tube by opening the tap very quietly, as in case (1), and then turning on more water, the water will not shoot out in a jet but will come out like any other stream—as it might do if there were no restriction.

At first, while the velocity through the neck is below 50 feet per second, there is no sound, but as soon as a velocity of 54 feet per second is attained, or a little more, a distinct sharp hiss is heard—exactly resembling that of the kettle or the hiss of the water through a tap.

So far however this is no proof that the hiss is the result of the boiling or disruption of the water. But the hiss is not the only evidence afforded by the experiment. If the glass tube, through which the water is flowing at velocities below that at which the sound comes on, be carefully examined against a black ground to see whether there are any imperfections in the glass in the region of the neck, such as minute bubbles, and the positions of any such carefully located; and if then, after increasing the flow so that the hiss just begins, it be carefully examined again, a small white speck will be observable somewhere in the region of the minimum section a little before where the water enters the neck. This is always observable unless obscured by imperfection in the glass. And a crucial test is afforded by varying the tap so as to bring the hiss on and off; when it will be seen that the appearance and disappearance of the spot and the starting and stopping of the hiss are simultaneous.

The white spot against a dark ground indicates reflection of light by a frost-like surface such as would be afforded by bubbles coming on and going off rapidly, and is thus a crucial proof of disruption in the water or between the water and the glass.

The sound, when it first comes on, is generally loud enough to be heard distinctly over a lecture room, and any increase in the flow augments the sound as well as the size of the spot. See Fig. 4, page 583.

During the experiment the water is quietly flowing out of the tube running with a full bore but with rather an uneven surface, which indicates some internal disturbance. If however the parallel parts of the tube leading to the open end be examined both when the hiss is on and off another phenomenon will be observed, which again furnishes evidence of the effects of boiling.

When the hiss is on, the water in the tube will be somewhat opaque—rather foggy—which fog disappears after the hiss is stopped.

This fog is caused by the separation of the air occluded in the water, and corresponds exactly to the separation of the air, as when the temperature of the water in the kettle is above 174° F. In the case of the tube the bubbles of air, which separate out, are very much smaller than those in the kettle on account of the greater violence of the action.

If however instead of holding the tube with the mouth inclined upwards the tube be immersed in a beaker of water, the water coming out of the tube into the beaker will present the appearance of clouds of white smoke from a chimney. On close examination it is seen that the whiteness is due to minute bubbles, while the cloud-like appearance in the beakers is owing to the fact that the air in these bubbles is being somewhat rapidly again occluded by the water in the beaker, while the motion of the water in the beaker, disturbed by the flow from the tube, wafts the foggy water as it leaves the tube in directions which are continually changing until the reocclusion terminates their existence, leaving those parts of the water farthest from the mouth of the tube practically clear.

In showing this experiment it is not my object to enter into the hydrodynamical and physical considerations, on which the explanation of increase of pressure as the water flows along the diverging tube depends. And I will conclude by pointing out that these considerations are entirely distinct

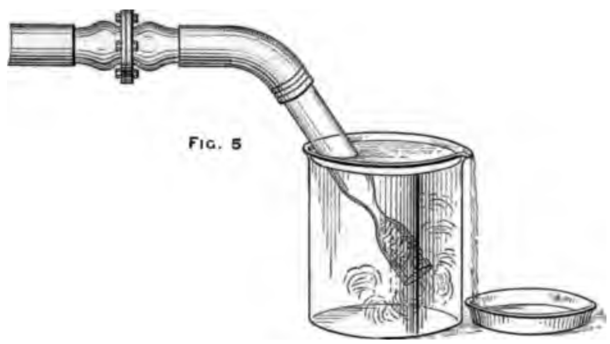


FIG. 5

from those on which the fall of pressure in the water proceeding along the converging channel depends.

In the latter eddies or tumultuous motion the water has no function other than that of diminishing the rate at which the pressure falls, while in the former the rate of increase in the pressure depends entirely on this sinuous eddying or tumultuous motion.

It has been proved definitely that water moving between solid boundaries has two manners of motion depending on whether the value of the quantity expressed by

$$\frac{\rho DV}{\mu} \text{ is greater or less than a certain numerical constant } K.$$

In a parallel pipe water in tumultuous motion entering with a velocity V such that

$$\frac{\rho DV}{\mu} \text{ is greater than 1400,}$$

will, as it flows in the pipe at a steady rate, convert all the eddying motion into heat.

While if water enters a pipe without tumultuous motion, such motion will be generated if

$$\frac{\rho DV}{\mu} \text{ is greater than 1900.}$$

These limits have been for some time established as the limits of the criterion K for straight smooth pipes, and having thus found the limiting values of the purely numerical physical constant, it still remains to find the form of the function corresponding to $\frac{\rho DV}{\mu}$ under other boundary conditions such as parallel pipes with sections other than round and smooth and for converging or diverging boundaries. Such determinations present analytical difficulties which have not been altogether overcome. But it has been possible to obtain from analysis evidences that in converging pipes the critical velocity increases very rapidly with the rate of convergence and, on the other hand, that the critical velocity in diverging pipes diminishes very rapidly as the divergence increases.

When the mean velocity of the water taken over the section of the pipe, whether parallel, converging or diverging, is greater than the critical velocity, there is a steady fall of pressure all along the channel and no rise of pressure in any part, as long as the flow is horizontal.

But as soon as the rate of mean flow exceeds the critical velocity the motion becomes tumultuous—the water moving in all directions across the channel as well as along the channel; so that the continual mixing up of the water which has high forward velocity with that which has less, effected by the lateral motion, ensures a nearly uniform velocity of mean flow across the channel between the boundaries, except at the actual boundaries.

The eddies or tumultuous motion represent an irreversible loss of head or vertical energy in the outflowing stream, but this loss is definitely controlled by the laws of momentum, and were it not for the resistance at the boundaries this law would admit of analytical expression.

Thus, taking u for the velocity of flow, $f(\tan\theta)$ as expressing the divergence of the boundaries, x as the direction of flow, Δ for the area of the section, Δ_c the area at the neck,

$$p_c + \rho u_c^2 - f(\tan\theta)\, pu_c^2 \,.\left\{1 - \left(\frac{\Delta_c}{\Delta}\right)^2\right\} = g\rho H.$$

Such law is only approximately fulfilled on account of our want of definite knowledge of the resistance at the boundaries. But it is comparatively easy to experimentally determine the value of the function $f(\tan\theta)$ for some particular arrangement, and it is found that the same law holds for all geometrical similar arrangements however different the dimensions may be, provided that the velocity is inversely proportional to the linear dimensions.

It also appears that if the divergence, as expressed by $\tan\theta$, is small, owing to the greater length the water has to traverse in the diverging channel to attain equal total divergences, the loss of head owing to the resistance at the boundaries exceeds the resistance where $\tan\theta$ is greater; so that there is a particular value of $\tan\theta$ for which the loss of head is a minimum. And it is found by experiment that when $\tan\theta$ is such that the loss is a minimum the loss of head is about 0·4. Taking this to be the total loss of head in the whole arrangement, it follows as a direct consequence that with the pressure of the atmosphere 14·70 lbs. per square inch, and the temperature 59° F. giving a vapour tension 0·241 per square inch, the minimum pressure necessary to reduce the pressure at the neck to the vapour tension would be

$$\frac{14{\cdot}70 - 0{\cdot}241}{0{\cdot}6} + 0{\cdot}241 = 24{\cdot}34,$$

or subtracting the pressure of the atmosphere the excess of pressure in the reservoir over and above that of the atmosphere is 9·64 lbs. per square inch.

In this case, supposing there were very little air occluded in the water there would be no boiling or rupture in the water, but with the usual amount of air the ruptures would occur under a somewhat less difference of head, such rupture corresponding to the preliminary discharge of air in the kettle.

If the head is increased the point of rupture takes place earlier, that is, at a point before the neck is reached, and the supply of air being strictly limited the pressure will fall until the water boils, sending forth the hissing, or it may be screaming, sound resulting from the sudden condensation of the vapour entering the higher pressure after passing the neck and producing the further evidence of disruption already pointed out. And thus demonstrating that the only sound due to the flow of water between solid boundaries results from the boiling or disruption of the water, whether the actual source of the sound is the disruption or the subsequent condensation of the vapour in the vacuum produced.

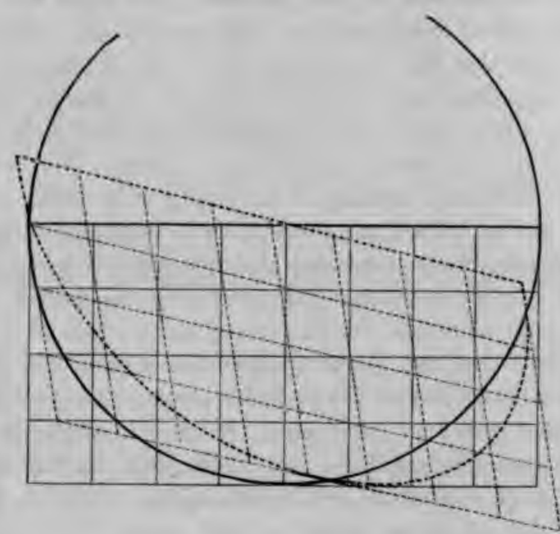
64.

ON THE BEHAVIOUR OF THE SURFACE OF SEPARATION OF TWO LIQUIDS OF DIFFERENT DENSITIES.

[*From the Ninth Vol. of the Fourth Series of the* "Memoirs and Proceedings of the Manchester Literary and Philosophical Society." *Session* 1894—95.]

(*Read* March 19, 1895.)

THE paradox first noticed by Benjamin Franklin which was brought before the Society by Dr Schuster at the last meeting, namely, that when a glass vessel containing water and oil, so that the oil floats on the top of the water, forming two surfaces, one the upper surface of the water and lower surface of the oil, the other the surface between the oil and the air, is moved with a periodic motion, the surface separating the two fluids is much more sensitive and much more disturbed than the upper surface, is very striking, even when the motion of the vessel is somewhat casual—such as may be imparted by the hand. And the paradox becomes even more pronounced when the vessel is, by suspension or otherwise, subject to regular harmonic motion in one plane, and compared with a vessel similar in all respects and similarly situated, except that it contains one fluid only. For while the upper surface of the oil appears to follow the motion of the vessel, remaining very nearly perpendicular to the line of suspension, as it would if the whole mass were a solid, the free surface of the water in the vessel without oil has a decidedly greater amplitude than that of the line of suspension, though the oscillations are exactly in the same phase and the amplitude is still small. On the other hand the surface separating the oil and water has an oscillatory motion about the line of suspension, much greater in magnitude than that of the surface of the water in the vessel without oil, and in exactly the same or the opposite phase. Another very striking fact is that all the surfaces appear to remain plane surfaces when the motion is within certain considerable limits. These motions, however, do depend on the relations between the length of the pendulum, the size of the vessel, and the depths of the fluids, the phase of the separating surface changing from the same to the phase opposite to that of the line of suspension if the pendulum is shortened. The solution of the problem presented by this paradox, although not altogether confirmed or fully worked out, appears to be indicated by the fact that the oscillations of all the surfaces are *steady*, and in the same or opposite phase with the line of suspension, that is, in the same or opposite phase with the disturbance, together with the fact that the free surfaces of the fluids remain nearly plane. For if any material system is, when disturbed, capable of oscillating in a particular period (its natural period), and such oscillation is subject to a viscous resistance, then if subject to a very gradually increasing disturbance, having a period longer than the natural period, the system will oscillate in the period of disturbance always in the same phase as the disturbing force. But if the disturbance has a period shorter than the natural period, the system will oscillate in the same period as the force, but in the opposite phase. Now, in the vessel with oil and water three systems of oscillation, or wave motions, are possible. If the vessel were completely full, so that there were no free surface, and if there were no oil, no oscillation would be possible except (1) the pendulous motion. If half full of oil and filled up with water, then, if disturbed and left, a wave motion (2) in its natural period would be set up in the surface between the oil and water. In the same way (3) if the vessel were half full of water without oil. But in the latter case (3) the natural period would be two or three times less than (2) between the oil and water. Now, when the vessel contains oil and water, disturbances (2) and (3) will both be set up, and might continue, till destroyed by viscosity, in their natural periods if these were the same, but the periods being different, the oscillations in the period (3) would cause periodic disturbance in (2), and the natural period of (3) being much shorter than that of (2), the oscillation so maintained in (2) would be in opposite phase to (3), but, owing to viscosity, such maintenance would be of short duration. If, however, the natural period of the pendulous motion (1) of the vessel were in magnitude between the periods (3) and (2), smaller than (2) and greater than (3), then it would maintain an oscillation in the same period as the pendulous motion in (3) and also in (2), that in (3) having the same phase as the pendulum, that in (2) having the opposite phase. So far this explanation is only partial, as it is assumed that there will be a disturbance in (2) in the same phase as in (3). That this must be the case, however, becomes evident when it is considered that the motion of the water cannot be that of a solid, but must be irrotational, and that the disturbance arises from the non-spherical form of the surfaces of the fluids. If the surface of the vessel were flexible, the motion of the fluids would be essentially that of a particular portion of the water in a long wave adjacent to the surface as shown in the figure. In this, the plain lines indicate lines in the water at rest, which take the position of the dotted lines when the wave surface has the position of the thick dotted line. The black circle indicates the surface



of the spherical vessel; and the dotted curve shows the shape this surface would become if it were subject to the same distortion as the water. In fact, the vessel is rigid, and the surface of the water must conform to it, which requires further internal distortional motion of the water. It is seen there is an excess of water at the top on the higher side and a deficiency on the lower, to supply which the upper surface must be still further tipped, while there is a deficiency on the higher side below and an excess on the lower side, to remedy which the lower surface must tip in the opposite direction. This is exactly what is seen with the oil and water, and is there though it cannot be seen in the water, although not to so great an extent because there is no possibility of an internal wave as between the oil and water.

65.

ON METHODS OF DETERMINING THE DRYNESS OF SATURATED STEAM AND THE CONDITION OF STEAM GAS.

[*From Volume* 41, *Part I. of* "Memoirs and Proceedings of the Manchester Literary and Philosophical Society." *Session* 1896–97.]

(*Read* November 3, 1896.)

WHEN, after all air has been expelled from a vessel partially filled with water and kept at rest at a constant temperature, equilibrium is established, the vapour is said to be dry saturated steam.

It is easy to show that under these circumstances the pressure of the steam is a definite function of the temperature. But it has been found very difficult to show, by direct means, that the density of the steam is also an invariable function of the temperature, although many experiments, from the time of Watt, have indicated that this is the case; those of Fairbairn and Tate being the least open to criticism.

That the density of dry saturated steam is a constant function of the temperature has, however, been completely established indirectly by the experiments of M. Regnault on the total heat of evaporation, although these experiments do not directly furnish a measure of the density. These experiments consisted in maintaining a vessel containing a definite quantity of water in steady constant condition as to temperature and pressure and quantity of water, by the steady admission of water at *any* constant temperature, and the withdrawal of the vapour in an upward direction, with a slow motion so as to preclude the convection of water out of the vessel by the steam, the steam so withdrawn being condensed in a calorimeter back again to water at any constant temperature. The results proving that the total amount of heat given up by the steam for each temperature in the boiler is consistently proportional to the weight of steam condensed.

It thus appears that the density of saturated steam at constant temperature must be constant, and that gravity alone is sufficient to free the saturated steam from any water that may have been entangled with it by the action of boiling, provided the rate of flow over the surfaces is not sufficient to carry along with the steam any water there may be on the surfaces. It was only after the utmost care in securing these conditions that Regnault succeeded in obtaining consistent results—which results have since been confirmed by many researches, including that of Messrs Harker and Hartog read before the Society last year.

It is to be noticed that the whole theory of the properties of steam, as at present accepted, and all the steam tables, are founded on these experiments of Regnault's on the total heat of evaporation, so that if any other definition is given of dry saturated steam, than that of the vapour of water which results from boiling the water under constant pressure after it is drained of entangled water by gravitation, these properties and tables will not apply.

Wet Steam.

For the most part the precautions taken by Regnault are precisely those under which steam is produced in practice. That is to say, in practice the conditions in the boiler are maintained, as far as practicable, steady, and the steam is withdrawn in a vertical direction from the steam space over the water, where it is drained by gravitation. Owing, however, to exigencies as to space and weight, a great deal more steam is often generated in proportion to the space than was the case in the experiments. Also the velocity of the steam after entering the steam pipes is, in practice, often so great that, even where these are ascending, any water that may have been drawn in with the steam, or produced by condensation owing to the radiation of heat from the pipes, is swept along with the steam; and where, as in cases like the locomotive, the engine is under the boiler, so that the pipes are descending, this must be so. Under such conditions the steam as it enters the engine will be accompanied by some water, and is then variously called "wet steam," "nearly dry steam," or "super-saturated" steam, though the last name is apparently intended to imply that, notwithstanding Regnault's experiments, the density of steam after drainage is not necessarily a definite function of the temperature or pressure.

Whatever may be the cause of the water entering the engine with the steam, its presence in unknown quantity prevents Regnault's formula for the total heat of evaporation from being used to form a correct estimate of the quantity of heat received by the engine. For the only measures of the steam supplied to the engine are obtained from the measures of the feed-water supplied to the boiler, or the water discharged from a surface condenser, so that, if an unknown quantity of water enters with the steam, estimates so formed must be in excess.

This is a matter of very serious consideration in all attempts to obtain a comparison of the actual performance of an engine in work done, as compared with the theoretical performance under ideal conditions. And, as the modern practice of steam engineering is largely guided by the results of such attempts, methods of assuring dry steam or, failing that, of in some way measuring the percentage of water passing with the steam into the engine, have attracted a great deal of attention.

For purely experimental purposes, it is always possible to supply the engine with dry steam, even where the boiler is at a distance, by passing the steam through a sufficiently large vessel close to the engine, so that the water may be disentangled by gravitation before the steam enters the engine. These are called water-separators. In some cases such separators form part of the engine, but, although their employment is becoming more common, it is only in comparatively few cases that this is practicable.

In other cases, that is, in the great majority of cases, the desire to obtain some experimental evidence of the percentage of water in the steam as it enters the engine, has led to the use of methods of testing samples of the steam drawn continuously from the steam pipe close to the engine.

Sampling the Steam.

In such methods, the question of getting a fair sample of the steam as it enters the engine is quite distinct from that of testing the sample so obtained. The water in the pipe, although moving in the direction of the steam, will not be uniformly distributed throughout the steam, and will, to a great extent, merely drift along the surface of the pipe and mostly on the lower surface, so that unless a sample taken from the lowest part of the pipe is found to be dry, in which case the steam is dry, such methods afford but little evidence as to the percentage of water entering the engine with the steam.

Testing the Samples.

For absolute dryness such samples may, where the pressure in the steam pipe is steady, be tested by allowing the sample to flow quietly through a separator, so as to drain out the water, the weight of which is then observed. But any attempt to estimate the percentage of water in the sample involves the subsequent condensation and weighing of the steam in the sample, as well as the drained water, which are difficult and complicated operations. Besides this, the pressure in the steam pipe near the engine is generally subject to considerable periodic alterations, owing to the intermittent and periodic demand for steam in the engine, which introduces complications of unknown extent.

Wire-drawing Calorimeters.

With a view to obtaining a test for the samples of steam, which should be independent of the separator, the so-called Wire-drawing Calorimeter has been introduced. In this, the sample of steam, whether it has been first drained or not, is received quietly in a vessel at the same pressure as the steam pipe, where it is at steady known pressure; from this it is allowed to escape continuously through a small orifice into a second larger vessel, maintained at greatly lower pressure than the first. In this its temperature and pressure are measured, the steam then passing on into a condenser or into the atmosphere.

The quantity of water present is then estimated from the observed pressures in the two vessels, and the difference between the observed temperature in the second vessel and the temperature of saturation at that pressure, as taken from Regnault's tables.

Such calculations are at once seen to be based on Regnault's determination of the relations between the pressure and temperature of saturated steam, together with the heat relations, whatever they may be, between saturated steam and superheated steam. And, as the second relation does not appear to be known except as a very rough approximation, the results so obtained must be doubtful.

Results.

The results obtained with these calorimeters have apparently revealed the presence of anything up to 5 per cent. more water in the samples than revealed by the *simple separator*, and this even when the steam has been drained in the separator before passing into the calorimeter.

This apparent experimental evidence of previously unsuspected water carried by steam has necessarily excited great interest, and is naturally welcomed, as it apparently brings the engines by so much nearer perfection.

On second thoughts, however, a very serious consideration will present itself, namely, that if the drained steam from a separator contains latent water, the drained steam from the separator on which Regnault made his experiments must also have contained similar latent water, and hence the theoretical volumes of steam, which are based solely on these experiments, must be subject to identically the same corrections as the observed results, so that the discovery, if true, would thus leave the percentage of theoretical performance unchanged, while it would upset the truth of Regnault's results as to the properties of steam—and, moreover, upset all other deductions from these properties, including the deductions involved in these estimations.

That such is the case cannot be admitted until after the fullest consideration and verification of the experiments, and of the method of reduction by which the novel results have been obtained.

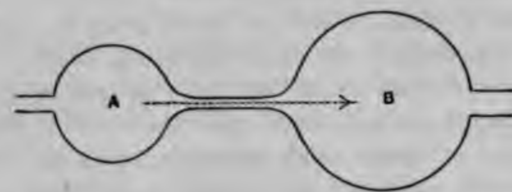
These experiments are many, and the methods of reducing the results have not been very fully, although widely, published, but in all that I have seen the results have been deduced by means of the properties of steam as determined by Regnault's experiments, by a formula which is based on a misunderstanding of the meaning of "the specific heat, at constant pressure, for steam when in the gaseous state," as determined by Regnault. And that this must have been the case with the other results would seem to follow from the fact that this formula, when based on the correct meaning, affords no definite result at all under the circumstances of the experiments.

It has thus seemed to me important not only to call attention to the error in reduction by which certain of these results have been obtained, but also to indicate, and if possible to verify, a method by which experiments could be made, so that Regnault's determination of the specific heat of steam gas could be correctly used to ascertain whether or not such latent water does exist in drained steam—that is, to ascertain whether Regnault's experiments on the specific heat of steam gas are consistent with his experiments on the latent heat of steam.

In the present paper the purpose is limited to pointing out the theory of the reductions, and to giving indications of the method of experimenting, the general character of the apparatus, and the precautions necessary.

The Theory of the Reductions.

By the law of conservation of energy, when a steady stream of matter flows through a chamber with fixed walls, so that the condition within the chamber is steady, the energy of the matter which enters (potential and actual) is equal to the energy which leaves in the same time, and hence is equal to the energy of the matter which leaves, together with such energy as may escape into the walls of the chamber. Thus, if a stream of fluid flows in a horizontal direction through a fixed passage and if



$$\left.\begin{aligned} P_1' &= \text{pressure,} \\ T_1' &= \text{temperature,} \\ V_1' &= \text{volume per lb. of fluid,} \\ H_1' - P_1'V_1' &= \text{mechanical equivalent of heat per lb. of fluid,} \\ u_1 &= \text{velocity of fluid} \end{aligned}\right\} \text{at } A,$$

$$\left.\begin{aligned} P_2' &= \text{pressure,} \\ T_2' &= \text{temperature,} \\ V_2' &= \text{volume per lb. of fluid,} \\ H_2' - P_2'V_2' &= \text{mechanical equivalent of heat per lb. of fluid,} \\ u_2 &= \text{velocity of fluid} \end{aligned}\right\} \text{at } B,$$

and H_j = the mechanical equivalent of heat received through the surface per lb. of fluid passing through;

then $$H_1' + \frac{u_1^2}{2g} + H_j = H_2' + \frac{u_2^2}{2g} \quad \text{.............................(1).}$$

Also, if the fluid at A consists, per lb., of

S_1 lb. of steam and $(1 - S_1)$ lb. of water,

and at B consists of

S_2 lb. of steam and $(1 - S_2)$ lb. of water,

and if h_1 and h_2 are put for the mechanical equivalents of heat per lb. of water respectively at the temperatures, T_1 and T_2, of saturated steam at pressures of P_1' and P_2' respectively, then $T_1' = T_1$, where P_1' and T_1 are pressure and temperature corresponding to the initial state of saturated steam at A, and T_2 may be taken to correspond to the temperature of saturated steam at pressure P_2'. And if, further, H_1 equals the equivalent of the total heat of evaporation at pressure P_1' per lb., then

$$H_1' = S_1 (H_1 - h_1) + h_1 \quad \text{..........................(2).}$$

And if, similarly, H_2 and h_2 correspond to the temperature of saturated steam at pressure P_2', then

$$H_2' = S_2 (H_2 - h_2) + h_2 + K (T_2' - T_2) \quad \text{..................(3),}$$

where K is the mean specific heat of steam at constant pressure between the temperatures T_2' the actual temperature at B, and T_2 the temperature of saturated steam at the actual pressure (P_2') at B. It being noticed that, if $1 - S_2$ is greater than nothing, $T_2' = T_2$, so that the last term in (3) vanishes. While, if $(1 - S_2)$ is zero, this last term expresses the heat, whatever it may be, requisite to raise steam, at constant pressure P_1', from the temperature of saturation T_2 to the observed temperature T_2'.

Substituting from equations (2) and (3) in equation (1), this becomes

$$S_1(H_1 - h_1) + h_1 + \frac{u_1^2}{2g} + H_j = S_2(H_2 - h_2) + h_2 + \frac{u_2^2}{2g} + K(T_2' - T_2) \quad \ldots(4).$$

If then u_1, u_2, and H_j are small enough to be neglected, since the values of H_1, h_1, H_2, h_2, T_2 are obtainable from Regnault's tables, when P_1', P_2' or T_1' are observed, all the remaining quantities may be known except S_1, S_2, and K. And either, if S_2 is not equal to unity, $(T_1' - T_2) = 0$, and

$$S_1(H_1 - h_1) + h_1 = S_2(H_2 - h_2) + h_2 \quad \ldots\ldots\ldots\ldots\ldots\ldots (5),$$

or, if

$$(1 - S_2) = 0,$$

$$S_1(H_1 - h_1) + h_1 = H_2 + K(T_2' - T_2) \quad \ldots\ldots\ldots\ldots\ldots\ldots (6).$$

Equation (5) gives S_1 in terms of S_2 when $T_2' = T_2$, but, since S_2 is unknown, this is of no use; while, if T_2' is greater than T_2, equation (6) gives S_1 in terms of K which is a function of T_2 and T_2', which has not been determined.

If it were possible to determine the exact value of T_2' at which $S_2 - 1 = 0$, then

$$S_1(H_1 - h_1) + h_1 = H_2.$$

But, here again, this is practically impossible, since the only indication that $S_2 - 1 = 0$ is that T_2' is greater than T_2 as given by Regnault's tables for steam at P_2', and, for any such excess as can be observed, the value of $K(T_2' - T_2)$ is considerable, since, at the point of saturation, K is apparently infinite, so that neither of these determinations is practical.

With a view to getting over these difficulties, the course that has apparently been adopted is to obtain a condition such that the temperature (T_2') after wire-drawing is from 10° to 20° F. higher than the saturation temperature (T_2), and then to assume that K is equivalent to the specific heat at constant pressure of steam gas as determined by Regnault, or that

$$K = 772 \times 0{\cdot}48,$$

an assumption which constitutes the error in reduction to which I have referred.

The possibility of obtaining an accurate estimate.

This depends on obtaining a certain condition in the experiment, and reducing by a formula proved by Rankine (*Trans. Roy. Soc. Edinb.*, 1849, 1855).

Rankine's formula is that the total heat to convert water from a liquid state at any particular temperature, say 32°, to steam gas at any temperature (T_2'), *the operation being completed under constant pressure*, is expressed by

$$\frac{H_2'}{772} = C_1 + a\,(T_2' - 32°),$$

C_1 being a quantity depending only on the initial state, and a being the specific heat at constant pressure of the steam gas, determined by Regnault to be

$$0{\cdot}48.$$

Taking the initial state to be at 32°, Rankine obtained, as the most probable value,

$$C_1 = 1092.$$

It is to be noticed, however, that although this value 0·48, as obtained by Regnault, has been universally accepted, the experiments by which he obtained it were independent of the method by which he determined the total heat of evaporation of saturated steam, and that, as Regnault observes*, the smallness of the scale, as compared with that by which the total heats were determined, rendered it necessarily less accurate, as regards the measurement of the total quantities of heat observed, although the extreme care with which the numerous experiments in the four cases were made, seems to assure their relative accuracy. The experiments consisted in determining the total heat necessary to raise water from 32° F. or 0° C. to temperatures of about 120° C. and 220° C. under the pressure of the atmosphere, then taking the differences as being the heat necessary to raise water from 120° C. to 220° C. It thus involves the assumption that steam at 20° C. (or 36° F.) above the boiling point, is in the condition of steam gas. This is probably very near the truth. Had, however, the experiments been as absolutely accurate as those for the total heat of saturated steam, they would have afforded the means of comparing the two methods of Regnault by Rankine's thermodynamical formulæ. As it is, such a comparison can be made. Thus, substituting the total heats as obtained in the experiments for specific heat in Rankine's formula, the constant C_1 is found to be not 1092, as given by Rankine, but between 1076·4 and 1053·7, with a mean of about 1055.

* *Mém. Acad. Sci.*, Vol. XXVI. pp. 170, 909.

Taking this value, the heat necessary to raise water from 32° to 248° F. at constant pressure of 14·7 lbs. per square inch is

$$1055 + 0\cdot48\,(216) = 1158\cdot68.$$

To raise water from 32° to saturated steam at 212° requires by Regnault's formula for total heat of saturated steam

$$1091\cdot7 + \cdot305\,(180) = 1146\cdot6.$$

Hence, to raise saturated steam from 212° to 248° at constant pressure would require 12·08 T.U., which, divided by the difference of temperature, gives for the mean specific heat of steam from saturation at 212° to 248° F. at constant pressure

$$\frac{12\cdot08}{36} = \cdot335,$$

which shows that the specific heat, at constant pressure, of steam rises with the temperature. And this, although in accordance with the results obtained by Regnault for other vapours, presents great thermodynamical difficulties; since many experiments have shown that the steam, on being heated from saturation to 36° F. above, expands three or four times as much as it would if it were gas. It is to be noticed that an error of 3 per cent. in estimating the total quantity of steam, which in these experiments would only mean an error of

$$0\cdot0004$$

in the actual weighings, would account for the differences in the values of C_1 as determined by Rankine, and as estimated from Regnault's experiment on specific heat, while such an error on the determination of the specific heat would fall within the limits of experimental accuracy. It thus seems probable that Rankine's determinations of the constants in his formula are approximately right.

In order to make use of this formula in the reduction of the experiments under consideration, all that is necessary is to bring about, by means of wire-drawing, the condition that T_2' shall be sufficiently larger than T_2 to insure that the final condition approximates to that of steam gas. That this difference must be more than 20° F. has been shown, but it would appear that with this difference the error is not great.

To use the formula,

$$\{1092 + 0\cdot48\,(T_2' - T_2)\}\,772$$

is substituted for the right member of equation (6).

H_j, $\dfrac{u_1^2}{2g}$, $\dfrac{u_2^2}{2g}$ being small, therefore

$$S_1(H_1 - h_1) + h_1 = 772\,\{1092 + 0\cdot48\,(T_2' - T_2^\circ)\}\;\ldots\ldots\ldots\ldots(7),$$

which only requires the experimental determination of T_1 and T_2' to give the value of S_1, provided that the final condition is that of steam gas.

The means of assuring the condition of Steam Gas.

Perhaps the most important fact to which attention is herein directed is that, although, as already stated, the limiting relations of temperature and pressure of steam gas are not known with any degree of precision, the wire-drawing experiments are capable of affording simple and direct evidence of the existence of such a final state. As the pressure of steam is reduced by wire-drawing, which is gradually increased, at first, owing to the great expansion, the temperature falls considerably, but, as the wire-drawing increases, by the diminution of pressure in the receiving vessel the fall of temperature gradually diminishes, until the gaseous state is produced, when the temperature T_2' will be unaffected by still greater wire-drawing.

So that to insure a gaseous state, all that is necessary is to gradually diminish the pressure in the receiving vessel, maintaining that in the first vessel, until the temperature T_2' in the receiving vessel becomes constant.

The only doubt is whether this point can be practically reached, and this can only be determined by experiments.

The remarkable circumstance in the flow of gases, of which I published the explanation in a paper read before this Society in 1885, that when steam or gas flows through a restricted channel from one vessel into another, in which the pressure is less than half that of the first, the quantity which passes is independent of the pressure on the receiving side, must have an important place in simplifying the apparatus required for such experiment.

Thus, with boiler pressure on one side of an orifice, opening into a vessel from which its escape is allowed by an adjustable valve, the whole experiment can be regulated by this valve, the quantity flowing through remaining constant for all pressures after the half is reached.

The only precautions necessary for accuracy, are those to secure approximately small velocities at the points where the temperature is measured, and those to render small the loss of temperature in the steam by radiation. And, although these must complicate the appliances, they appear to be practical. I may also notice that, should such experiments be accomplished, they will afford the means of verifying or correcting Rankine's value for C_1, which he has only given as a probable approximate value.

I hope these experiments may shortly be made, as Mr J. H. Grindley, B. Sc., Fellow of Victoria University, has undertaken the research in the Whitworth Engineering Laboratory, Owens College.

66.

BAKERIAN LECTURE.—ON THE MECHANICAL EQUIVALENT OF HEAT.

[*From the* "Philosophical Transactions of the Royal Society of London," 1897.]

(*Read* May 20, 1897.)

PART I.

By Professor OSBORNE REYNOLDS, F.R.S., and W. H. MOORBY, M.Sc., late Fellow of Victoria University and 1851 Exhibition Scholar.

ON THE METHOD, APPLIANCES AND LIMITS OF ERROR IN THE DIRECT DETERMINATION OF THE WORK EXPENDED IN RAISING THE TEMPERATURE OF ICE-COLD WATER TO THAT OF WATER BOILING UNDER A PRESSURE OF 29·899 INCHES OF ICE-COLD MERCURY IN MANCHESTER. —BY OSBORNE REYNOLDS.

The Standard of Temperature for the Mechanical Equivalent.

1. THE determination by Joule, in 1849, of the expenditure of mechanical effect (772·69 lbs. falling 1 foot) necessary to raise the temperature of 1 lb. of water, weighed *in vacuo*, 1° Fahr. between the temperatures of 50° and 60° Fahr. (at Manchester), together with the second, in 1878, 772·55 ft.-lbs., to raise the temperature of 1 lb. (weighed *in vacuo*) from 60° to 61° Fahr., at the latitude of Greenwich, established once for all the existence of a physically constant ratio between the work expended in producing heat and the heat produced; while the extreme simplicity of his methods, his marvellous skill as an experimenter, and the complete system of checks he adopted, have led to the universal acceptance of the numbers he obtained

as being within the limits he himself assigned (1 foot), of the true ratio of work expended in his experiments in producing heat and the heat produced as measured on the scale of the thermometer on which he spent so much time and care.

The acceptance of $J = 772$, as the mechanical equivalent of heat, amounts to the acceptance of the scale between 50 and 60 on Joule's thermometer *b* as the standard of temperature over this range.

Joule's thermometers are now in the custody of the Manchester Literary and Philosophical Society (having been confided to its care by Mr A. Joule); so that this material standard is available. But the standard of temperature actually established by Joule is universally available wherever the British standard of length is available, together with pure water and the necessary means and skill of expending a definite quantity of work in raising the temperature of water between 50° and 60° Fahr., since in this way the scale on any thermometer may be compared with that on Joule's.

The difficulty of access to Joule's thermometer, and the inherent difficulty of making an accurate determination of the equivalent, have limited the number of such comparisons.

The most serious attempts have been made with the very desirable object of determining the mechanical equivalent of a thermal unit, measured on the scale of pressures of gas at constant volumes, first recognised by Joule as the nearest approximation to absolute temperature.

The results of these comparisons have been various, all having apparently shown that Joule's standard degree of temperature is less than the one-hundred-and-eightieth part between freezing and boiling points on the scale of pressure of gas at constant volume, the differences being from 0·1 to 1·0 per cent. Joule himself contemplated comparing his thermometer with the scale of air pressures, but did not do so. So that only indirect comparisons have been possible.

Hirn, who was the first to follow Joule, in one of his researches introduced a method of measuring the work done which afforded much greater facility for applying the work to the water than the falling weights used by Joule in his first determination, and this was adopted by Joule in his second determination. But notwithstanding the greater facilities enjoyed by subsequent observers, owing to the progress of physical appliances, the inherent difficulties remained. The losses from radiation and conduction could only be minimised by restricting the range of temperature, and this insured thermometric difficulties, particularly with the air thermometer, which, it seems, does not admit of very close reading. This, together with certain criticisms, of which some of the methods employed admit, appear to have left it still an open question what exact rise in the temperature in the scale of air pressures corresponds to the 772 ft.-lbs.

2. The research, to the method and appliances for which this paper relates, has been the result of the occurrence of circumstances which offered an opportunity, such as might not again occur, of obtaining the measure, in mechanical units, of the heat in water between the two physically fixed points of temperature to which all thermometrical measurements are referred, and of thus placing the heat as defined in mechanical units, on the same footing as the unit of heat as defined by temperature, without the intervention of scales, the intervals of which depend on the relative expansions of different materials such as mercury and glass.

It has been, so far as I am concerned, undertaken with considerable hesitation, on account of the responsibility even in attempting such a determination, and the harm to science that might follow from further confusion owing to error in what, in spite of opportunities, must be the extremely difficult task of making such complex determinations within less than the thousandth part. These considerations, together with my inability to find the large amount of time necessary for making the observations, prevented any attempt until July, 1894. At that time Mr W. H. Moorby offered to devote his time to the research, and so relieve me of all responsibility except that which attached to the method and the appliances; and having, from experience, the highest opinion of Mr Moorby's qualifications for carrying out the very arduous research, there seemed to be no further excuse for delay, particularly as after seeing the appliances in the laboratory both Lord Kelvin and Dr Schuster expressed strongly their opinion as to the value of the research.

The Opportunity for the Research.

3. This consisted in the inclusion in the original equipment of the laboratory, in 1888, of the following appliances :—

(1) A set of special vertical triple-expansion steam-engines, with separate boiler, closed stoke-hole, and forced blast; these engines being specially arranged to give ready access to the shafts, 3 feet above the floor, and being capable of running at any speeds up to 400 revolutions per minute, and working up to 100 H.-P. (Plate 1).

(2) Three special hydraulic brake dynamometers, on separate shafts, between and in line with the engine shafts, with faced couplings, so that one brake shaft could be coupled with the shaft of each engine to work its own shaft; or the brakes on the high-pressure and intermediate engines could be removed, and their shafts coupled by means of intermediate shafts,

so that all three engines worked on the brake connected with the low-pressure engine. These brakes, which are shown (Plate 1), are separately capable of absorbing any power up to a maximum of 30 horse-power at 100 revolutions, and increasing as the cube of the speed; so that a single brake is capable of absorbing the whole power of the engine at any speed above 100 revolutions a minute.

The whole of the work is absorbed by the agitation of the water contained in the brake, while the heat so generated is discharged by a stream of water through the brake, with no other functions than of affording the means of regulating, independently, the temperature of the brake and the quantity of water in the brake. The moment of resistance of the brake at any speed is a definite function of the quantity of water in the brake. And as, except for this moment, the unloaded brake is balanced on the shaft, the load being suspended from a lever on the brake at 4 feet from the axis of the shaft, if the moment of resistance of the brake exceeds the moment of the load, the lever rises, and *vice versâ*. By making the lever actuate the valve which regulates the discharge from the brake, and thus regulate the effluent stream, the quantity of water in the brake is continually regulated to that which is just sufficient to suspend the load with the lever horizontal, and a constant moment of resistance is maintained whatever may be the speed of the engines.

(3) Manchester town's water, of a purity expressed by not more than 3 grams of salts in a gallon, brought into the laboratory in a 4-inch main at town's pressure (50 to 100 feet head), and distributed either direct from the main or at constant pressure from a service tank 10 feet above the floor of the laboratory.

(4) Two tanks, each capable of holding 60 tons of water, one in the tower, 116 feet above the floor, the other 15 feet below the floor, connected by 4-inch rising and falling mains, each 500 feet long, passing in a chase under the floor. The rising main is in communication with a special quadruple centrifugal pump, 2 feet above the floor, capable of raising a ton a minute from the lower to the upper tank. (Shown in Plate 5.) Also a set of mercury balances, showing continually the levels of water in the two tanks, and the pressures in the rising, falling, and town's mains. (Shown in Plate 2.)

(5) A special quadruple vortex turbine, supplied from the falling main and discharging into the lower tank, capable of exerting 1 H.-P., and available for steady speeds at all parts of the laboratory. (Shown in Plate 5.)

(6) A supply of power to the laboratory by an engine and boiler, quite distinct from the experimental engine, and distributed by convenient shafting which is always running. (Shown in Plate 1.)

The Measurement of the Work.

4. Of the appliances mentioned, the brake on the low-pressure engine is the centre of interest, as it was by this that the work was measured, as well as converted into heat.

The existence of the appliances was largely due to the interest in educational work taken by Mr William Mather, who, together with the other members of the firm of Mather and Platt, not only placed at my disposal the facilities of their works, but, inspired the enthusiasm which alone rendered the execution of such novel and special work possible.

The development of the brake dynamometer, from its introduction by Prony, has an interesting and important history, but into this it is not necessary to enter. The purpose of these dynamometers is to afford continuous frictional resistance, adapted to the power exerted by the prime mover in causing a shaft to revolve, and of a kind that is definitely measureable. To fulfil the first of these conditions, the mean moment of resistance of the brake must just balance the mean moment of effort of the engine, and the means of escape of heat from the brake must be sufficient to allow all the heat generated to depart, without accumulating to an extent which may interfere with the action of the appliances. In the first brakes the resistance was obtained by the friction of blocks or straps pressed against a cylindrical wheel on the shaft, and, small powers being used, radiation and air-currents round the brake were found sufficient to carry off the heat, but, when larger powers were used, these sources of escape failed to keep the temperatures down to practical limits, which necessitated the application of currents of water to carry off the heat.

The measurement of the work was invariably accomplished by attaching the brake blocks, or straps, to a lever, or arm, so that the whole brake would be free to revolve with the brake-wheel, except for the moment of the weight of the parts which, adjusted to the power of the engine, was kept in balance by the adjustment of the pressure of the blocks on the wheel. Then, since the work done is equal to the product of the mean moment of resistance, over the angle turned through, multiplied by the angle, if the resistance is constant over time, the moment of the *brake*, multiplied by the whole angle, measured the work done.

It is however to be noticed that the assumption, that the *time-mean* of the moment on the brake is the same as would be the *angle-mean* of this moment, might involve an error of any extent, provided the resistance and the angular velocity varied in conjunction. And as steam engines invariably exert an effort, varying within the period of the revolution, while the friction and the pressure causing it are apt to respond to any variations of speed, it is probable that there has been some error from this cause in all such measurements, although not previously noticed.

Hirn appears to have been the first to recognise that in a steady condition the resistance of fluid between the brake-wheel and the brake would answer instead of the solid friction, so that the mean time moment of effort exerted in turning a paddle in a case, containing water, with bafflers, would be strictly measured by the mean time moment of the case. And although subject to the same error from periodic motion as the friction brake, the facility this fluid brake offered for cooling and regulating led to its simultaneous adoption and development by several inventors, for measuring power —the late William Froude, for the purpose of measuring the work of large engines, inventing that arrangement of paddle vanes and bafflers which gives the highest resistance, regulating the resistance by thin sluices between the vanes and bafflers, and always working with the case full of water.

The brake under consideration differs from that of Mr Froude in only one fundamental particular—the provision by which a constant pressure in the interior of the brake is secured by the admission of the atmosphere to that part of the brake where the dynamical effect of the water is to cause the lowest pressure—this admits of working the brakes with any quantity of water from nothing to full, and thus allows of the regulation of the resistance, by regulating the quantity of water in the brakes, without sluices.

The description of this brake has already been published, together with that of the engines*, but it will be convenient to give a short description.

This brake consists primarily of (1) a brake wheel, 18 inches in diameter, fixed on the 4-inch brake shaft by set pins, so that it revolves with the shaft (Figs. 2 and 3), and (2) a brake (or brake case) which encloses the wheel, the shaft passing through *bushed* openings in the case which it fits closely, so as to prevent undue leakage of water while leaving shaft and brake-wheel free to turn in the case, except for the slight friction of the shaft (Figs. 1, 2 and 3).

The outline of the axial section of the brake-wheel is that of a right cylinder, 4 inches thick. The cylinder is hollow—in fact, made of two discs which fit together, forming an internal boss for attachment to the shaft, and also meet together at the periphery, forming a closed annular box, except for apertures to be further described (Fig. 3). In each of the outer disc faces of the wheel are 24 pockets, carefully formed, $4\frac{1}{2}$ inches radial, and $1\frac{1}{2}$ inches deep measured axially, but so inclined that the narrow partitions or vanes ($\frac{1}{4}$ inch) are nearly semicircular discs inclined at 45° to the axis; the vane on one face being perpendicular to the vane on the opposite face (Fig. 2).

* "Triple Expansion Engines," by Professor Osborne Reynolds, *Minutes of Proceedings, Inst. C. E.*, vol. 99, 1889, p. 18. (See Paper 56, page 336.)

The internal disc faces of the brake case, as far as the pockets are concerned, are the exact counterparts of the disc faces of the wheel, except that there are 25 pockets, so that the partitions in the case are in the same planes as the partitions meeting them in the wheel, there being $\frac{1}{64}$ inch clearance between the two faces.

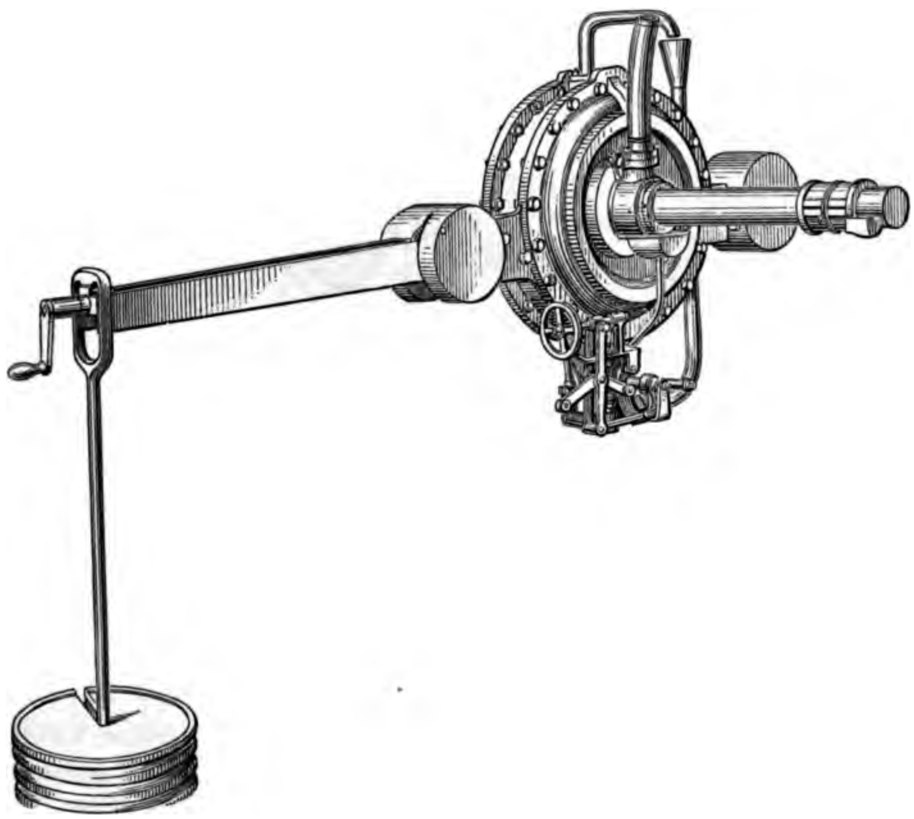


Fig. 1.

The pairs of opposite pockets, when they come together, form nearly closed chambers, with their sections, parallel to the vanes, circular. In such spaces vortices in a plane inclined at 45° to the axis of the shaft may exist, in which case the centrifugal pressure on the outside of each vortex will urge the case and the wheel in opposite directions inclined at 45° to the direction of motion of the wheel, which will give a tangential stress over the disc faces of the wheel of $1/\sqrt{2}$ of the sum of these vortex pressures. The existence and maintenance of these vortices is insured by the radial centrifugal force of the water in the pockets in the wheels owing to its motion.

This is the late Mr W. Froude's arrangement. But an essential feature of the brake is the provision which insures the pressure of the atmosphere at the centre of the vortices, even when the pockets are only partially filled.

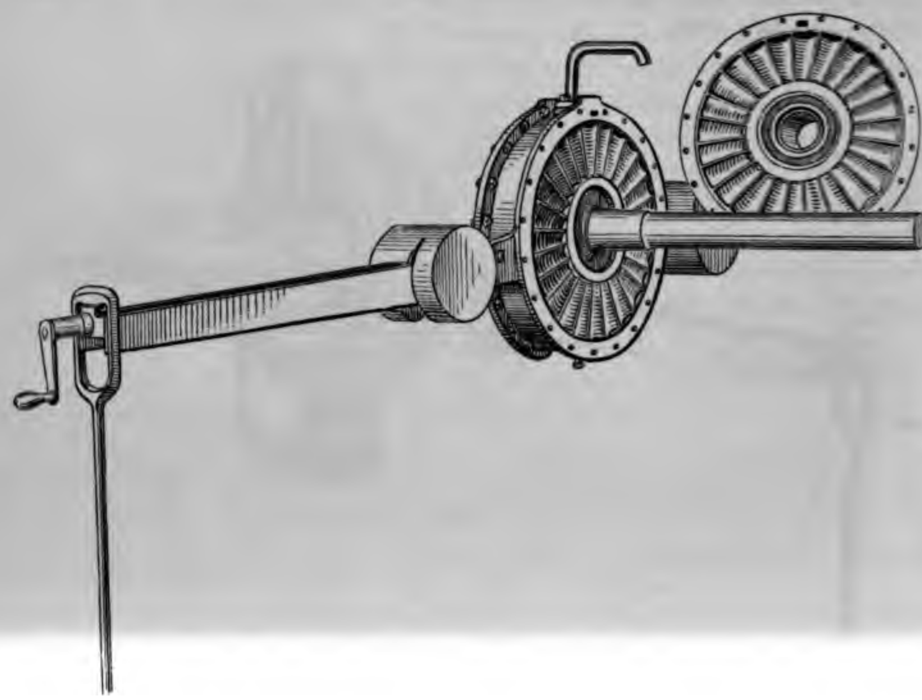


Fig. 2.

The vortex pressure is greatest at the outsides of the vortices, which occurs all over the annular surfaces of the pockets, but the actual pressure on these surfaces is not determined solely by the vortex motion unless the state of pressure at the centre of the vortices is fixed, for the vortex motion only determines the difference between these pressures. To insure the constant pressure, and at the same time to allow of the pockets being only partially full—that is, to allow of hollow vortices with air cores at atmospheric pressure, it is necessary that there should be free access of air to the centres of the vortices, and as this access cannot be obtained through the water, which completely surrounds these centres, it is obtained by passages ($\frac{1}{8}$ inch diameter) within the metal of the guides, which lead to a common passage opening to the air on the top of the case (Figs. 2 and 3).

To supply the brake with water there are similar passages in the vanes of the wheel leading from the box cavity, which again receives water through ports which open opposite an annular recess in one of the disc faces of the case into which the supply of water is led, by means of a flexible indiarubber pipe, from the supply regulating valve.

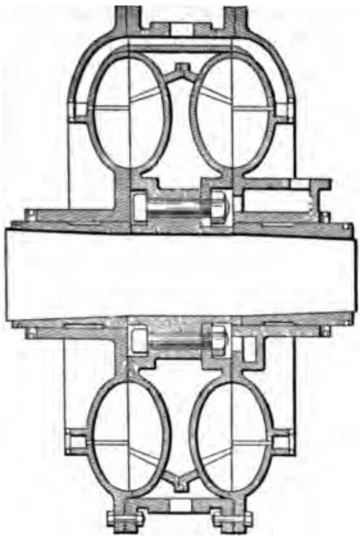


Fig. 3.

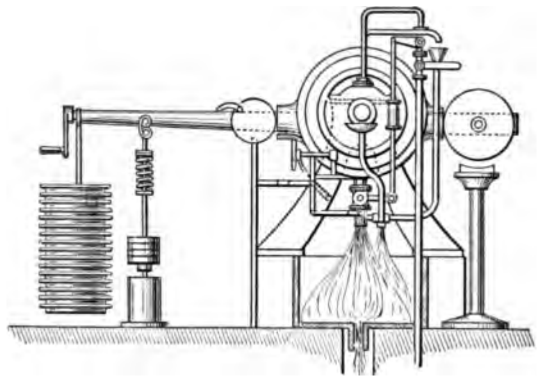


Fig. 4.

The water on which work has been done leaves the vortex pockets by the clearance between the disc surfaces of the wheel and case, and enters the annular chamber between the outer periphery of the wheel and the cylindrical portion of the case, which is always full of water when the wheel is running, whence its escape is controlled by a valve in the bottom of the case, from which it passes to waste.

By means of linkage connected with a fixed support and the brake case, an automatic adjustment of the inlet and outlet valves, according to the position of the lever, is secured without affecting the mean moment on the brake case. And this also affords means of adjusting the position of the lever. To admit of adjustment for wear, the shaft is coned over that portion which passes through the bushes, the bushes being similarly coned, and screwed into short sleeves on the casing, so that by unscrewing them the wear can be followed up and leakage prevented.

The brake levers for carrying the load and balance weight, are such as to allow the load to be suspended from a groove parallel to the shaft, at 4 feet from the shaft, by a carrier with a knife edge, the carrier and the weights each being adjusted to 25 lbs. (shown figs. 1 and 4). In addition to this load, a weight is suspended from a knife edge on the lever nearer the shaft, this weight being the piston of a dash-pot in which it hangs freely, except for the viscous resistance of the oil. This weight being adjusted to exert a moment of 100 ft.-lbs., and again a travelling weight of 48 lbs., is carried on the lever and worked by a screw with ¼ inch pitch, so that one turn changes the moment by 2 ft.-lbs., while a scale on the lever shows the position. A shorter lever on the opposite side of the case carries a weight of 74·6 lbs., which is adjusted to balance the lever and sliding weight when the load is removed.

The Accuracy of the Brake.

5. The principle of these hydraulic dynamometers is that when moment of momentum is introduced into a fixed space without altering the moment of momentum within that space, the rate at which moment of momentum leaves the space must equal the rate at which it enters. The brake-wheel imparts moment of momentum to the water within the case, and the friction of the shaft imparts moment of momentum to the case. The water in the case, when its moment of momentum is steady, imparts moment of momentum to the case as fast as it receives it, and the time mean of the moment of the load is equal to the time mean of the moment of the effort of the shaft.

This is not affected by water entering and leaving the case at equal rates, provided it enters and leaves radially.

The condition of steadiness is, however, essential, in order that the moment of effort shall be at each instant equal to the moment of resistance on the case; any change in the moment of momentum of the water in the case being the result of the difference of the moment of effort on the shaft and that of resistance on the case.

The Time-Mean of the Moment of Effort.

6. When, however, the shaft is run over an interval of time, the mean moment of resistance on the case, less the difference of the moments of momentum of the water, at the end and beginning of the interval, divided by the time, is the time-mean moment of effort on the shaft.

The possible limit of this error may be estimated when the maximum moment of momentum of the water is known as well as the minimum moment of resistance, and the minimum interval of time.

Thus taking the limits to be 30 lbs. of water, with radius of gyration 0·66 foot, at 300 revolutions a minute (< 14), the interval of running 3600 seconds, the moment of the load 400 ft.-lbs., the limit of the time-mean of change of moment of momentum of the water is 14/3600, and this divided by the mean moment of resistance gives as the limits of relative error,

$\pm 0{\cdot}00001$. This is supposing the whole of the water to be absent at the beginning or end of the trial, while the actual difference never amounts to more than 2 or 3 lbs., so that the limits do not exceed $0{\cdot}000001$, which is neglected.

The Angle-Mean of the Moment of Effort on the Shaft.

7. As already pointed out in Art. 4, when both the angular velocity of, and the moment of effort on, the shaft are subject to fluctuations of speed, the time-mean of the moment of effort may differ from the angle-mean. This applies to all brakes, but in hydraulic brakes, in which the resistance is proportional to the square of the speed, although lagging by an unknown interval, it becomes possible to estimate the possible limits of this error when the limits of fluctuation of speed are known.

Taking ω the angular velocity of the shaft and ω_0 the time-mean of the angular velocity, $2a^2\omega_0$ the extreme differences of speed, and assuming the variation to be harmonic,

$$\omega = \omega_0 \{1 + a^2 \cos n (t - T_1)\} \quad \ldots\ldots\ldots\ldots(1),$$

$$\omega^2 = \omega_0^2 \left\{1 + \frac{a^4}{2} + 2a^2 \cos n (t - T_1) + \tfrac{1}{2}a^4 \cos 2n (t - T)\right\} \quad \ldots\ldots(2).$$

Then to a second approximation, neglecting a^6, if T_2 is the interval of lagging in the resistance, and M the moment of resistance at the time t,

$$M = M_0 \{1 + 2a^2 \cos n (t - T_1 - T_2) + \tfrac{1}{2}a^4 \cos 2n (t - T_1 - T_2)\} \quad \ldots(3),$$

where M_0 is the time-mean of the moment of resistance. Also the rate at which work is done with uniform velocity, is $M\omega_0$, of which the mean is $M_0\omega_0$, and is the rate of work as measured by the mean moment on the case, multiplied by the mean-angular velocity.

To a second approximation the rate of work with varying speed is

$$M\omega = M_0\omega_0 \{1 + 2a^2 \cos n (t - T_1 - T_2) + \tfrac{1}{2}a^4 \cos 2n (t - T_1 - T_2)\}$$
$$\{1 + a_2 \cos n (t - T_1)\} \quad \ldots\ldots(4),$$

and from this it appears that the mean rate of work is

$$\omega_0 M_0 (1 + a^4 \cos nT_2),$$

which shows that the relative error in taking this as $M_0\omega_0$ is $+ a^4 \cos nT_2$. Thus the error arising from fluctuations in speed of $2a^2\omega$ is within the limits $\pm a^4$, when the resistance varies as the square of the speed, as in the hydraulic brakes.

Where, as in the brake under consideration, there is an automatic adjustment, by which the quantity of water in the brakes is adjusted to the speed, so as to maintain the resistance constant, there will be no error caused by such gradual variations of speed as result from changes in the boiler pressure, since the automatic adjustment can keep pace with them. But it takes time for the water to get in and out, and any variations, so rapid that, owing to the inertia of the brake case with its load, their effect has been reversed before the case has moved sufficiently to affect the water in the brake, will produce errors.

Such cyclic variations of speed attend all motions derived from reciprocating engines, and it is only these, and not the secular variations, that produce errors.

The Variations in the Speed of Rotation of the Steam-Engine.

8. The cyclic variations all go through one or two complete periods in the time of revolution of the engine, and are approximately simple harmonic functions of the time.

They arise from three distinct causes:—

(1) The varying energy of motion of the reciprocating parts;

(2) The varying moment of the effort of the steam pressures on the cranks;

(3) The effect of gravitation on the unbalanced parts in the engine.

In the case of a simple vertical engine, unbalanced and working with moderate expansion, these variations of speed may be severally estimated when I, the moment of inertia of the revolving parts, r the half-stroke of the reciprocating parts, and W the weight of these parts are known, together with N the number of revolutions per minute, and U the work done per stroke.

For, considering the variations as existing separately, we may assume that the angular motion would be steady but for the particular effect, thus:

(1) The moment of effort on the crank being constant, and the resistance constant, and equal to the effort, the energy of motion of all the parts is constant.

Putting $\omega = 2\pi N/60$, and $i = r^2 W/g$,

$$\tfrac{1}{2} I\omega^2 + \tfrac{1}{2} i\omega^2 \sin^2 nt = C,$$

where C is constant, t is the time since the axis of the crank-pin has crossed the axis of the cylinder and n is ω_0, the mean value of ω or $2\pi N/60$.

Whence neglecting i as compared with I, the extreme variation of ω is approximately

$$2a_1^2\omega_0 = \tfrac{1}{2}\frac{i}{I}\omega_0$$

whence

$$a_1^2 = \tfrac{1}{4}\frac{i}{I}.$$

(2) In the same way, considering the effect of the crank effort alone, with a moderate expansion, the energy that has to be absorbed and given out by the revolving parts is about one-fourth part of the work per stroke, and

$$\tfrac{1}{2}I\omega^2 - \tfrac{1}{8}U\cos 2n(t-T) = C,$$

where nT, say $\frac{\pi}{3}$ is the angle of the crank at which ω^2 is a minimum.

The extreme fluctuations in velocity are

$$2a_2^2\omega_0 = \frac{U}{4}\frac{\omega_0}{I\omega_0^2}, \qquad a_2^2 = \tfrac{1}{8}\frac{U}{I\omega_0^2},$$

$$\omega = \omega_0\left\{1 + \frac{U}{8I\omega_0^2}\cos 2(nt - \tfrac{1}{3}\pi)\right\}.$$

(3) The effect of the weight of the reciprocating parts acting alone, causes a fluctuation on the revolving parts of $2rW$; thus approximately

$$\tfrac{1}{2}I\omega^2 - rW\cos nt = C,$$

and

$$\omega = \omega_0\left(1 + \frac{Wr}{I\omega_0^2}\cos nt\right),$$

giving an extreme fluctuation on the angular velocity of

$$2a_3^2\omega_0 = 2\frac{Wr}{I\omega_0^2}\omega_0.$$

The equation of velocity is thus approximately expressed by

$$\omega = \omega_0\left[1 + \frac{1}{4}\frac{i}{I}\cos 2nt + \frac{U}{8I\omega_0^2}\cos 2(nt - \tfrac{1}{3}\pi) + \frac{Wr}{I\omega_0^2}\cos nt\right].$$

In the low-pressure engine used in these experiments, the values of the several quantities are, the units being linear feet, lb., seconds,

$$I = 126, \quad i = 2{\cdot}47, \quad r = 0{\cdot}625, \quad W = 200, \quad rW = 125, \quad U = 1650,$$

$$\frac{1}{4}\frac{i}{I} = 0{\cdot}0049, \quad \frac{U}{8I\omega_0^2} = \frac{148}{N^2}, \quad \frac{rW}{I\omega_0^2} = \frac{90}{N^2},$$

whence, substituting

$$\omega = \omega_0 \left(1 + 0{\cdot}0049 \cos 2\omega_0 t + \frac{148}{N^2} \cos 2\left(\omega_0 t - \frac{\pi}{3}\right) + \frac{90}{N^2} \cos \omega_0 t\right),$$

from this the approximate joint error can be found. But it is sufficient here to show that the individual errors are negligible.

The first gives an error in the mean moment

$$< \pm\, 0{\cdot}000024\, (Ma_1^4).$$

The second and third are inversely proportional to N^4. If N is 300, which is the lowest value,

the second error is between

$$< \pm\, 0{\cdot}0000025\, (Ma_2^4).$$

The third

$$< \pm\, 0{\cdot}0000001\, (Ma_3^2).$$

These are all negligible quantities, and, as the corresponding effects in the high-pressure and intermediate engines, owing to the cranks being set at angles of 60°, would only be to compensate those of the low-pressure engine, the greatest error would not exceed $\frac{1}{40000}$th part.

9. Besides the errors resulting from the terminal differences in the moment of momentum of the water and the fluctuations of speed in the engine, error in the measurement of the work may arise from imperfect balance of the brake, from the frictional resistance of the automatic gear, from unequal resistance in rising and falling of the piston of the dash-pot, and from the end oscillation of the brake.

The Error of Balance of the Brake.

Although, when the shaft is running, the brake levers are perfectly free between the stops, yielding to the slightest force even when carrying a load of 400 pounds in addition to the weight of the brake-case of over 300 pounds, yet, when the shaft is standing, it requires a moment of some 40 ft.-lbs. to move the lever in either direction, so that the balance can only be obtained as the difference of these moments, and this can only be obtained to about 1 foot pound. But, it is to be noticed that as long as the distribution of weights is unaltered and the lever is in the same position, any error of balance, whatever might be its cause, would be the same for all trials, no matter what might be the difference in the suspended load; so that, in taking the difference of the trials, the error would be eliminated, and, to insure this, the automatic adjustment was so arranged that, by a screw adjustment, the lever could be raised or lowered without affecting the automatic adjustment of the valves (see fig. 4, p. 609). Also an index was arranged adjacent to the end of the lever, to which it might be always adjusted (shown in Plates 2 and 3).

The Error of Balance resulting from Friction of the Automatic Gear.

This had been a matter of serious consideration in designing the brakes, for, although it was obviously possible to so balance the parts of such gear that there should be no pressure, arising from the weight of this gear, against the fixed support, it was not obvious that the friction of these valves and their gear would not allow of a steady resistance to motion being maintained, that is, would not allow the brake to lean against the fixed support within the limits of friction. However, after careful consideration of various contrivances, I came to the conclusion that, if the gearing between the support and the valve were inelastic, the joints being an easy fit, the tremor of the shaft and the brake, when running, might be depended upon to release any frictional resistance in this gear; so that, after any change, the gear would rapidly return to equilibrium. This proved to be the case, even to an unexpected extent, as was shown by the freedom of all the pins.

It was subsequently found by experiment that, even when the valves were so tight that it required a moment of 30 ft.-lbs. on the brake to move the automatic gear alone, with the shaft standing, in either direction, when the shaft was running any tendency to lean upon the support in either direction was the result of imperfect balance in the gear; and that, by adjusting this balance to an extent which would not cause a moment on the brake of 0·01 ft.-lb., the tendency of the brake to lean either in one direction or the other might be reversed, showing that, with a load of 600 ft.-lbs., the relative limits of error are $< \pm 0{\cdot}000016$, and in the difference of the trials would be zero.

The Work done in the Brake by End Play in the Shaft.

The clearance in the brake-case would allow of nearly $\frac{1}{32}$-inch end play along the shaft; and when the brake is running, owing to the slight *end* play of the engine-shaft, there is at times a slight backwards-and-forwards movement, in the period of the engine, of the brake-case on the shaft, but not more than the 64th of an inch at the greatest. This end play, when it existed at 300 revolutions and 1200 ft.-lbs. load, could always be prevented by an end pressure on the case of < 50 lbs. Hence the limit of work done on the brake is $< 2 \times 50/12 \times 64 = 0{\cdot}13$ ft.-lb., which, compared with the work in one revolution with a load of 1200 ft.-lbs., is

$$0{\cdot}13/1200 \times 2\pi = 0{\cdot}000017.$$

This would be the limit if the error is proportional to the load, while if constant, the error on the difference of two trials would be zero; so that the greatest relative error is less than

$$+\,0{\cdot}000017.$$

The Error from the Dash-Pot.

Since the piston is suspended freely in the oil-cylinder, and the resistance of the oil is viscous and expressed by $\mu vs/a$, where μ is the coefficient of viscosity, v the velocity of the piston, s the area of surface, and a the distance between the surfaces, the total resistance is thus $\mu s/a$ multiplied by the total displacement (which never exceeds 0·1 ft.) divided by the time (3600 seconds). This is infinitesimal. Besides which, with 1200 or 600 ft.-lbs. load at 300 revolutions, the lever remains perceptibly steady, there being no vertical vibration perceptible to the finger on the lever. Hence, as long as there are no oscillations, the limit of error from the dash-pot, if any, is imperceptibly small.

The only circumstances under which the lever oscillates is when the water flowing through is less than about 4 lbs. a minute; then a slow oscillation appears, the lever moving some half-inch, which causes the automatic gear to lean on the fixed support, and may cause a small error.

The Development of the Thermal Measurements.

The appliances were originally designed, in 1887, solely for the purpose of the study of the action of steam in the engines, and certain problems in hydraulics and dynamometry, without any intention of their being used for the purpose of measuring the heat equivalent of the work absorbed, but rather the other way.

It was, of course, obvious that, as the primary purpose of the brakes was to afford accurate measurement of the work spent in heating water, it was only necessary to measure the change of temperature of the water between entering and leaving the brake, as well as its quantity, to obtain an approximate estimate of the heat equivalent of the work done. But the recognition of the extreme difficulty of obtaining any first-hand assurance as to the accuracy of scales of thermometers, and the fear of creating erroneous impressions as to the value of the equivalent, made me reluctant to allow such determinations. For this reason, as well as to avoid complicating the brake, in the first instance I made no provision for the introduction of thermometers, as may be seen in Plate 1.

But, after the engines and brake had been in use for two years, and had been found to possess attributes in steadiness of running, delicacy of adjustment and balance, beyond what I had dared to expect, and particularly in being able to work with an almost absolutely steady supply of water between steady temperatures, and the same temperatures for different powers, arising either from differences of speed, or differences of load, I realized that by working with the same thermometers on the same parts of their scales, and with the same loads and temperatures at different speeds, since the relative error of balance would be the same, if the surrounding temperatures were the same, the difference of two trials would afford the means of determining the loss of heat by radiation, and, this being determined, the difference of two trials made at the same temperatures as the previous trials, and both at the same speeds, but with different loads, would afford data for determining the error of balance without introducing the value of the equivalent or the use of the scales of the thermometers, except to identify equal temperatures.

I then yielded to the very general wish on the part of those who worked in the laboratory, and added such provision to the brake on the low-pressure engine as would admit of the measurement of the heat carried away by the effluent water, but only for the purpose of verifying the accuracy of balance as determined by mechanical means.

The Thermal Verification of the Balance of the Brakes.

10. The desirability of such independent determination of the balance arose in the first instance from the circumstances already described (Art. 9), viz., that the statical balance could only be determined to 1 ft.-lb., while the absence of effect from the friction of the automatic gear, &c., was only arrived at by somewhat complicated considerations.

The supply of water to the brake came from the service tank, 10 feet above the floor, and 7 feet above the shaft, the tank being supplied direct from the town main, and regulated by a ball-cock. The pipe from the tank passes beneath the concrete floor to a point conveniently close to the brake, whence a branch, in which is a hand-cock, rises vertically to a height of 4 feet above the floor, at which height is the automatic inlet valve, and from this the pipe is bent over, so that its mouth is directly over the inlet opening into the brake, with which the pipe is connected by a flexible indiarubber tube.

The first provision made for measuring the temperature of the entering water was an opening in the bend of the pipe over the inlet valve, with a vertical ⅜-inch brass tube soldered in, about 4 inches long. This admitted of an indiarubber cork, through the centre of which a thermometer was passed into the pipe, as shown in Fig. 5. This was afterward replaced by a glass thermometer chamber, as shown in Plate 3.

Fig. 5.

To measure the temperature of the water leaving the brake it was necessary, by means of a pipe fixed to the mouth of the outlet valve, to bring the effluent water above the balancing lever of the brake, and to one side of it. This pipe was arranged so as to admit the introduction of a vertical thermometer into the ascending pipe, much in the same way as the other. In the first instance, the extension passage and the thermometer were all rigidly attached to the brake, and moved with it, which entailed a re-balance of the brake. Subsequently another arrangement was made. The thermometers used were divided to one-fifth of a degree Fahrenheit; they were both immersed in the flowing water to within a few degrees of the top of the mercury. They were compared at equal temperature, but otherwise subjected to no tests for accuracy of scale.

In making the experiments the link connecting the inlet valve with the automatic gear was removed and the valve was set open, the supply being adjusted by the hand-cock below. The head on the inlet being constant, when the cock was set the flow was practically steady. The quantity of water in the brake then depended on the outlet valve, which, with the exception of a little trouble at starting and stopping, soon overcome, kept the brake lever steady.

To catch the water after leaving the outflow thermometer, the extension pipe turned horizontally over the lever and then turned downwards into a basin, the lip of which was above the mouth of the pipe, and from the basin flowed in a short trough, from which it was caught in buckets. In these it was taken to the scales and carefully weighed. This was a primitive arrangement, and required several assistants, but was found capable of considerable accuracy up to about 40 lbs. a minute.

In making these experiments the engines were kept running at nearly constant speed by keeping constant pressure in the boiler. The speed being indicated on the speed gauge as well as recorded on the counter.

The water entering the brake, coming, as it did, from the town's main, was at nearly constant temperature between 40° and 50° Fahr., according to the time of the year, and varying less than a degree throughout several trials.

The rise of temperature was adjusted by the quantity of water admitted, according to the work, so that the final temperatures as well as the initial were as nearly as possible the same in the different trials.

This rise was such as admitted of the temperature of the brake being the same as that of the laboratory, which could always be adjusted to about 70° Fahr., so that the rise was from 25 to 30 degrees. This, with 40 lbs. a minute, required from 25 to 30 H.-P.

Before commencing the actual trial everything was adjusted, and the engines running with steady load and steady speed until the thermometer showed the heat to be steady at the desired temperature, then, at a signal, the counter was put in and the water caught, each of the thermometers, and one giving the temperature of the laboratory, being then read at minute intervals over 15 or 30 minutes, when, on a signal, the counter was removed and also the last bucket.

The results of these tests were very consistent, within about 0·3 per cent. which was within the limits of accuracy then aimed at.

Trials with equal loads and different speeds showed that the loss by radiation was very small, while those at the same speed with different loads showed the balance was within the limits determined by mechanical tests.

In these trials the only correction was that for the lubricating water which escaped from the brake bushes. This was caught at each bearing, and the temperature taken so that the heat might be added, this being seldom more than 3 per cent. It may also be noticed that in these trials the heat lost or gained by conduction to or from the shaft was included in the radiation. As the brake is on an overhanging shaft which extends no farther than the outer bush of the brake case (Plate 1), the only conduction is on the side at which the shaft is continuous, where the brake bush is only some 4 inches from the brass of the shaft bearing. As the temperature of the brake on this side, which is opposite to that at which the cold water enters, was kept by the lubricating water at the temperature at which the water left the brake, and this was at the temperature of the laboratory, there would be no cause of conduction unless the friction of the shaft in its bearing caused its temperature to rise above that of the laboratory. When the lubrication was good this was small, although on one or two occasions it made itself felt.

The Idea of Raising the Temperature from 32 *to* 212.

11. These tests became an annual exercise in the laboratory, and a very instructive exercise. But, as the subject—the value of the equivalent—was attracting much attention, the desire to obtain measures of it from these trials, by those engaged in them, resulted in Mr T. E. Stanton, M.Sc., then Senior Demonstrator, effecting, for his own satisfaction, a comparison of the scales of the thermometers used in the experiments with a thermometer used in the Physical Laboratory, which had been compared with the air thermometer, and introduced these corrections into the results of the trials, which so gave values very close to what might be expected. I could not see however that determinations made with thermometers so corrected could have any intrinsic value, but, as the matter was exciting great interest in the laboratory, I carefully considered the conditions which would be necessary in order to render the great facilities, which this brake was thus seen to afford, available for an independent determination.

The institution of an air thermometer was carefully considered and rejected. But it occurred to me that it might be possible to avoid the introduction of scales of the thermometers, just as before, and yet obtain the result. If it could be so arranged that the water should enter the brake at the temperature of melting ice and leave it at the temperature of water boiling under the standard pressure, all that would be required of the thermometers would be the identification of these temperatures. At first the difficulties appeared to be very formidable. But on trying, by gradually restricting the supply of water to the brake when it was absorbing some 60 H.-P., and finding that it ran quite steadily with its automatic adjustment till the temperature of the effluent water was within 3° or 4° of 212° Fahr., I further considered the matter and formed preliminary designs for what seemed the most essential appliances to meet the altered circumstances.

These involved—

(1) An artificial atmosphere, or a means of maintaining a steady air pressure in the air passages of the brake of something like one-third of an atmosphere above that of the atmosphere.

(2) A circulating pump and water cooler, by which the entering water (some 30 lbs. a minute) could be forced through the cooler and into the brake, at a temperature of 32°, having been cooled by ice from the temperature of the town main.

(3) A condenser by which the effluent water leaving the brake at

212° Fahr. might be cooled down to atmospheric temperature before being discharged into the atmosphere and weighed.

(4) Such alteration in the manner of supporting the brake on the shaft as would prevent excess of leakage from the bushes in consequence of the greater pressure of the air in the brake, since not only would the leaks be increased, but when the rise of temperature of the water was increased to 180°, the quantity for any power would be diminished to one-sixth part of what it would be for 30°, so that any leakage would have six times the relative importance.

(5) Some means which would afford assurance of the elimination of the radiation and conduction, as, with a rise of 140° Fahr. above that of the laboratory, these would probably amount to two or three per cent. of the total heat.

(6) Scales for greater facility and accuracy in weighing the water, with a switch actuated by the counter.

(7) A pressure gauge or barometer, by which the standard pressure for the boiling point might be readily determined at 3° or 4° Fahr. above and below the boiling point, so as to admit of the ready and frequent correction of the thermometers used for identifying the temperature of the effluent water.

(8) Some means of determining the terminal differences of temperature and quantity of water in the brake, which would be relatively six times larger with a rise of 180° than with 30°.

The Special Appliances and Preliminaries of the Research.

12. Having convinced myself by preliminary designs, not only of the practicability of the appliances, but also of the possibility of their inclusion in this already much occupied space adjacent to the brake, there still remained much to be done in the way of experimental investigation to obtain data from which the requisite proportions of these appliances could be determined, and these preliminary investigations were not commenced till the summer of 1894, when Mr Moorby undertook to devote himself to the research.

Weighing Machine and Tank.

13. The first step consisted in obtaining a somewhat special table weighing machine (Plates 2 to 4), having two rider weights on independent

scales, one divided to 100 lbs. from 0 to 2200, the other to 1 lb. from 0 to 100. Also a galvanized iron tank, $5' \times 2'\,9'' \times 2'\,9''$, capable of holding above one ton of water, with a 4-inch screw valve at the bottom, opening inwards by a handle above the top of the tank, the top of the tank being covered with carefully fitted, but separate, $\frac{1}{2}$-inch pine boards, previously steeped in melted paraffin-wax, to prevent adhesion or absorption of water. This machine and tank, which is a large affair, was placed in the only position available, opposite the end of the shaft and behind the standing pipes for supplying the condensing water to the engine, thus leaving the passage between these pipes and the end of the shaft open, an important matter, as this passage was the only place from which the observations on the brakes could be made. This entailed the carrying the outflow from the brake over the passage, about 6 feet 6 inches from the floor.

Design of the Outflow.

14. This extension of the pipe further entailed the necessity of making this pipe a fixture, and connecting it with the outlet below the automatic cock by a *bent* wire-bound flexible indiarubber pipe, so as to prevent any moment on the brake. (See Fig. 6.)

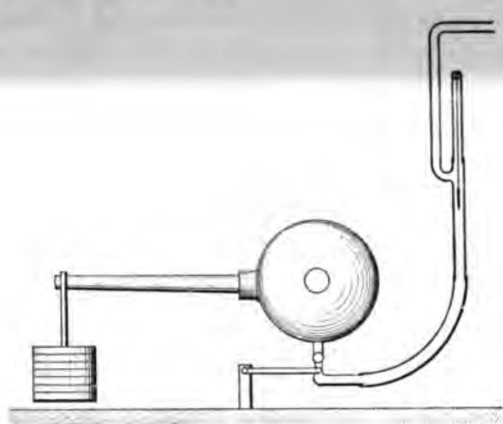


Fig. 6.

The Thermometer Chambers.

15. A glass chamber for the outflow thermometer was introduced as shown (Fig. 6), and another for the inlet, somewhat similar. These were arranged so that the bulbs of the thermometers were down in the full current while the scale was in the glass tube, through which a portion of the water was allowed to flow, that from the inlet thermometer being con-

ducted away to waste, while that from the outlet was conducted back again into the outflow pipe. In this way, not only the bulbs of the thermometers, but the entire thermometers were immersed in the flowing water.

The Two-way Switch.

16. A switch, as shown in Plate 3, was also constructed for diverting suddenly the stream of effluent water from waste to the tank, or *vice versâ*, without exposing the stream for more than an inch, and without any splashing or uncertainty.

Experience in Making Observations.

17. When these arrangements were completed, and whilst the other appliances were progressing, Mr Moorby commenced a series of experiments similar to those which had been previously made, using the water from the tank at the temperature of the town's water, and raising it to temperatures which were successively increased. This was with a view of testing the improved facilities, and also of gaining experience and facility in making and recording the observations.

The engines and brakes were occupied two or three times a week in the ordinary work of the laboratory, so that there were only one or two days a week available for these experiments, and every opportunity was valuable.

The Design of the Condenser.

18. At the same time he made experiments to determine the necessary length of pipe in order that the water flowing along it at the rate of 20 lbs. a minute would be cooled from 212° to 70°, when the pipe was jacketed by a stream of town's water at 50° Fahr.; by the result of which experiments the condenser in which the effluent water is cooled to 75° was designed (Plates 2 to 5).

Design of the Ice-Cooler.

19. To cool the water to 32°, or as near as practicable, I had, on account of the danger of some ice being carried through with the water if the ice were once put into the water, decided to pass the water through a long coil of ordinary water piping, immersed in water, towards the top of a tank with ice under the coil, and from experiments made by Mr Moorby, I decided on the coil and arrangements shown. The coil consists of $\frac{3}{8}$-inch composition pipe, 200 feet long, the tank being 2 feet 6 inches wide and deep and 4 feet long, the

coil being placed near the surface of the water on a shelf, with a wire netted space at the end for the introduction of the ice, which is pushed down under the shelf, and with a paddle which is kept in continual motion by

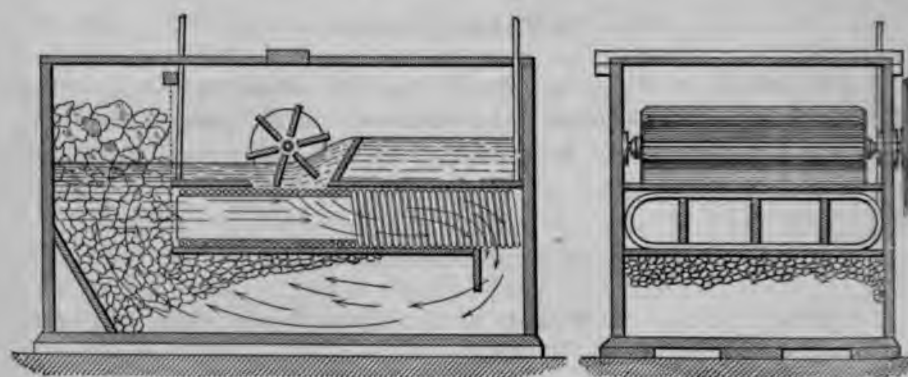


Fig. 7.

a cord from the line shaft, thus securing a rapid circulation of the water. The tank is constructed of 1-inch pine saturated with paraffin wax, in preference to a metal tank.

In this design account had to be taken of the requisite head of water necessary to force some 20 lbs. a minute through the coil. It was estimated that this would require some 30 lbs. on the square inch, which, together with the 5 lbs. excess of pressure in the brake above the atmosphere, and a margin of some 25 lbs. in order to secure steadiness of flow, made a total of 60 lbs. on the square inch, or 122 feet of head.

The Circulating Pump.

20. It was essential that this head should be approximately steady, and under control during the trials, also that the water should be drawn as directly as possible from the town's mains, in order to secure both the low temperature and great purity of this water. This precluded the direct use of the water from the large tank in the tower, which would otherwise have just afforded this head. It also precluded the use of such head as there might be in the town's mains, as this was insufficient and continually varying, so that some special means of imparting the steady head to the water after drawing it from the mains was necessary. This involved pumping the water through the ice-cooler and brake. It might be done by pumping it from the service tank in the laboratory into an accumulator under a constant load, or by passing the water through a centrifugal pump, running at a steady speed, on its way to the brake.

The facilities in the laboratory decided this question. There already existed the quadruple vortex turbine, with four three-inch wheels in series, worked from the water in the tower, which would work steadily up to 1 h.-p., in a position which would be convenient for driving a centrifugal pump in the in-circuit of the pipe leading to the brake; I also had a quintuple centrifugal pump with five $1\frac{1}{2}$-inch wheels in series which was adapted to the purpose. It was decided, therefore, to lead the water from the surface tank, 9 feet above the floor, into the quintuple pump, driven by the turbine under a constant and controllable head, so that the head would be raised to the required amount. Then, to lead the water through the cooling coils to a pressure gauge close to the brake, and thence through a regulating valve into the passage, with the thermometer, leading into the brake. (See Plates 4 and 5.)

The Outlet from the Condenser.

21. In order to prevent the formation of steam, owing to the presence of air in the water, before it had passed the outlet thermometer, it was necessary to maintain a certain pressure in the effluent water as it passed the bulb of this thermometer. At first it was thought that a head of 5 or 6 feet would suffice. In order to secure this, the level of the condenser being some 3 feet above the bulb, the pipe leading from the condenser was carried up vertically about 3 feet higher, then turned over and led down again to an orifice immediately over the switch, while from the top of the bend a vertical branch extended upwards about 3 feet, with its mouth open, to the air. This was subsequently raised. (See Plate 2.)

Preliminary Experiments at 212° under Pressure.

22. The preliminary investigations and the construction of the appliances so far described were not completed till May, 1895. It then became possible to make some experiments as to the working of the brake under pressure and at high temperature, so as to obtain guidance as to the artificial atmosphere and means of controlling the leakages at the bearings. From these experiments two things came out clearly. It was found that all that was necessary for an artificial atmosphere was to connect the outlet of the air passage on the top of the brake by means of a flexible indiarubber pipe capable of bearing the pressure to a vessel of very moderate capacity.

The Artificial Atmosphere.

23. A tin can, holding about 3 gallons, with the bottom and top coned upwards, and strong enough to stand the full pressure of 60 pounds, was

adopted. The air connection with the can was at the top, at which there were also two side openings, one with a cock, to admit of air being pumped into the can, and the other with a fine screw stop for allowing a slow and

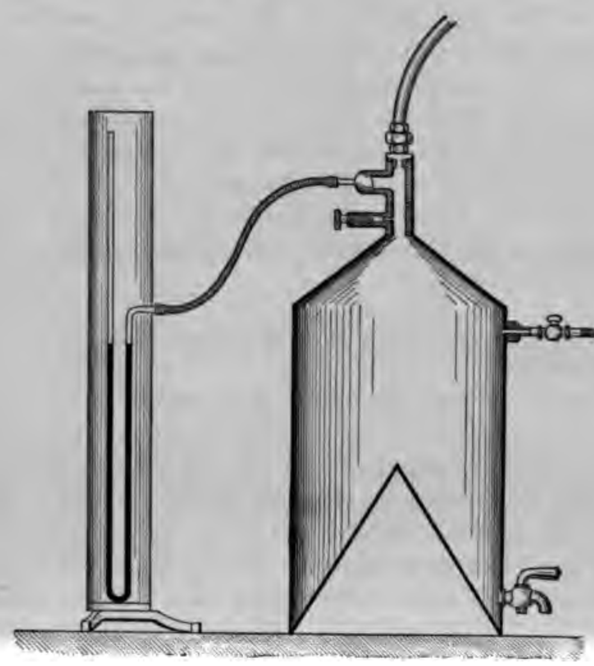


Fig. 8.

definite escape of air. An opening at the bottom, with a cock for drawing off water, was also provided. For forcing the air in, a syringe for inflating bicycle tyres was used in the first instance and proved ample; in fact, when once the pressure was raised, the small amount of air released from the water was more than sufficient to maintain the pressure, so that it was continually allowed to escape.

The Stuffing-box and Cap to prevent Leakage.

24. The thing that was revealed by the experiments at high temperatures, was that the leakage of water at the coned bushes of the brake was so much increased by the pressure within the brake, that even when these bushes were adjusted to run, as close as was practicable, on the cones of the shaft, this leakage was very considerable, so that some other method of controlling this escape became necessary.

This matter threatened to present great difficulties. It was apparently impossible to close in the bushes with stuffing-boxes and stop the leakage altogether, as that would prevent the lubrication of the shaft, and, apart from this, would cause the temperature on the shaft side of the brake to rise to the temperature of the brake, 212° Fahr., which would cause a large escape of heat along the shaft. Besides this, the adaptation of stuffing-boxes to the existing brake presented such difficulties that it almost seemed as though it would be necessary to have a new brake, which, besides the delay, would entail an addition of some £200 to the expenses, which were otherwise very considerable.

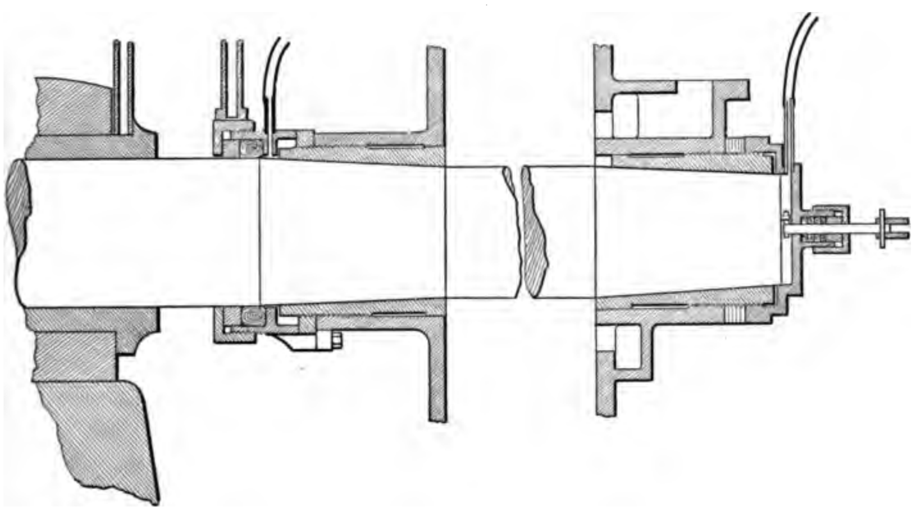


Fig. 9.

To avoid this I determined to try a stuffing-box on the shaft side, constructed in halves to be bolted together on the shaft, and then sweated into one, this stuffing-box to screw on to the exposed screw of the bush, and make a joint against the lock ring; then to open a passage through the box inside the packing-ring, with a tap to control the escape of water, and at the other end to screw a cap on to the bush, entirely inclosing the end of the shaft, with an aperture and a tap to regulate the water, also a small stuffing-box in the cap, to allow of a spindle for connecting the shaft with the counter.

These entailed very difficult and exceptional work, but were beautifully executed by Mr Foster, in the laboratory (Fig. 9).

However, the result was very doubtful, as the water flowing from the brake through the aperture in the stuffing-box not only raised the temperature of the shaft, but was itself of uncertain temperature.

It was in July, 1895, that this experience was obtained, and for a time the success of the research seemed doubtful. During the vacation, however, an idea occurred to me which at once promised to do away with the whole difficulty.

The Cooling and Lubricating of the Bushes.

25. This idea consisted of what seemed to be a practicable plan of forcing a relatively small, but sufficient portion of the ice-cold water into the brake through each of the bearings, the quantities being strictly under control.

That this plan should not have presented itself as soon as the addition of the stuffing-box and the cap were contemplated, becomes intelligible when it is remembered that the main object in the invention of this brake had been to secure a constant pressure in the air space within the vortices, so that by admitting the water through passages in the vanes directly into this air space a constant resistance, whether that of the atmosphere, or artificial atmosphere, on the entering water would be secured, and that the possibility of maintaining an even flow through the brake, so essential to any success in the research, depended entirely on the realization of this constant resistance. Except the inlet passage, the interior of the wheel, and the air space in the vortices, all the spaces in the brake and brake-case are under the full vortex pressure, excepting where, as in the bush on the closed side of the brake, and that between the solid disc faces on the inlet side, the pressure is relaxed by the escape of the water. This vortex pressure depends on the load on the brake, and may be anything up to 25 pounds on the square inch greater than that in the air cores. It thus seemed like starting *de novo* to interfere with this arrangement; and it was only when one came to realize that the possibility of preventing all leakage by the introduction of the stuffing-box and the cap had rendered it possible, by controlled subsidiary supplies under pressure, to reverse the flow of the lubricating water, and so to do away with leakage, and not only to secure lubrication, but also to cool the bushes, and then only after considering the amounts of water required, and the provision in the way of pumping appliances, separate supplies of water and thermometers, &c., that the altered facilities afforded by the circulating pump came to be recognized.

The By-channels and Regulator admitting Cooled Water to the Bushes.

26. Since the main supply must enter, as before, at the same pressure as the air within the vortices, while, in order to reverse the flow through the bushes, that entering the cap must enter at a little, but only a little, above

that of the air within, while that entering on the brake side of the packing-ring in the stuffing-box must enter at any pressure up to 20 lbs., according to the load, above that of the air within, it was clear that there must be three supplies of water at different pressures under separate control; and it was equally clear that these supplies must all be at the same temperature.

Fortunately, the arrangements already made for the new supply afforded ready means of securing these conditions, as, in order to insure steadiness in the supply through the regulating valve, it had been provided, in arranging the pump, that there should be an excess of 20 lbs. on the square inch above that necessary to force the maximum water through the coil and to overcome the air pressure in the brake; also, as the regulating cock was only an inch or two from the thermometer chamber, the water would be subject to little heating by radiation after leaving the cock, while the effect of radiation to the by channels would be of secondary importance, as it is eliminated with the rest of the radiation in the difference of the trials.

It thus became possible, by leading cooled water through two short by-branches, with separate regulators, from the supply pipe, before passing the main regulator respectively into the aperture through the stuffing-box on the inside of packing-ring, and into the cap on the inlet end, to secure controlled inflows of ice-cold water between each of the bushes and the shaft, and so to adjust the temperature of the bearing and insure lubrication of the shaft (Fig. 9).

In order to render such inflows steady and constant, it was desirable that the pressures before passing the regulator should be kept at a considerable and constant quantity above the vortex pressure in the brakes.

From the first preliminary trials made with the branches it appeared that the turbine and pump were capable of supplying sufficient pressure for this, so that the only additions necessary were the branches. These were made of $\frac{1}{4}$-inch brass pipe from the main pipe from the cooler as far as the branch regulators, and thence continued by $\frac{1}{8}$-inch indiarubber vacuum tube $\frac{3}{4}$ inch outside wrapped with tape. The branch regulators have cocks, with provision for fine adjustment, so that the very small quantities which passed might be definitely regulated to great nicety (Plate 3). With these it was found practicable to maintain the temperature of the bushes from anything a few degrees above 32 to any required temperature.

It is to be noticed that the work done by pressure over and above the pressure p_a in the inlet thermometer chamber, is that due to the difference between the pressure in the main pipe before passing the regulators and p_a, through whichever passage the water enters. And since in that water which passes into the thermometer chamber through the main regulator this work has been converted into heat, and is measured as entering heat by the inlet thermometer, the assumption that the water through the branches enters at the pressure p_a, and the temperature given by the inlet thermometer, involves no other error than that resulting from radiation, which is constant for all trials, and is eliminated in the difference.

The Regulation of the Temperature of the Bushes.

27. In the preliminary trials this temperature was only ascertained by touch, and regulated so as to be as nearly as possible that of the laboratory, the branch cocks being set with a definite opening, and the excess of pressure maintained as nearly as possible constant, a plan which was found to give consistent results. But it also appeared that in order to maintain the same temperature in the stuffing-box for the large and small trials with the same pressure in the main pipe, it was necessary to open the branch cock wider in the large trials. This was to be expected from the greater vortex pressure in the large trials. And as owing to the greater resistance of the cooler in the large trials there was difficulty in maintaining a great excess of pressure over the vortex pressure, it was decided to run both large and small trials with the same setting of the cock, and the same head in the cooling pipe, keeping a record until some means was obtained of estimating the comparative slopes of temperature in the shaft in the large and small trials.

The Measurement of the Comparative Slopes of Temperature in the Shaft.

28. The desirability of some more definite knowledge of the slope of temperature in the shaft between the brass of the nearest shaft bearing and the stuffing-box was strongly felt, but it was not at first apparent how this might be done, the shaft being 4 inches in diameter, and the gap between the end of the stuffing-box and the brass of the bearing being only 3 inches.

However, as it became more evident, with the branch cocks set at a constant opening and the same pressures in the supply pipe, that the temperatures in the stuffing-box were greater in the large than in the small trials, and that a small difference in the adjustment of the branch cock to the stuffing-box affected the apparent loss of heat to the extent of some 0·1 or 0·2 per cent. of the total heat, I determined to try and obtain some definite evidence of the relative slopes of temperature in the two trials, by measuring the relative temperatures of the brass and the stuffing-box as far as was practicable. For this purpose, I had thick brass tubes, radiating outwards, sweated on to the end of the stuffing-box, to hold thermometers. Two such tubes were necessary on account of the screwing-up of the box, which had to be done whenever it began to leak; and although this was not done during a trial, one tube would sometimes face downwards, which was inconvenient. In a similar manner two tubes were attached, one to the top and one to the bottom brass of the bearing, holes being bored into the brass and the tubes screwed in. These tubes are shown in Fig. 9.

In this way, with a thermometer in one of the tubes on the stuffing-box and one in each of the tubes on the bearing, although the thermometers might not give the actual temperatures of anything in particular, still the steadiness of the conditions of the brake warranted the conclusion that the differences in the readings of the thermometers would serve to identify similar conditions as to slope of temperature, and this turned out to be the case.

These thermometers threw a flood of light on to conditions which had before been hardly perceptible. Thus, after reading the thermometer during three large trials and three small trials, with the cocks set as before without having been displaced, and with the same pressures, it was found that the mean of the three large trials indicated 13° Fahr. greater slope from the stuffing-box to the brass than that indicated by the mean of the three small trials.

The Constants and Limits of Error of Conduction.

29. It thence became possible in the subsequent trials, by adjusting the cocks, to bring about a mean condition in which the mean slope in the large trials was the same as that in the small, and by comparing the mean results of those trials in which the difference of slope had been in one direction with the mean of those in which it had been in the opposite, to obtain a constant expressing the quantity of heat lost for each degree of the recorded slope.

These thermometers, read to 1° Fahr. 7 times during the trial of each sort, would give a limit of error of the $\frac{1}{7}$ of a degree, which, taking 12 thermal units per hour as the loss per degree, would give as limit of relative error on 100,000 thermal units of, on one trial,

$$0{\cdot}00002,$$

and these being casual, when taken over 40 trials would be less than a millionth.

The Hand-Brake for Regulating the Speed of the Engines.

30. Although it had been found possible to maintain the speed of the engine constant within 2 or 3 per cent. when the engines were working

with a considerable margin of pressure in the boiler, by maintaining the pressure in the boiler constant, the care and attention required on the part of Mr J. Hall, who had charge of the engine, became excessive when the engines were indicating over 80 H.-P., particularly as he could not be attending to the fire and lubrication, and at the same time watching the speed indicator. To meet this difficulty, as there is no known automatic governor which will regulate an engine working against a resistance which is independent of the speed, without fluctuations, I arranged a hand-brake on the rope pulley, 3 feet in diameter, on the brake shaft, to be applied by one of the assistants in the laboratory during the trial. The amount of power to be absorbed by this being less than 2 H.-P. at the most, a ⅜-inch cotton rope, with one end fast, passed round in one of the grooves of the pulley, the other end being attached to a spring balance, the position of which could be regulated with a screw, would answer the purpose (shown in Plate 3).

In this way, as the natural variations of speed of the engines are very slow, Mr Matthews was able, after a little experience, to keep the speed to within something like one revolution, or 0·3 per cent.

The Corrections for the Terminal Heat of the Brake.

31. As the temperature of the effluent water could be continually regulated by regulating the supply of water to the brake, whatever might be the speed, the chief importance of keeping the speed regular arose from the errors (1) caused by small differences of temperature in the brake together with the water it contained at the commencement and end of the trial, and (2) by small differences in the weight of water in the brake at the commencement and end of the trial.

Such errors belong to the class of casual errors to be eliminated in the mean of a number of trials. Still, it seemed desirable to have some assurance that such elimination was effected, and, in order to obtain this, I proposed that the actual quantity of water in the brake for each of the loads used in the experiments should be determined experimentally at several speeds covering the range of variations likely to occur, and so to obtain a curve for each load, showing the water at each particular speed; this to be done by running the brake as in the trials, steadily, at a particular speed, the water passing as in the trial. Then, suddenly, by forcing down the lever, to close the automatic outlet valve, and, at the same time shutting the inlet valve and stopping the engines, and thus trapping the working charge of water in the brake. The water could then be drawn out and weighed.

Putting B for the capacity for heat of the metal of the brake, w for the weight of water, and T for the temperature observed on the effluent thermometer, the total heat in the brake is expressed by

$$(B + w) T^\circ,$$

and, if w_i, T_i° refer to the weight of water and temperature at starting, and w_f, T_f° to the corresponding quantities at the end of the trial, the correction which has to be subtracted from the heat observed is expressed by

$$(B + w_i) T_i^\circ - (B + w_f) T_f^\circ.$$

The Method of Conducting the Trials—Elimination of Radiation.

32. The entire system of working was designed to secure the most perfect elimination of radiation possible. Thus, it was arranged in the first place that the trials be made in pairs, one heavy trial and one light trial, made under circumstances as nearly similar as possible, except in respect of load and water. The loads in the first instance being 1200 and 600 foot-pounds, and the quantities of water such that the final temperature should be as nearly as possible 212° Fahr., and, after the preliminary trials, 300 revolutions per minute was adopted as the speed for all the trials, 60 minutes as the time of running. The inlet and outlet thermometers to be read after the first minute, and every two minutes; also the temperature of the laboratory as shown by a thermometer in a carefully-chosen place. This temperature to be maintained as nearly constant as possible. The setting of the regulators during each trial to be recorded; also the pressure of the artificial atmosphere, and that in the supply pipe after passing the coil; and, subsequently, the reading of the thermometers in the stuffing-box and bearings taken every five minutes, and the speed gauge every two minutes. The observations and incidents being recorded by the rules in surveying, in ink, in a book, and distinct from any reductions. The initial and final reading on the scales and counter being included, as were also the initial and final readings of the inlet and outlet thermometers and speed gauge for the purpose of determining the terminal differences of the heat in the brakes.

As it was impossible to make trials simultaneously, and so secure similar conditions in the laboratory, it was at first arranged that the trials should be made in groups, including four pairs of trials.

The regular work in the laboratory monopolised the engines and brakes on all days in term time, except Mondays and Thursdays, so that the trials were confined to two days in the week. There was a certain likelihood of the state of temperature of the walls and objects in the laboratory being systematically different on the Mondays, after the laboratory had been without steam all Sunday, from what it would be on the Thursday, after the steam had been on for three days. And besides this, there would be a systematic difference in the temperature of all the objects during the first trial in the day, although the brake had been running for an hour before, from that which would hold in the following trials. In the first instance, therefore, it was arranged that a heavy and a light trial should be made on the same day, and a light and a heavy trial on the next available day, under as nearly similar circumstances as possible, except for the inversion of the order. Then again, a light and a heavy trial on the next day, followed by a heavy and a light on the following, so as to break the order and secure the same arrangement, in days of the week as well as in hours of the day, for the four light trials as for the four heavy trials.

As the results of any group of four pairs of trials would furnish a tolerably close approximation to the loss of heat by radiation, assuming this to be proportional to the observed mean difference of temperature between the laboratory and the *brake*, it was easy to obtain an approximate constant, R, for radiation for each degree of difference of temperature, and so to introduce a correction, $R(T_2 - T_a)$, in each trial for the radiation resulting from the observed mean difference of temperature of laboratory and brake, $T_2 - T_a$.

These corrections would serve two purposes—first, affording a better comparison of the results of the separate trials for future guidance, and secondly, by recording the mean difference of temperature, would show how far the mean differences of temperature in the large trials had differed from those in the small trials, and thus how far the radiation had been eliminated.

Lagging the Brakes.

33. In order to obtain still more definite assurance as to the elimination, it was arranged that after consistent results had been obtained in several groups of four pairs of trials, as above, with the brake naked, the brake should be covered with non-conducting material, in the best way practicable, so as greatly to reduce the radiation, at the same time leaving it definite, and then similar trials should be run.

If the coefficient of radiation could in this way be reduced to one-fourth that of the naked brake, such error as there might be remaining in the mean results with the naked brake would be reduced to one-fourth with the lagged brake.

In this, however, there was danger of introducing errors of other kinds.

The non-conducting material would absorb heat slowly and take a long time to arrive at a state of equilibrium, and during the interval the rate of loss of heat from the brake would be irregular. The total error that could result from this cause would be the product of the specific heat of the material used multiplied by the weight, and again by the 75°, or the half of whatever was the difference in temperature of the brake and the air. This decided the choice of the material to include cotton-wool. Two pounds of this would, if not too tightly pressed, cover the brake about $1\frac{1}{2}$ inches thick, and the total heat it would absorb would be less than 0·4 lb. of water raised from 32° to 212° Fahr., and would then be only 0·0008 of the heat generated by 30 H.-P. in an hour, while it would reduce the radiation to about $\frac{1}{7}$. But as the cotton-wool would gradually collapse if subjected to any elastic pressure, it was decided only to use this to such thickness as it could be protected by light cotton strings extending in axial planes round the brake, and to prevent absorption of moisture by the cotton-wool, to cover it with thick anti-rheumatic flannel about 1 inch to $1\frac{1}{2}$ inches in thickness, as shown in Plate 5, which would raise the capacity for heat of the entire lagging to about $\frac{1}{600}$ that of the heat generated in the small trials, and as the brake was kept at steady temperature for about one hour or more before the trial commenced, the actual differences would not exceed some one ten-thousandth part.

The Conduction by the Levers.

This lagging only extended over the body of the brake covering all the brass-work, leaving the levers and balance weights on the levers bare.

These levers being in metallic contact with the brass of the brake assumed at these points the temperature of the brake, and would conduct the heat along to the balance weights till it was lost by radiation. As the temperatures were constant in all the trials this loss of heat would merely form part of the radiation and be eliminated as the rest; but, owing to the masses of the balance weights and the length of the levers, it must take a long time for the balance weights and the further parts of the levers to arrive at a steady temperature, a fact which would account for a greater loss of heat in the first trial made in the day.

In order to obtain assurance that this delay produced no error it was arranged that after the completion of the series of trials with the brakes lagged, corresponding to that with the naked brakes, that the balance weights should be removed, and only the load at 4 feet from the brake left, and a third series of trials made.

Starting and Stopping the Trials.

34. Having adopted an hour as the length of each trial, and 300 revolutions as the normal speed, the engines having been running for an hour previously, while the water entering the brake was being adjusted, and afterwards, so as to ensure the temperature, not only of the brake, but of the surrounding objects, having become approximately steady at the time of starting the trial, all that was necessary was that the counter should be pushed into the gear, and at the same time the water-switch pushed over, and the reverse operation at the end of the trial. These operations, simple

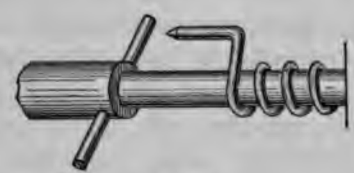


Fig. 10.

as they were, entailed errors, which arose partly from the impossibility of instantaneous engagement of the counter simultaneously with the switching of the water. In order to diminish these as far as possible, the spindle of a counter, on which was the worm which drove the worm wheel, was wrapped with a spiral spring of steel wire, which gripped the spindle so tight that it would not slip, the end of the wire being bent, so as to form a clutch standing off the shaft half-an-inch, the end of the wire being pointed, the shaft of the counter projecting a little beyond the wire. Facing the end of this shaft, and in line with it, was a socket in the end of the engine shaft, which was brought down to three-quarters of an inch diameter and carried two round pins, a sixteenth of an inch diameter, standing out radially, the engagement being effected by pushing the counter forward till the wire crank engaged on one of the pins. (Owing to the wire being pointed and the pins rounded, the chance of the wire striking plumb on to the pin and so preventing engagement was reduced to a minimum.)

This engagement was the result of a great deal of experience, and answered perfectly, but it involved the mean chance of a quarter of a revolution of the engine-shaft after the wire had passed the pin before the actual engagement was effected, whereas on coming off the disengagement was instantaneous, the counter stopping by the friction of the worm before the momentum had carried it through any appreciable angle.

This would leave a mean error of the work done during one-fourth of a revolution on each trial, whence, the number of revolutions during the trial being 18,000, the relative mean correction would be one seventy-two thousandth part, or 0·000013. As, however, when the two operations were executed by different observers on a signal, the personal equations might amount to more than this, although it involved a difficult piece of linkage, an automatic connection was effected, as shown in Plate 3, the pushing of the counter into engagement shifting the switch, so that in making the trials no error was introduced.

The Leakage of Water.

35. As the loss of any of the water, which had entered the brake before it was weighed, would constitute a corresponding error in the results, the perfect tightness, not only of all the fixed joints, but of the casting and the pipes, was a matter of first consideration and of continual care. This was one of the reasons why the lagging was delayed till after consistent results had been obtained; for, as long as the brake and pipes were naked, such leakage could not fail to be observed on close inspection, and before lagging it was arranged to test the brake and pipes to an excess of pressure, so as to insure perfect soundness. Besides the fixed joints there were only two working joints, in addition to the openings into the switch and again into the tank.

(1) The working joints were: The stuffing-box on the main shaft and the stuffing-box on the automatic cock on the outlet from the brake.

Any leakage from these was open to observation both before and after lagging, as they were in no way covered; and arrangements were made so that such leakage could be separately conducted by pipes and caught in bottles. With care such leakage could be reduced to insignificant quantities.

The absolute loss of heat resulting from a leakage of $w_{S.B}$ lbs. of water from the stuffing-box on the shaft was equal to the product of the difference of temperature of the stuffing-box $T_{S.B}{}^\circ$, and inlet ($T_1{}^\circ$) multiplied by $w_{S.B}$,

$$w_{S.B}(T_{S.B}{}^\circ - T_1{}^\circ),$$

and in the few trials in which this became a sensible quantity it was to be added as a correction.

The Loss of Heat by the Leakage of Water from the Automatic Cock.

36. This was the product of (w_c), the weight of water which esc[illegible] multiplied by the total rise of temperature. Since the water passin[illegible] cock was on its way to the high temperature thermometer. where a[illegible] water was caught it was put into the tank, and so re[illegible]

This leakage was very small, at most 2 oz. in a trial, but as there must be some evaporation as the water escaped through the hot gland, which, though small, might be of some importance on account of the latent heat of evaporation, it was desirable in some way to enclose this stuffing-box in an indiarubber bag closing on the spindle, so that the vapour could not escape, and this was eventually accomplished very effectively and neatly by Mr Foster, in a way which did not interfere at all with the free action of the cock (Art. 14, Part II.).

The result of this, besides preventing any subsequent loss of water, in this way, was to show that any error that had previously existed from evaporation was inappreciable.

The Loss of Water at the Switch.

37. Apart from evaporation, which would result from the exposure to the air, and in passing the air gap into the switch, there was no loss, as the water descended almost tangentially on to the surface of the tube on the switch which received it, the switch itself being a vertical knife-edge extension of this surface, which passed through the vertically descending water at starting and stopping; and further, to prevent any minute drops of water going astray from the bursting of an occasional bubble in passing, a sheet brass hood was placed round the descending pipe directly the trial started.

The outside of the weighing tank is completely exposed to observation, and is perfectly tight. The valve in the bottom, being a 4-inch leather-faced screw-valve on a brass seat, is also tight, but for satisfaction it was arranged to place a clean tin dish under the valve before starting a trial, and only to remove it after the water was weighed, so that there should be absolutely no loss of water from any of these causes.

That there must be some loss of water by evaporation to the air as long as the temperature of the water, after leaving the condenser, was above that of the dew-point of the surrounding air, was certain. By using sufficient cooling water it would be possible to bring the temperature down to that of the dew-point; but it was found that this could not be done under all circumstances without a larger condenser, for which room was wanting, and, as long as the water lost by evaporation was the same in both trials, all error would be eliminated in the difference of the large and small trials. After careful consideration, it was arranged that the condensing water should be adjusted so that the water in all trials entered the tank at a temperature as nearly as possible 85°; it being probable, as the surface exposed to the air was nearly the same in the large and small trials, if the differences in temperature between the air and the water were the same, the evaporation would be the same, or would at least differ by a constant amount. In order to test this, it was further arranged that, after the trials were finished, the centrifugal pump should be temporarily re-arranged so that it could be used to draw water out of the tank and force it round through the condenser and switch, and so back again into the tank at rates corresponding to those of the large and small trials, and at the same temperature (85°), the water in the tank being at this temperature, the arrangement of the pump being such that, when stopped, all the water in the pipes would run back again into the tank. This would practically insure the same loss of water by evaporation during one hour's pumping as during one hour's trials, and any difference (w_e) thus established between the large and small trials would then be treated as a standing correction on the difference of the heavy and light trials. This relative correction, taking W as the mean difference of water in the heavy and light trials, would be

$$\frac{w_e}{W}.$$

The Standards of Measurement.

38. In these experiments the expressions obtained for the work done in heating the water and the heat generated are, respectively,

$$2\pi N \,.\, R W_i \text{ and } S W_w (T_2^\circ - T_1^\circ),$$

where R, W, T°, S are respectively length, weight, temperature, and capacity for heat.

Since these expressions both represent the same absolute quantity of energy, the difference in the numerical values of these expressions results only from the difference in the units in the two expressions. These units may be considered as the unit of work and the unit of heat respectively, as it is the inverse ratio of these units, measured in absolute quantities of energy, that is expressed by the ratio obtained from

$$\frac{2\pi N R W_i}{S W_w (T_2 - T_1)}.$$

But, as there are no actual standards either of work or heat with which quantities of work and heat can be respectively compared by a simple measurement, such comparisons can only be accomplished by the comparison of the several factors involved in each of these expressions with the several absolute standards which exist for such factors.

These standards are the standards of mass, length, and force, on the one hand, and of mass, quality of matter, and temperature, on the other.

Thus, work being defined as the mean product of force multiplied by the distance, and the standard of force being the force of gravitation on the unit of mass wherever it occurs, the work is represented by $W.h$, where W expresses the number of units of mass, and h the number of units of length through which it has been raised. Taking (M) and (L) as expressing these units, the unit of work is expressed as (ML).

Again, the unit of heat is defined to be one nth part of that quantity which is required to raise one unit of mass (M) of a standard substance (pure water) from one definite state of temperature to another definite state. And calling this interval θ, the unit of temperature is defined to be θ/n. And, taking S to express the ratio of the number of units of heat required to raise W_ω units of mass of matter from T_1° to T_2° compared with $W_\omega(T_2^\circ - T_1^\circ)$, the heat expressed by $SW_\omega(T_2 - T_1)$ is in units $\left(M\frac{\theta}{n}\right)$.

So that, from the physical equivalence of the absolute energy expressed in the respective forms, it appears that the unit of heat as defined by $\left(M\frac{\theta}{n}\right)$ is equivalent to

$$\frac{2\pi NRW}{SW_\omega(T_2 - T_1)} \text{ units of work as defined by } (ML),$$

or that the heat required to raise one unit of mass of pure water through the definite interval of temperature θ is equivalent to

$$n\frac{2\pi NRW}{SW_\omega(T_2 - T_1)} \text{ units of work } (ML).$$

This is the definition of the mechanical equivalent of heat in Manchester, adopted by Joule, if $n = 1$, and θ is 1° Fahr. between 50 and 60, as determined on his thermometer. But, since the absolute kinetic value of the unit of force as here defined varies with the latitude and height of the place, while that of the unit of heat is constant, this mechanical equivalent varies from place to place with $1/g$, where g is the expression, in kinetic units, for the unit of force (M).

Thus, expressing the work in kinetic units, the unit of heat, as already defined, is equivalent to

$$g\frac{2\pi NRW}{SW_\omega(T_2 - T_1)} = C,$$

where the dimensions of C are $(L^2T^{-2}n\theta^{-1})$.

Whence, since g has dimension (LT^{-2}),

$$\frac{2\pi NRW}{SW_w(T_2-T_1)}=\frac{C}{g},$$

where the dimensions of C/g are $(Ln\theta^{-1})$.

The object in this research being to replace the standard of temperature, as defined by the scale on a particular thermometer, by the standard obtained from the states physically defined by melting ice and by water boiling under a standard pressure, θ is here defined to express this interval, and S is, in accordance with the definition already given, used to express the ratio which the heat required to raise unit mass over any interval, per degree of rise, bears to that required to raise pure water over the interval θ, per degree of rise.

The Standards Involved.

39. It appears from the dimensions of C/g, as obtained in the last article, that the only general standards to which reference need be made are those of length and temperature.

It is, however, to be noticed that the determination of the work and the heat involve the determination of separate masses, and that the units only disappear on the condition that they are equal.

The Measurement of Mass.

40. Since it was not necessary to refer the mass to a general standard, the weights used were only referred to a Board of Trade standard for convenience.

Thirteen of the 25 lb. weights used for loading the brake were adjusted to the Board of Trade weight, then carefully balanced against each other, till, balanced in groups of four in any arrangement, there was less than 0·01 lb. difference. Four of these weights were then taken as the standard.

The compound lever machine, which had two scales on the same lever, one notched to each 100 lbs. for the position of the large rider, the other with a flat scale for every 1 lb. for the position of the small rider, was taken to pieces and the knife edges re-ground and re-set (by Mr Foster) till consistent results were obtained to the one-hundredth of 1 lb. Another rider was made to work on the same scale as the small rider, being adjusted to one-hundredth of the weight, so as to read 0·01 lb.

The scales were then carefully surveyed by the standard 100 lb. weight, the original small rider being adjusted till the difference between its extreme positions on the scale balanced the standard to $< 0{\cdot}01$ lb., and the corrections for each V-notch into which the feather on the large rider fitted ascertained by balancing the standard to a like degree of accuracy.

The dead load on the scales, including the empty tank, came to 340 lbs. about, and between this and 2200 lbs. the scales would weigh any quantity with the lever swinging to 0·01 lb.

The weights to which the scales had been adjusted were then exclusively used on the brake. Thus the brake was balanced by the same weights as were used as the standard in weighing the water, with a sensitiveness which gave the error less than one forty-thousandth part of the weight of water in the smallest trials, while the casual error, which would not exceed this in a single weighing, would be eliminated in the mean of a large number of weighings. Thus the relative limits of error in weighing would not exceed ·000025.

The Correction for the Weight of the Atmosphere.

41. The balances being made in air, it is necessary to add the weight of air displaced in each case.

As the relative weights only are concerned, if D_a is the weight of a unit volume of air, D_w that of water, and D_i that of cast-iron, the weights in air of unit masses are:—

$$1 - D_a/D_w \ldots\ldots\ldots\ldots\ldots\ldots \text{ for water,}$$
$$1 - D_a/D_i \ldots\ldots\ldots\ldots\ldots\ldots \text{ for cast-iron.}$$

The load on the brake is therefore subject to the correction expressed by the factor $(1 - D_a/D_i)$, while that of the water balanced against cast-iron weights, has the correction factor

$$\frac{1 - D_a/D_i}{1 - D_a/D_w},$$

and the relative correction for the actual weight of water, as against the load on the brake in air, is

$$1 \quad \left(1 - \frac{D_a}{D_w}\right) \text{ or approximately } 1 + \frac{D_a}{D_w},$$

for the temperature 67° Fahr., $D_a = 0{\cdot}0752$, $D_w = 624$.

Hence, the relative correction factor for the equivalent is

$$(1 - 0{\cdot}001205).$$

The Correction for g in Latitude of Greenwich and 45°.

42. Since the latitude of Manchester is 53° 29′, Greenwich 51° 29′, the value of g being (*Mémoires sur le Pendule*, Soc. Française de Physique)

$$g_{45°}(1 - 0{\cdot}00259 \cos 2\lambda) = g_{45°}(1 + 0{\cdot}0007558) \text{ at Manchester,}$$

$$\text{,,} \qquad \text{,,} \qquad = g_{45°}(1 + 0{\cdot}0005814) \text{ at Greenwich,}$$

whence the correction factor is......... $(1 + 0{\cdot}0001746)$ at Greenwich,

and for 45° .... $(1 + 0{\cdot}0007558)$.

The Specific Heat of the Water.

The standard capacity for heat being that of distilled water, the obvious course would have been to have used distilled water in the trials, had this been practicable; but as it was apparent from the first that the quantity of water which would have to pass through the brakes during the trials would amount to some 20,000 gallons, or, say, 100 tons, all of which would have to be brought down to a temperature of 32° Fahr.; and that to do this, using distilled water, whether or not the water was used over again, the necessary appliances for producing, storing and cooling the water, were impracticable in the laboratory. the last 40° must be removed with ice, and this would require some 25 or 30 tons of ice. While using the town's water direct from the main, the average temperature, from February to June, would not exceed 45°, so that only 12° or 13° would have to be removed by ice, which would require from 7 to 10 tons, with no appliances except the relatively small appliance for cooling.

The only practical course, therefore, was to use the town's water. And had it not been for the known purity of this, the research would never have been undertaken.

As affording definite assurance of the consistent purity of this water, as delivered in the college, Professor Dixon kindly undertook to furnish the mean results of the analyses which he makes periodically for the Manchester Corporation, of the water drawn from the supply in the college. These show that the impurities are almost negligible, and taking 0·2 as the specific heat of the salts, the relative correction is $0{\cdot}8s$, where s is the relative weight of the salts.

The Effect of Air in the Water.

43. Even distilled water contains air unless special precautions are taken for its removal; so that any effect such air may have on the capacity for heat as measured would not have been avoided by using distilled water.

The direct effect of the same 0·00323 per cent. of air which water exposed to the atmosphere usually contains at normal temperatures, is so small as to be altogether negligible, and it would seem to be an open question whether the standard condition of water, as regards the capacity for heat, does not involve the inclusion of this air. But the indirect effect of such air on the heat necessary to raise water from normal temperatures to near the boiling-point, is by no means negligible.

It does not appear that any definite study has hitherto been made of this effect; but it is a matter of common observation that as water reaches a temperature some 40° Fahr. below the boiling-point, bubbles appear on the sides and bottom of the vessel, which gradually increase in size and rise to the surface, increasing rapidly in size as they rise. The bubbles are usually referred to as bubbles of gas or air. But, a moment's consideration will show that, although the air or gas is the immediate cause of the premature formation and subsequent expansion of the bubble, it is none the less certain that the space occupied by the bubble is filled with saturated steam at the temperature of the water, the function of the air being merely that of balancing the excess of pressure of the surrounding water over the pressure of the saturated steam.

It thus appears that every bubble so formed represents a quantity of heat, which is the latent heat of the volume of the saturated steam in the bubble, over and above the heat of the weight of water in this steam.

Thus, if bubbles of air exist in water at a temperature of 212° Fahr., the weight of air per lb. of water being a, and p the pressure of the water in inches of mercury, then, since the pressure of the air is $p-30$, and the volume of 1 lb. of air at 212° Fahr. under 30 inches of mercury is 16·9 cubic feet, the volume of air per lb. of water is

$$V = \frac{16{\cdot}9 \times 30}{p-30} \times a,$$

or, if $p = 40$, $$V = 50{\cdot}7 \times a.$$

This is the volume, in cubic feet, of saturated steam at 212°; whence, since the latent heat per cubic foot is 36·6 at 212°, the excess of heat will be per lb. of water

$$V \times 36{\cdot}6 = 1855 \times a,$$

and this, divided by 180°, gives a relative error

$$10{\cdot}31 \times a.$$

If $a = 0{\cdot}0000323$, the error is

$$0{\cdot}00033, \text{ or } 0{\cdot}033 \text{ per cent.}$$

The water, after being exposed to the atmosphere in the service reservoir, where it discharges any excess of air, enters the brake cold with this normal air, there it is heated by work, under the pressure of the artificial atmosphere at pressure p, to maintain which it parts with some of the air, which, in passing out into the flexible pipe, carries out saturated steam, which is condensed by radiation from the pipe. The water, with the remainder of the air, is then carried by the centrifugal pressure into the outer chamber in the brake case, under a pressure of about 50 inches of mercury. It then passes the automatic cock, into the flexible pipe, at 41 inches pressure, thence rising to the thermometer bulbs at 40 inches. In passing the automatic cock with a difference of pressure of 9 inches, the pressure will be further reduced, probably 9 inches below that in the pipe, so that any air that might have been retained would come out at that point, and expand further as it approached the thermometer bulb.

In the first instance, it was thought that a pressure of 5 feet of water would prevent the formation of bubbles, and the air gap in the pipe leading from the condenser was placed at this height above the thermometer. But many, and sometimes large, bubbles of air were observed passing up the thermometer chamber; and Mr Moorby observed that he could detect the passage of a large bubble by a fall in the thermometer before the bubble appeared in the glass chamber.

To prevent this, the air-gap was raised till it was 12 feet above the thermometer bulb; so that the error is limited to three ten-thousandths. Even so, as it is much larger than any of the errors of constant sign, it was important to try, by assimilating the conditions under which the water leaves the brake, to obtain experimental evidence which would narrow the limits.

It may appear at first sight as though these losses from the air in the water would, like the radiation, be eliminated in the difference of the large and small trials, but this is not so, since the quantity of heat so lost is proportional to the amount of water used, or it may be greater in the heavy trials.

The Standard of Length.

44. The measures of length that the research involves are—

(1) The horizontal distance of the centres of gravity of the adjustable loads on the brake from the axis of the shaft.

(2) The vertical heights of the barometer at which the boiling-points of the water were determined.

In order to secure a definite reference of these to the British standard, recourse was had to two carefully-preserved and independent measures derived from this standard.

(1) A set of gauges by Sir Joseph Whitworth and Co., consisting of three steel bars, 9, 6 and 3 inches respectively, with parallel plane ends $\frac{3}{4}$ inch in diameter, adapted to a 20,000th of an inch measuring machine, which constitute the standards used in the engineering laboratory.

(2) A brass bar by Elliott and Co., 39 inches long, and graduated in inches, used as the standard in the physical department in Owens College.

From the Whitworth gauges, two steel bars, $\frac{3}{4}$ inch in diameter and 9 inches long, with parallel plane ends, were made by Mr Foster, and compared with the 9-inch Whitworth bar by the measuring machine.

With these and the Whitworth gauges, placed end to end, an outside gauge consisting of two surfaced angle-plates on a surfaced cast-iron bed was set out, and then a steel bar $\frac{3}{4}$ inch in diameter with plane ends fitted to these. Careful comparison showed that this bar did not differ from the sum of the lengths of the gauges by $\frac{3}{10000}$ parts of an inch. This length was then carefully laid off by the surfaced angle plates on the surface plate, and was so compared with the scale of the Elliott brass bar, account being taken of the temperature, and found to agree within less than $\frac{3}{10000}$ of an inch.

The 30-inch bar so obtained was then taken as the standard both for the levers of the brake and the barometer, to be carefully preserved.

Lengths of the Levers.

45. The V-groove, in which the knife-edge of the carrier, by which the load on the brake was suspended, rested, was originally made at a distance of four feet from the axis of the shaft at ordinary temperatures, and as, whatever the error might be when the brakes were hot, it would be the same for all the trials, since the temperatures were the same, it was decided to take this as the length of the levers in estimating the loads during the progress of the research, and to treat whatever error there might be as a standing correction on the final results. Such correction to be obtained by laying off four feet, less the radius of the shaft, from the carefully squared end of a steel plate 3 inches broad and $\frac{3}{16}$ inch thick, then placing this, flat, in a vertical plane perpendicular to the shaft, with its edge horizontal, as near as practicable to the knife-edge groove with the squared end touching

the shaft. Then by means of a theodolite, set so that its line of collimation was in a vertical plane parallel to the axis of the shaft, and intersecting the vertical line on the plate, to observe the distance of the groove from the line on the plate, while the brake was running under the same conditions of temperature, and load as in the trials; but with the carrier temporarily displaced further along the shaft, so as to leave the bottom of the V-groove visible through the theodolite, and in this way to obtain the actual distance of the groove from the axis of the shaft, as affected by the expansion of the brake, and any displacement of the bearing on the shaft which might result from the running.

By using a scale divided to the one-hundredth of an inch, and taking several readings, this could be determined to a thousandth of an inch, so that the limits of accuracy would be

$$\pm 0{\cdot}00002.$$

The Standard of Temperature.

46. As the most general standard is the difference between the two physically fixed points of temperature, corresponding to the temperature of ice melting under the pressure of the atmosphere, and that of water boiling under a pressure corresponding to 760 millims. of ice-cold mercury in the latitude of 45°, taking account of the variation of g, the standard in Manchester is the interval between melting ice and water boiling under a pressure of $760 \times 1{\cdot}0001721$ millim. of ice-cold mercury, which corresponds to 29·899 inches. And this interval divided by 180 is one degree Fahr.

According to Regnault's tables, a divergence of one thousandth of an inch from the boiling point would correspond to an error of 0·0017° Fahr., and this would be less than the one-hundred-thousandth part of 180°.

In order to obtain this degree of accuracy in comparing the pressure of the vapour of pure water, in which thermometers could be placed, with the height of mercury over a range of two or three degrees above, and two or three below the point, at almost any time, irrespective of what might be the actual pressure of the atmosphere, it was necessary that the barometer, or pressure gauge, while in free communication with the vapour chamber, should be shut off from the atmosphere, and at the same time so far removed, that the temperature of the mercury should not be affected by the heat from the gas or boiling water. And, further, although in direct communication with the vapour, this must be such that no moisture could reach the mercury; and, such as involved no current in the passages which might affect the relative pressures, as would result by the interposition of a condensing vessel.

It was also necessary that the arrangements for reading the vertical distances between the upper and lower surfaces of the mercury should not only give absolute differences of height, but also that they should afford ready means of at any time determining the presence of vapour or gas, other than that of mercury, in the upper limb of the barometer.

The Barometer.

47. To meet these requirements, the barometer shown (Plate 8) was designed. The vessel which holds the mercury consists of a bottle-shaped casting of iron, 3 inches in diameter. Through a stuffing-box in the neck of this, the stem of the barometer tube passes. To admit of reading the level of the surface of the mercury in the bottle, two parallel plate-glass windows are arranged, $\frac{3}{4}$ inch diameter, having their axis $\frac{3}{4}$ inch from the axis of the bottle. These are sunk into the casting so as to leave the outer cylindrical surface of the bottle clear, the joints between the glass and the cast-iron being faced and made tight with a trace of beeswax, the other openings into the bottle being one for the admission and abstraction of mercury, fitted with a screwed valve, and one for the admission of air, with a mouthpiece for the attachment of a tube from the vapour chamber.

The glass stem of the barometer is drawn down into a neck towards the lower end, and this is bent through 180° so as to bring the mouth upwards, and thus admit of its introduction into the mercury in the bottle without letting in air. This bend has to be passed through the stuffing-box, then the tube is secured by screwing the gland on to the beeswax stopping. A brass guard tube is then screwed into the neck, to support the glass tube, to a height of 24 inches from the mercury in the vessel.

For reading the height of the lower limb, a cylindrical brass curtain, with a conical contraction on the top, the aperture in which is threaded internally at twenty threads to an inch to correspond to the screw on the outside of the neck of the bottle, is screwed on to this neck, the lip or bottom of the curtain being truly turned so that, when screwed down to the level of the mercury, it cuts off the light through the windows from a white sheet behind.

To the top of the brass casting, which forms the curtain, a brass cylindrical tube is rigidly attached coaxial with the curtain which fits over the brass guard round the barometer tube, this extends to a height of 26 inches from the lower lip, the internal diameter for the last inch being a little smaller and internally screwed at twenty threads to an inch. Into this is screwed a brass tube, externally screwed throughout its length, about 6 inches long, with parallel opposite slots $\frac{1}{8}$ inch wide extending to within an inch at either end, to form windows through which to see the light over the upper limb of the mercury. And on to the upper portion of this tube there is screwed a long cap, capable of screwing down to the bottom of the slot. The lower lip of this cap forms the curtain which cuts off the light when the lip is level with the upper limb of the mercury.

By this arrangement the variation of the distance between the lips of the lower and the upper curtains depends only on the change in their relative angular positions. For, since the slotted tube has a uniform thread, it can be turned, screwing into the lower curtain and out of the upper, both of which remain unmoved. Thus the position of the windows may be fixed, while the curtains are moved. So that for reading the distances it is only necessary to measure the relative angle.

This angle is measured by dividing the circumference of the cap just above the lip into five equal divisions, from 0 to 5, and these again into ten, then a turn through one of the smaller divisions means an alteration in the distance of one-fiftieth of one-twentieth of an inch. As this angle is measured relatively to the lower curtain, a vertical brass scale, divided to tenths and twentieths of an inch, is fixed externally to the top of the extension of the lower curtain, extending vertically just outside the graduated limb of the upper curtain, and thus serves for reading the angular distance of the index mark on the limb of the upper curtain, on any particular thread, and the number of threads from the index on the scale.

The Adjustment of the Indices on the Barometer.

48. The lower curtain, together with the slotted tube and cap, is unscrewed from the neck of the cast-iron bottle and lifted off over the tube. Then the 30-inch standard bar is set on end upright on a surface plate, and the lower curtain, &c., are lowered over the bar until the lower lip of the curtain rests on the surface plate, and the top of the bar is 30 inches from this lip. The cap is then screwed down until light is seen over the top of the bar through the slot just cut off. Then a vertical line drawn on the cap just above the lip, at the edge of the scale, is the index on the cap, and a horizontal line, drawn on the scale level with the lip of the cap, is the index point on the scale. And, when these two lines are brought into this position, the distance between the lips will equal the length of the bar.

In order to check this the curtain is raised, and two thin pieces of chemical paper are placed on the surface plate, one on each side of the bar, so as to leave a space between the paper and the bar. Then the curtain is replaced so that it rests on the paper, and light can be seen through the interval between the paper and the bar. Then light should be seen to an equal extent over the bar, and by screwing down the cap till the light disappears, the thickness of the paper will be measured by the angle turned through.

The construction of this barometer, the first of its kind, was undertaken by Mr Foster, who has produced a very beautiful instrument by which direct reading can be taken to the ten-thousandth of an inch. The mercury having been re-evaporated for the purpose, in an apparatus belonging to Dr Schuster, by his assistant, Mr S. Stanton.

This barometer could be used as a pressure gauge for pressure up to 34 inches and down to 26 inches, and by connecting the mouthpiece with a receiver in connection with a mercury or water syphon gauge, with the other limb open to the atmosphere, the differences of reading of the barometer for different pressures in the receiver can be readily compared with the corresponding differences in the syphon gauge, and by such comparisons, taken at intervals till the mercury reaches the closing in of the tube, a test is obtained as to the absence of anything but mercury vapour above the mercury.

When the barometer is in connection with the vapour chamber in which the thermometer is immersed, the passage of moisture back into the barometer is prevented by connecting the tube by a branch with an air receiver, in which the pressure is maintained higher than that in the vapour chamber; the branch pipe communicating with the chamber through a piece of quarter-inch glass pipe, 3 inches long, plugged as tightly as possible throughout its length with cotton-wool, through which the air has to pass from the receiver into the vapour chamber. In this way, an indefinitely slow current of clean dry air can be maintained into the passage from the vapour chamber to the valve which controls the exit of the steam into the atmosphere, so that the air does not enter the vapour chamber in which the thermometers are, but directly passes out with the overflow steam.

There is necessarily some resistance to the air passing along the pipe to the vapour chamber, but this could easily be tested by removing the pipe from the vapour chamber, and leaving it open to the atmosphere, so that the barometer would adjust itself to that of the atmosphere, plus the pressure due to the resistance of the current in the pipe; then, stopping the current by closing the branch pipe, and reading again, the difference would give the pressure due to the current. With the plug as described this was so small as to be negligible, even when the pressure in the receiver was two atmospheres. As during the testing of the thermometers the pressure in the vapour chamber was generally greater than that of the atmosphere, in order to maintain this steady, a governor on the gas burner was necessary, as well as an accurately adjustable exit valve.

With these appliances the scale of the high temperature thermometer could be tested at intervals, over a sufficient interval on each side of the boiling point (212° Fahr.), the corrections for surface tension, temperature, and gravitation being applied to within the thousandth of an inch of mercury.

This gives the limits of error $\pm 0{\cdot}00001$.

Correction of the Low Temperature Thermometer.

49. The correction on the thermometer for 32° would be at any time obtained in the usual way by immersing the thermometer vertically in a bath of soft snow, but as there was no ready means, as with the scale about 212°, of testing the scale at 32°, while this would be used for one or two degrees, this correction could only be made by comparison with a thermometer already corrected with the air thermometer, which comparison Dr Schuster allowed to be made in the physical department.

Corrections of the Thermometers for Pressure.

50. The pressures in the thermometer chambers of the brake being both some 10 or 15 inches of mercury above that of the atmosphere, it would be necessary to determine the corrections on each of the thermometers under the pressures and temperatures at which they had to work.

Thus, if e_1, e_2 are the corrections per unit of pressure in the initial and final thermometers, the correction for the heat is $(e_1p_1 - e_2p_2)$.

The Range of Temperature over which the Specific Heat would be Measured.

51. The temperature of the effluent water from the brake can be regulated either up or down to any required extent, and although there would necessarily be some divergence from the boiling-point, with care and experience it would be possible to bring the mean result in a number of trials within a close approximation of 212° Fahr.

On the other hand, there has been no means provided of regulating the temperature of the water entering the brake. This is determined by the rate at which the water passes through the iced coil and the temperature at which it entered, as determined by the temperature in the town's mains, which varies from 38° in the winter to 55° in the summer. Thus the temperature in the light trials would be from half to a degree above 32°, and that of the heavy trials from a degree to two degrees.

In calculating the heat of each trial, the actual difference with the correction for the thermometers is taken, but if, as is shown by previous investigations by Regnault and others, the specific heat at and near 32° is less than the mean specific heat between 32° and 212° by something like 0·5 per cent., there would be errors in taking the results so obtained as the mean specific heat between 32° and 212°.

Owing to the extreme difficulty of determining the specific heat over a very short range of temperature to such high degrees of accuracy as ·01 per cent., the experimental evidence as to the exact value of the specific heat within a few degrees of 32° is but vaguely surmised from the general fall of the specific heat with the temperature.

The law of the thermal capacity of water between 0° C. and $t°$, as deduced by Regnault from his experiments, is avowedly vague as to the lower temperatures. It shows no singular point at the maximum density, as would be expected; and Rankin deduced another law from these experiments, making the minimum specific heat coincide with the point of maximum density. Also other experimenters have obtained higher specific heats near 32° than are given by Regnault's formula. It would seem probable, therefore, that the difference between the specific heat at 32° and the mean between 32° and 212°, as given by Regnault's formula, is too large.

In that case, the correction obtained by this formula in order to reduce the specific heat between the observed temperature in the trials to that between the standard points, would probably be too large, and thus afford an outside limit of error.

Thus, putting s for the mean specific heat between 32° and 212°, $s(1+X)$ for the specific heat between $T_1°$ and 212°, when $T_1°$ is small compared with 180°, and, by Regnault, taking $s(1-0{\cdot}005)$ for the specific heat at $T_1°$, then the total heat from $T_1°$ to 212° is

$$s(1+X)(212-T_1°) = s\{180-(T_1°-32)(1-0{\cdot}005)\}$$

$$= s(212-T_1°)\left(1-\frac{T_1°-32}{212-T_1°}\times 0{\cdot}005\right),$$

or, neglecting $(T_1-32)^2$,

$$X = 0{\cdot}005\,\frac{T_1°-32}{180} = 0{\cdot}000028\,(T_1°-32).$$

Thus, taking the mean capacity of water between the temperatures of 32° and 212° as the standard capacity, the mean specific heat between T_1 and 212° would be

$$1+X = 1+0{\cdot}000028\,(T_1°-32);$$

and, if $T_1°$ is the mean initial temperature of the water of any number of trials, $1+X$ is the mean specific heat of the water in all the trials. The mean specific heat of the difference of two trials would be $1+X$; this appears as follows:—

Suppose $1+X_1$ to be the mean specific heat for a set of heavy trials, and W_1 the mean weight of water, and $(1+X_2)$ to be mean specific heat of a corresponding set of light trials, and W_2 the mean weight of water, T_1°, T_2° being respectively the initial temperatures of W_1 and W_2, the difference of the total heats would be

$$(1+X_1)(212-T_1^\circ)\,W_1-(1+X_2)(212-T_2^\circ)\,W_2,$$

and the mean specific heat would be approximately

$$\frac{(212-T_1^\circ)\,W_1-(212-T_2^\circ)\,W_2+180\,(X_1W_1-X_2W_2)}{(212-T_1)\,W_1-(212-T_2)\,W_2}$$

$$=1+\frac{180\,(X_1W_1-X_2W_2)}{180\,(W_1-W_2)};$$

and, as in the heavy and light trials $W_1=2W_2$ approximately, the mean specific heat by Regnault's formula would be

$$1+2X_1-X_2=1+0{\cdot}000028\,[2\,(T_1-32)-(T_2-32)].$$

This result is obtained by merely summing the trials, but counting the water in the light trials as negative,

$$X=\cdot000028\Sigma\left\{\frac{W\,(T_1-32)}{\Sigma\,(W)}\right\}.$$

The Gradual Rising of the Indices of the Thermometer.

52. Where, as is generally the case, the indices of the thermometers are gradually rising, if they are used between the intervals at which they are corrected, the last observed correction being applied, there will be an error which will be negative, and of magnitude equal to the rate of rise during the interval multiplied by the interval. Thus, if the trials are uniformly distributed between the intervals of correction, the correction would be $0{\cdot}5a$, where a is the observed rise in the interval, hence the relative correction on the equivalent, taking $\bar{a}_1$ and $\bar{a}_2$, as the mean rises between the intervals of correction of the initial and final thermometers, would be

$$\frac{0{\cdot}5}{180}\cdot(\bar{a}_2-\bar{a}_1).$$

The Work done by Gravity on the Water.

53. The difference of pressure on the bulbs of the initial and final thermometers which are at the same level, expressed in feet of water, is

the work done by gravity per lb. of water. If p_1 and p_2 express these pressures in inches of mercury, the work done by gravity is

$$1\cdot14\,(p_1-p_2),$$

which gives as the relative correction for the equivalent, approximately,

$$+\,0\cdot000008\,\Sigma\,[W(p_1-p_2)]/\Sigma\,(W).$$

The Work absorbed in Wearing the Metal of the Bushes and Shaft.

[54. During the six years the brake had been in use, before the trial commenced, the shaft and bushes were occasionally lubricated with oil, chiefly to prevent oxidation of the shaft when standing, and, up to the commencement of the trials, there was hardly any appreciable sign of wear. After the closing of the bushes by the stuffing-box and cap, when the use of oil was purposely discontinued, there was no means of observing the wear of the metal as long as the brake worked satisfactorily, as it did during all the trials. But when, after the completion of the trials, the stuffing-box and cap were removed, in order to return to the original manner of working, the excess of leaking through the bushes showed that there had been considerable wear.

At that time it did not occur to me that the proportion of this wear, which took place during the actual running of the trials, would represent a certain amount of work absorbed in disintegrating the metal, or a certain amount of heat developed by the oxidation of the metal, and no attempt was then made to form a definite estimate of the amount of metal which had disappeared. As, however, the worn metal was replaced by a coating of white metal, the thickness of this (less than $\frac{1}{32}$nd of an inch) and the extent of surface (less than 124 square inches) subsequently showed that it could not be more than 1 lb.

This was after it occurred to me that however small might be the effect of this wear, since it was definitely observed to have taken place during the twelve months when the bushes were closed for the purpose of the trials, it was desirable, in order to complete the research, that some outside estimate should be obtained of the limits to its possible effect, whether from disintegration or from oxidation.

In as far as the loss of metal was due to the abrasion of the clean metal surfaces, it would be proportional to the number of revolutions, while in as far as it was owing to the oxidation of the metal surfaces, left bright after each run, it would be probably proportional to the number of runs.

The number of revolutions with the bushes closed, counting ordinary work as well as the trials, is found from the records to be less than

$$300 \times 60 \times 360,$$

and the number of runs to be 80, the mean time being 4·5 hours. The revolutions during any one of the accepted trials were 300×60. And the trials were made in threes, so that the coefficient for oxidation would be $\frac{1}{240}$.

Hence, the metal worn by abrasion in a single trial would be less than $\frac{1}{360}$th of 1 lb. = 0·0028 lb., and the metal oxidised in one trial less than $\frac{1}{240}$th = 0·004 lb. So far the estimate is fairly definite, but, for its completion, it is necessary to arrive at some conclusion as to the work absorbed in disintegrating the metal, and of the heat developed by its oxidation.

There does not seem to be any reason why there should be more oxidation of the bright surfaces in a light trial than in a heavy trial, so that there would have been no error from this cause in their difference.

As regards the abrasion and the oxidation of the abraded metal, there would be a difference, as the weight on the shaft in a heavy trial is 1·23 of the weight in a light trial. Thus the differences of abrasion would have been

$$0·0006 \text{ lb.}$$

The work necessary to produce a state of disintegration, such as exists in the vapour of the metal, would be the total heat of vaporization, less the kinetic energy and work $[\kappa_v/(T-32)+PV]$, and, although the heat of vaporization of the metal is not known, it would seem that it cannot greatly exceed, when subject to the deductions mentioned, the heat of vaporization of ice subjected to like deductions (1,000,000 ft.-lbs.).

Assuming this, since the difference in the work of two trials is about 70,000,000 ft.-lbs., the correction would be

$$-0·00001,$$

which, considering that the disintegration would be very imperfect, may be taken as an outside limit, while the effect may have been even reversed by the oxidation of the degraded metal.—Nov. 9, 1897.]

Accidents.

55. In contemplating such an extensive and complex research, the result of which depends on the mean of a number of experiments, it was impossible to overlook the question as to how such accidents, as would probably occur, should be dealt with.

It was clear that, whatever the rule might be, it must be definite and rigorously applied.

Two other things were also clear, that, as in surveying, accidents might occur, say in reading the counter or the scales, which would only be apparent from the reduction of the results after the trial was finished. Also, that in these experiments there would be no such rigorous check on the results as in surveying; so that, without danger of sorting the results, anomalous results, the cause of which was not noted during the trial, could only be rejected when the results themselves contained evidence of the cause of the anomaly, say an abnormal difference between the mean speeds by the counter and the speed gauge.

It was therefore, from the first, decided to reject all trials in which there was definite evidence either during the trial or in the results, of uncertainty to which no definite limits could be assigned, in any one of the measurements, without regard for the apparent consistency of the results, and in the same way to retain all other trials.

56. The following table contains a summary of all those circumstances on which the accuracy of the result of the investigation depends, together with references to the several Articles in which they have been discussed. In line with each circumstance is placed the formula for the relative correction in the equivalent, necessary in consequence of the observed deviation from the conditions of equality between the heavy and light trials. In the same line with each circumstance are also given, to the millionth part, the limits of relative error as deduced in the corresponding Articles.

Reference No.	References to Articles	Circumstances affecting the accuracy of the results	Formulæ for the relative corrections	Limits of relative errors	
1	6	Terminal differences in the moments of momentum of the water in the brake	0·00000	+0·000000	−0·000000
2	8	Cyclic-fluctuations in the speed of the engines	0·00000	0·000025	0·000025
3	9	Work done on the water by end-play in the shaft	0·00000	0·000017	0·000000
4	9	„ „ dash-pot	0·00000	0·000000	0·000000
5	9	Effect of the automatic-gear on the balance of the brake	0·00000	0·000016	0·000016
6	10	Imperfect elimination of the error of balance	0·00000	0·000000	0·000000
7	29	„ „ „ heat conducted by the shaft	$-C\Sigma(T_B^\circ - T_{SB}^\circ)/\Sigma(H)$	0·000000	0·000000
8	32	„ „ „ „ radiated from the brake	$-R\Sigma(T_2^\circ - T_a^\circ)/\Sigma(H)$	0·000010 (?)	0·000010 (?)
9	34	The engagement of the counter	0·000013	0·000000	0·000000
10	31	Terminal differences in speed and temperature	$-\Sigma\{(B+w_i)T_i^\circ-(B+w_f)T_f^\circ\}/\Sigma(H)$	0·000000	0·000000
11	35	Leakage of the stuffing-box	$-\Sigma\omega_{SB}(T_{SB}^\circ - T_1^\circ)/\Sigma H$	0·000000	0·000025
12	36	„ at the automatic valve	0·00000	0·000000	0·000000
13	37	Imperfect elimination of the water lost by evaporation	0·00000	0·000000	0·000000
14	40	Limits of accuracy in weighing the water	0·00000	0·000025	0·000025
15	41	Weight of the atmosphere	−0·001205	0·0000000	0·000000
16	42	Correction for gravity at the latitude of Greenwich	0·000172	0·0000000	0·000000
17	42	„ „ „ „ 45°	0·000745	0·0000000	0·000000
18	43	Salts dissolved in the water	$+0·8s$	0·0000000	0·000000
19	43	Air „ „ „	$-10·3/a$	0·000100 (?)	0·000100 (?)
20	45	Length of the lever	?	0·000020	0·000020
21	50, 46	Effect of pressure on the thermometers, limits of error	$-\Sigma\{W(e_1 p_1 - e_2 p_2)\}/\Sigma(W)\times 180$	0·000010	0·000010
22	52	Rise of the standard readings of the thermometers in the intervals of correction	+0·5 × (the difference of rise / the number of intervals) / 180	0·000000	0·000000
23	51	Differences between the initial temperature of water and freezing	$-0·000028\,.\,\Sigma\{W(T_1^\circ - 32^\circ)\}/\Sigma(W)$	0·000000	0·000000
24	53	Work done by gravitation on the water	$-0·000008\,\Sigma\{W(p_1 - p_2)\}/\Sigma(W)$	0·000000	0·000000
25	54	Work absorbed in wear of the metal	0·00000	0·000010	0·000010
			Summary of limits of error	+0·000233	−0·000241

The quantities under the signs $\Sigma(\quad)$ are to be taken positive for the heavy trials and negative for the light trials.

Significance of the Symbols in the Formulæ.—C, the constant for conduction obtained from the trials; R, constant for radiation, as defined in trials, with the lagged and naked brake; s and a, are weights of *salts* and *air* in unit weight of water; p_1 p_2, pressures in inches of mercury in the initial and final thermometer chambers; e_1 e_2, are the corrections, per inch of mercury, pressure on the initial and final thermometers; W, weight of water used in a trial; ω_{SB}, weight lost at the stuffing-box; T°, temperature Fahr.; T_{SB}°, stuffing-box; T_B°, bearing; T_a°, air; T_i°, temperature at beginning; T_f°, at the end; w_i, water in brake at beginning; H, the heat generated during the trial; B, capacity for heat of the metal in the brake.

PART II.

On an Experimental Determination of the Mechanical Equivalent of the Mean Specific Heat of Water between 32° and 212° Fahr., made in the Whitworth Engineering Laboratory, Owens College, on Professor Osborne Reynolds' method.—By William Henry Moorby, M.Sc.

1. In view of the frequent and extremely careful and accurate determinations of the value of the mechanical equivalent of heat which have been made of late years by different experimenters using different methods the present series of experiments may on first thoughts seem superfluous. There did, however, seem to be sufficient disagreement between the results previously published—more particularly between values of the equivalent, as derived from the direct methods described by Joule, Rowland, and Miculescu, and the indirect electrical methods of Griffiths, and Gannon, and Schuster, to warrant a new investigation into the value of this important constant, if the proposed new method of working should carry with it advantages not available in previous investigations. I was accordingly very glad to fall in with the wishes of Professor Reynolds that I should undertake a research bearing on this point on lines which he suggested to me in July, 1894.

2. In Part I., par. 3, a full description is given of the apparatus whose existence in the Whitworth Engineering Laboratory led up directly to the institution of this research into the value of the mechanical equivalent of heat.

The advantages which the proposed method offered were briefly :—

(1) The possibility of obtaining a result which in no way depended for its accuracy on the value of the scale divisions of the thermometers used in the measurements of temperature (Part I., par. 11).

This was done by supplying a stream of water to the brake at a temperature of 32° Fahr., and there raising its temperature to 212° Fahr. before admitting it to the discharge pipe where its temperature was again taken.

(2) A means of eliminating from the result all losses of heat due to radiation and conduction from the calorimeter employed (Part I., par. 32). The manner in which this elimination was accomplished is indicated below.

Let U and u represent the quantities of work done in two trials which differed only in the moment of resistance offered by the brake—the number of revolutions of the engine shaft and the duration of the trials being the same in each case.

Also let H' and h' be the apparent quantities of heat generated in the brake in these trials. These quantities will be less than the true equivalents of the works U and u by quantities which represent the losses of heat from the brake by conduction, radiation, &c. These losses were made as nearly as possible equal by keeping the temperatures of the brake and its supports and surroundings at the same levels in the two trials.

Then the quantity of work $(U - u)$ should be exactly equivalent to the quantity of heat $(H' - h')$, and by dividing the first of these by the second, a value of the constant required is obtained.

The power available for the purposes of the investigation enabled me to deal with quantities approaching the following values in trials of one hour's duration:—

Revolutions, 18,000.

Total work done, 135,000,000 ft.-lbs.

Total weight of water raised 180° Fahr. = 960 lbs.

Total apparent heat generated = 170,000 B.T.U.

In quantities so large as these some of the small errors inevitable to all physical experiments became quite or nearly negligible.

Preliminary Apparatus and Trials.

3. It will, perhaps, be sufficient to indicate the general arrangement of the apparatus as first set up. This is illustrated in the annexed sketch. The water was supplied from the mains through the iron stand-pipe, A, and the regulating cock, B. Before it entered the brake its temperature was measured by means of the thermometer, C, inserted through a cork in the stand-pipe, the part of the stem on which readings were taken being exposed to the atmosphere. After being discharged from the brake, D, the water entered a flexible rubber pipe, E, bent through an angle of 90°, which connected a horizontal nipple at the bottom of the brake with a vertical one forming the lower end of a fixed line of copper piping, F. The temperature of discharge of the water was indicated by the thermometer, G, which was enclosed in a glass tube opening through a stuffing-box into the discharge pipe, the whole length of the stem being therefore kept at the temperature of discharge. On leaving the copper discharge pipe the water was directed

at will by the two-way tipping switch, K, either to the left to waste or to the right into the tank, L, standing on the platform of the weighing machine, M.

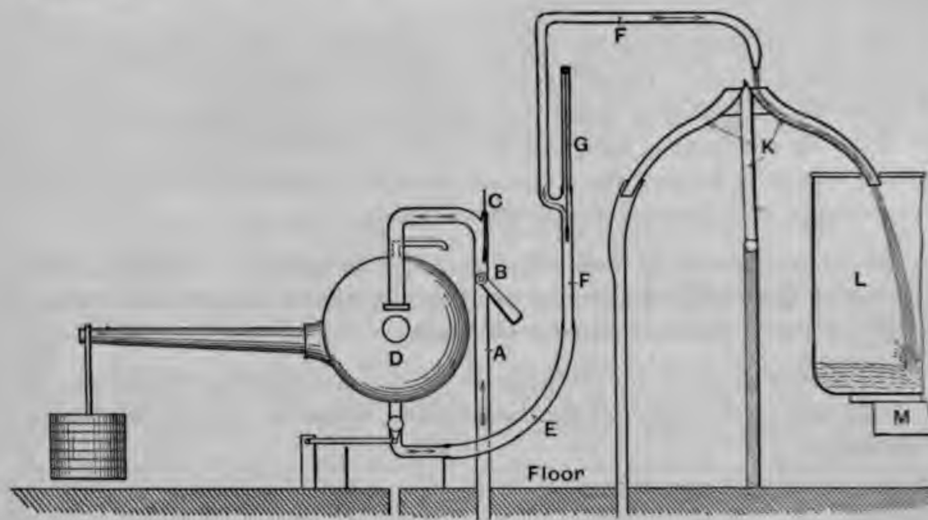


Fig. 1. Preliminary Apparatus. Course of water shown by arrows.

A series of trials were made with this apparatus, the water being raised through varying intervals of temperature between 35° Fahr. and 100° Fahr. For obvious reasons the results were not satisfactory, and are therefore not published. Experience was gained, however, which helped very materially in the design of the final apparatus.

Common thermometers were used, and calibration errors on the comparatively small range of temperature through which the water was raised were of sufficient importance to vitiate all results. Again, the exposure of the stem of the thermometer, C, was a weak spot in the apparatus. I was much troubled also with leakage of water from the two bushed bearings of the brake.

In so far as could be judged, the bent rubber pipe, E, was found to be a satisfactory connection between the brake and the copper discharge pipe, and this has been retained in the subsequent apparatus.

Details of the Constituent Parts of the Final Apparatus.

Artificial Atmosphere.—(Part I., par. 23.)

4. To prevent loss of water by evaporation at the centres of the vortices formed in the brake, the ports in the vanes of the outer casing were connected

through a flexible rubber tube some 4 feet long, with an artificial atmosphere formed in a tin receiver, the pressure in which was maintained by means of a cycle tyre inflator at about 9 inches of mercury, as measured on a U-gauge. The shape of this vessel is made clear in the sketch (Part I., Fig. 8). The ends were made conical for greater strength. The receiver was also provided with an air valve, with which to relieve the pressure when too high, and a cock, with which water accidentally lodging inside could be drained away.

The Ice Cooler.—(Part I., par. 19.)

5. Some preliminary experiments indicated that a length of about 200 feet of $\frac{3}{8}$-inch diameter lead piping would, when immersed in a mixture of ice and water, be sufficient to cool a stream of some 16 lbs. of water per minute very nearly to 32° Fahr.

The ice cooler was accordingly made as follows: A wooden box, $4'\,0'' \times 2'\,3'' \times 2'\,0''$, and lined inside with waxed cloth, was fitted with a horizontal wooden shelf about 2 feet 6 inches long, and on this was laid a flat oval coil of $\frac{3}{8}$-inch composition piping nearly 200 feet in length, the left-hand end of the coil and shelf stopping short at a distance of 1 foot from the end of the box, the right-hand end of the coil reaching the end of the box, but the shelf stopping some 6 inches short of that point. The coil was about 5 inches diameter, vertically, and over it were placed the wooden guide plates shown (Part I., Fig. 7). An 8-inch diameter paddle, having 6 wooden floats, was placed about the middle of the box, at a height just sufficient to ensure the lower edges of the floats clearing the coil of pipe below it. A galvanized iron wire netting, extending from the shelf upwards to the top, separated the well at the left-hand end of the box from the compartment to the right containing the coil and paddle.

When working, the well and space beneath the shelf contained broken ice, well rammed in; while the level of the water was automatically kept at about 3 inches above the top of the coil. The paddle, driven by a cord from the line shafting in the engine-room, revolved in the direction shown by the arrow, and caused a circulation of water up through the ice in the well, and then horizontally through the coil and back to the ice under the shelf.

Circulating Pump.—(Part I., par. 20.)

6. In order to supply sufficient water to the brake against the resistance offered by the 200 feet of pipe in the cooler and the augmented pressure in the brake itself, it was necessary to use a circulating pump. This was a small Mather-Reynolds centrifugal pump with four $1\frac{1}{2}$-inch wheels, driven

by a turbine available for this purpose in the engine-room. This pump was capable of supplying 16 lbs. of water per minute, against a pressure of 25 lbs. per square inch at the supply valve.

Some difficulty was encountered in the summer of 1896 with this combination, because the excessive demand for condensing water for the engine hardly left sufficient flow in the falling hydraulic main to work the turbine at the requisite speed to maintain the above pressure.

On the whole, however, the combination was exceedingly efficient, and with a graduated supply valve afforded a very delicate means of regulating the flow of water into the brake.

Water-tight Joints between the Brake and the Engine Shaft.

7. In Part I., par. 24–29, the necessity of obtaining control over the leakage of water at the bearings of the brake, and the methods by which this was accomplished, are fully discussed. The bearing on the up-shaft end of the brake was provided with a stuffing-box, while the shaft end was covered with a cap. The annexed sketches show the general design of the stuffing-box and cap:—

A—The engine crank shaft.

B—The outer skin of the brake.

C—Conical brass bushes screwed into the outer skin of the brake.

D—Lock nuts on these bushes.

E, *F*, and *G*—Stuffing-box, ring and cover.

K—Set screws fastening stuffing-box to the lock nut.

L—Cap covering the end of the shaft.

M—Small spindle driven by a pin on the end of the engine shaft, passing through a stuffing-box on the cap, and required to drive the revolution counter.

The cap completely stopped all leakage from the bearing to which it was fixed, and, when the stuffing-box had worked for a short time, only a few drops of water escaped from the up-shaft bearing.

The brass bush bearings needed lubricating, and this was accomplished by supplying a small stream of water to each bearing through the pipes *N* and *P*, each provided with a regulating cock. This water came from the supply pipe between the ice cooler and the regulating valve controlling the main supply to the brake. It was consequently under considerable pressure and at a temperature very little over 32° Fahr. The water thus supplied had, of course, to enter the brake, and the amount supplied afforded a very convenient means of controlling the temperatures of the bearings.

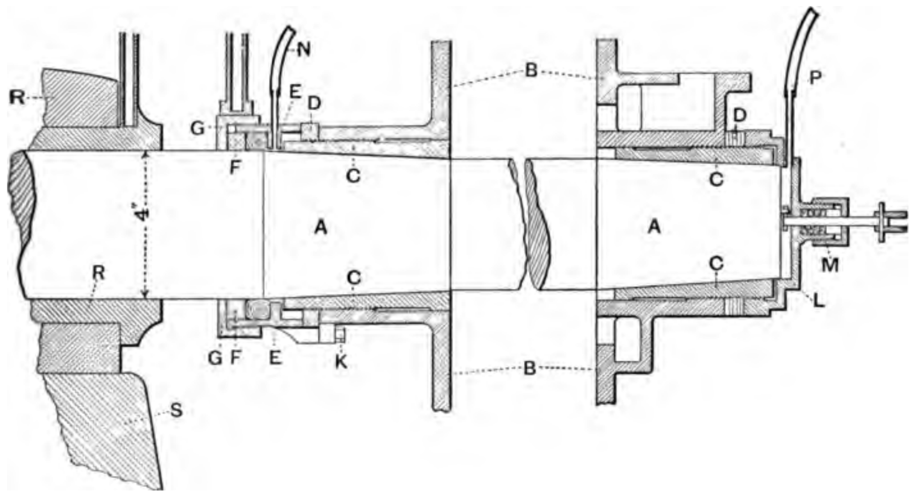


Fig. 2. Joints between brake and shaft.

At a distance of $2\frac{3}{4}$ inches from the cap of the stuffing-box was the end of one of the main bearings, *R*, carried on the cast-iron pedestal, *S*.

It was important that I should have some control over the loss of heat by conduction along this length of shaft. Accordingly, two pieces of brass pipe were soldered on to the cap of the stuffing-box, while two others were screwed, the one in the upper and the other into the lower brass forming the main bearing. Thermometers were placed inside the tube affixed to the stuffing-box cap, which happened to be uppermost at the time, and into the two pipes screwed into the main bearing. It was then assumed that the loss of heat along the shaft would vary with the difference of temperature between the stuffing-box cap and the bearing. In order that the losses of heat occurring in this way in any two trials should be identical, it was sufficient under the above assumption that this difference of temperature should be the same in both trials, and the temperature of the stuffing-box was regulated to this end by means of the amount of cold water passing into it.

Considerable difference of temperature was observed between the upper and lower brasses of the bearing, and as it seemed probable that the lower one approximated the more closely to the temperature of the shaft, that thermometer was the one used in determining the loss of heat by conduction.

In the later trials I endeavoured to keep the temperatures of the stuffing-box and the bearing at the same level, thus entirely eliminating this cause of loss from the experiments.

Water Jackets for the Low and High Temperature Thermometers.—
(Part I., par. 15.)

8. It was evident that the temperatures of the water would be much more easily and accurately taken if the whole stem of each thermometer was kept at one temperature. To this end each of the principal thermometers was completely jacketed with a stream of the water whose temperature was required.

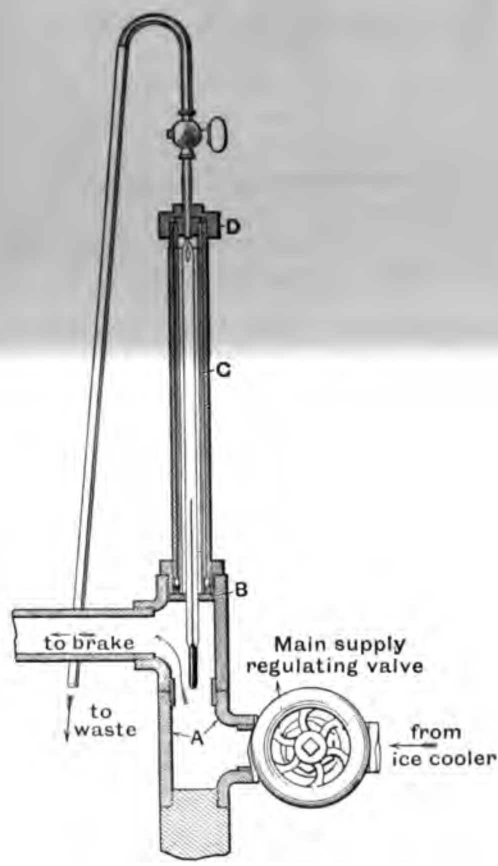


Fig. 3. Cold water thermometer jacket.

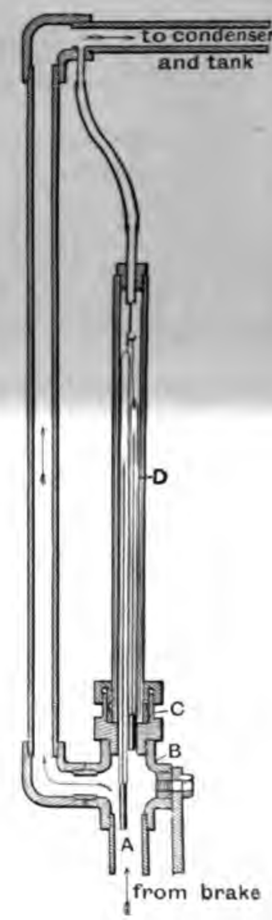


Fig. 4. Hot water thermometer jacket.

The arrangements adopted for this purpose are illustrated in the annexed sketches. (Figs. 3 and 4.)

After leaving the main regulating valve the cold supply water entered a vertical brass **T**, shown at A. The main volume of the water flowed on to the brake through the horizontal arm of this **T**. At its upper end the **T** carried a small stuffing-box, B, into which was fixed a vertical ½-inch diameter glass tube, C. This tube was closed at its upper end by means of a rubber stopper, held in place by the brass cap, D, screwed on to the upper end of a ¾-inch slotted copper pipe surrounding the glass tube. The stopper and cap were both penetrated by a short length of ⅛-inch diameter brass tube, which carried a gas-cock at its upper end. The thermometer was hung by a piece of string from the lower end of the ⅛-inch pipe—the graduated part of the stem being all clearly visible through the glass walls of the chamber while the bulb was well in the main stream of water flowing through the brass **T**.

A small stream of water was allowed to run to waste through the small gas-cock at the top, thus ensuring the whole of the stem of the thermometer being kept at the proper temperature.

The hot water discharged by the brake flowed from the bent rubber tube, previously mentioned, into the lower end of the vertical 1-inch diameter copper pipe, A. This pipe carried a brass cross, B, at its upper end, while fitted to the top of the cross was the stuffing-box, C, in which was fixed a piece of ¾-inch diameter glass tubing, D, forming the thermometer chamber. The upper end of this chamber was closed by a rubber stopper penetrated, as before, by a piece of ⅛-inch diameter brass pipe, connected by a piece of rubber tubing to the main discharge pipe above.

The left arm of the cross carried an upward-turning elbow, and that again a ¾-inch diameter copper pipe, up which most of the water flowed.

The thermometers, two of which were used, were hung to the lower end of the ⅛-inch pipe in the rubber stopper, so that the bulbs were immersed in the whole stream of water flowing up the 1-inch copper pipe from the brake. One of these thermometers was only used as a finder to indicate the temperature of the water as it rose after first starting the engine, and no record of its readings was kept.

The Condenser.—(Part I., par. 18.)

9. In order that there should not be a large loss of water before weighing, by evaporation from the tank into which it flowed from the brake, it was necessary to cool the stream to a temperature approaching that of the atmosphere.

For this purpose a condenser was constructed after the ordinary chemical pattern. It consisted of a length of 21 feet of $\frac{3}{4}$-inch diameter pipe inserted in an equal length of $1\frac{1}{4}$-inch diameter iron pipe.

Stuffing-boxes were used to form the joints between the two pipes. The hot water from the brake flowed through the inner tube, while a supply of condensing water flowed in the opposite direction through the annular space between the two pipes. By means of this condenser the water entering the tank was always cooled at least to 100° Fahr., and to lower temperatures in the earlier experiments when the water available in the mains was considerably colder.

The Rising Pipe.—(Part I., par. 21.)

10. The thermometer indicating the discharge temperature often gave readings more or less above 212° Fahr.

To provide against any fall in temperature at the thermometer bulb, which might occur by reason of the formation of bubbles of steam in the water, it was found desirable to keep some pressure on the water at that part of its course.

Accordingly, instead of discharging the water directly from the condenser into the tank, it was conducted up a vertical pipe, which was open at the top through a T to the atmosphere. The water then drained down another pipe provided with a nozzle at its lower end, opening into the two-way switch, to be described later. By this means a head of 11·3 feet of water was maintained at the thermometer bulb, and at a temperature of 220° Fahr. I had not much trouble with bubbles of vapour.

The Two-way Tipping Switch.—(Part I., par. 16.)

11. This was constructed to provide a means of rapidly diverting the water at will, either to waste or into the tank. It consisted, as shown in the sketch, of two curved copper pipes of rectangular section, meeting at their upper ends at an angle of about 30°. Their common side was produced for about $\frac{3}{4}$ inch, and formed into a knife-edge, separating the two orifices.

These pipes were rigidly connected to a wooden link which worked about a horizontal axis, distant 25 inches below the knife-edge. Wooden stops were provided to limit the swing of the switch to rather less than 2 inches. One arm of the switch worked in a funnel forming the top of a pipe leading to waste, while the other worked through a hole in the cover of the tank. The whole arrangement was fixed so that when in the central position the knife-edge was $\frac{1}{4}$ inch vertically below the nozzle at the end of the discharge pipe.

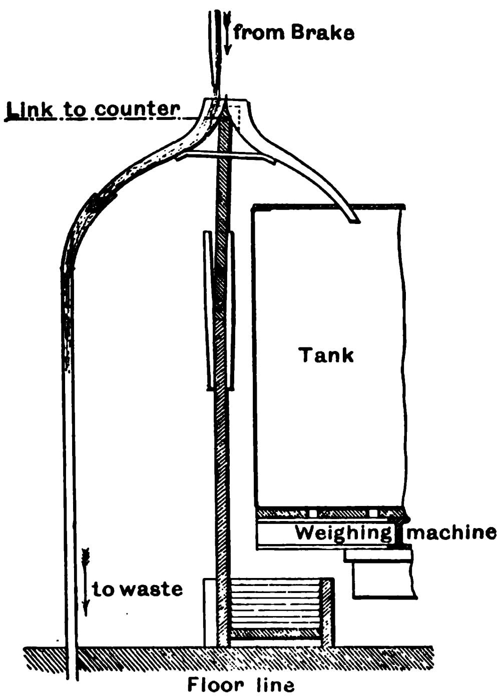


Fig. 5. Tipping Switch.

This switch worked exceedingly well, diverting the stream of water almost instantaneously, without making any perceptible splash.

In the later trials this switch was connected by a chain of links with the revolution counter, so that when the latter was pushed into gear with the engine shaft the switch simultaneously directed the water into the tank, and *vice versa.*

Weighing Machine and Tank.—(Part I., par. 13.)

12. To facilitate the weighing, the stream of water was led during each experiment into a galvanized iron tank which stood on the platform of a weighing machine. The tank was 4 feet long by 2 feet 9 inches deep, by 2 feet 9 inches wide. During the experiments it was kept covered by a lid of thin boards, steeped in paraffin wax. These boards were always weighed with the tank, so that any water they might absorb was accounted for. A $2\frac{1}{2}$-inch valve in the tank bottom was used for discharging the water after weighing.

The weighing machine was graduated up to 2200 lbs., and was supplied with three rider weights.

No. 1, the largest, was provided with a knife-edge which fitted into grooves cut in the lever of the machine, each division representing 100 lbs.

No. 2 worked on another scale on the lever, each division representing 1 lb., and graduated up to 100 lbs.

No. 3 was made by Mr Foster, in the laboratory, and indicated 0·01 lb. per division of the second scale. The lever was $32\frac{1}{2}$ inches long, and readings were taken only when the middle of the swing of a pointer fixed to the end of the lever coincided with a line marked on a brass plate alongside it.

It was quite easy in each individual weighing to set the machine to 0·01 lb., but owing, no doubt, to shifting of the platform, levers, &c., I do not think the readings taken were reliable beyond the $\frac{1}{50}$th of a lb.

This machine was not at first quite as sensitive as was necessary to attain the high degree of accuracy required for the purposes of the research. On examination this was found to be due to the slightly imperfect adjustment of the knife-edges attached to the graduated lever. The fault was rectified by Mr Foster, and since then the performance of the machine has been highly satisfactory.

The Rubber Pipe Connections to the Brake.

13. On account of the very considerable pressure to which all the fittings of the brake were subjected, it was found necessary to bind with tape the rubber pipes supplying the water to ensure them against bursting.

The extra stiffness thus given to these pipes did not much affect the free working of the brake, since none of them had a leverage of more than 4 inches from the centre of the shaft.

The case was, however, different with the bent rubber connection between the brake and the discharge pipe, since in this case the leverage is about 1 foot 6 inches. This pipe was eventually inserted in a cage consisting of a spiral of copper wire, $1\frac{1}{4}$ inches in diameter, through the coils of which were threaded two longitudinal wires to prevent elongation of the cage and rubber tube. By this arrangement the flexibility of the rubber tube was almost unimpaired.

The Device for Catching the Leakage at the Bottom Regulating Cock.—(Part I., par. 36.)

14. It was found impossible to prevent leakage taking place, generally to a small extent, from the automatic cock controlling the amount of water in the brake. It was, therefore, necessary to provide some means of catching this water, and it was very important that no impediment should be placed in the way of the free working of the cock spindle.

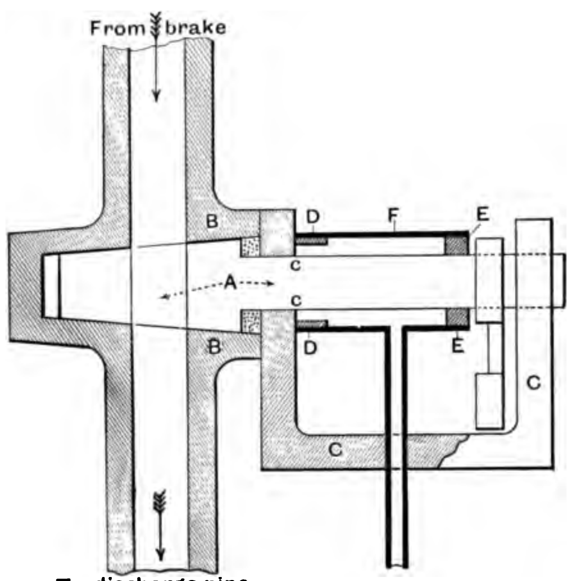


Fig. 6.

A tight joint was made between the valve seating, *B*, and the bracket, *C*, which carried the overhanging end of the valve, *A*. All the leakage, therefore, occurred along the valve spindle at *cc*. The method adopted to catch it was to solder a brass ring on to the bracket at *D*, and fit a ring of cork of the same diameter tightly on to the spindle at *E*. A piece of thin rubber tubing, *F*, was bound tightly to the ring, *D*, and the cork, *E*.

This tube caught all the leakage, which then drained down the smaller tube (shown in the sketch) into a bottle standing on the floor.

To prevent evaporation, the end of this small tube contained a short length of glass tube, the capillarity of which always kept the end closed by a bead of water.

General Arrangement of the Final Apparatus.

15. The general arrangement of the apparatus, as finally set up, is shown in the drawings at the end of the paper and in the annexed diagram. The course of the water was as follows:—

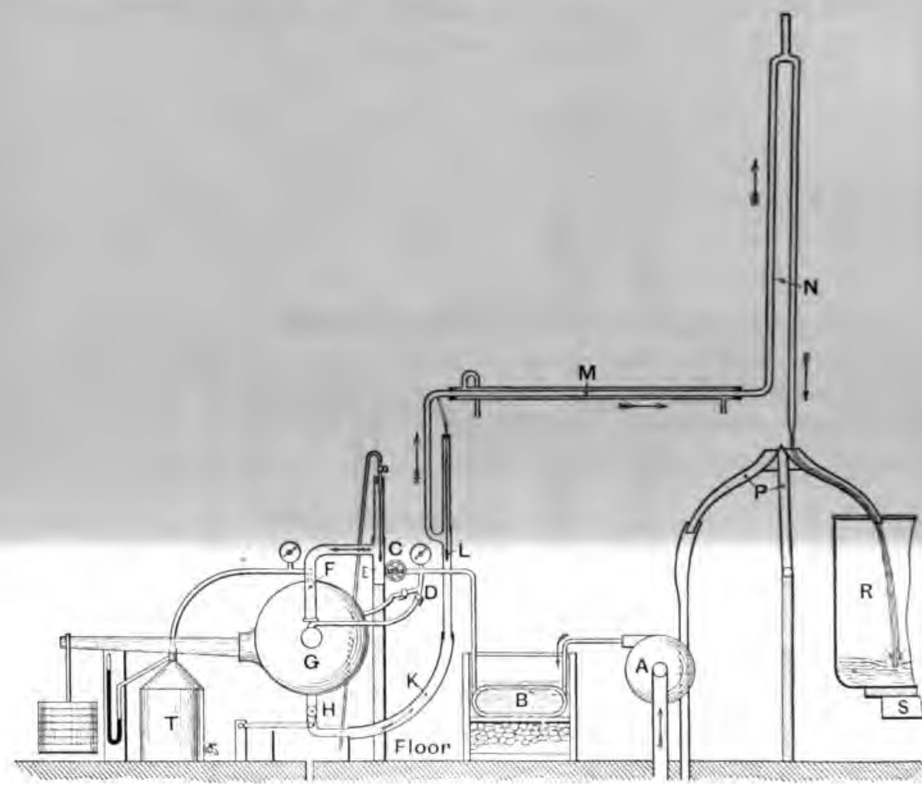


Fig. 7. Final apparatus.

It was drawn from the mains by the circulating pump, *A*, and forced through the ice cooler, *B*, to the main regulating valve, *C*. Between the ice cooler and this valve there was a Bourdon pressure gauge and a branch-pipe, *D*, supplying water to the bearings of the brake. Entering the vertical stand-pipe, *E*, the water flowed round the bulb of the initial temperature thermometer, a small stream being diverted to waste through the jacket. The straight flexible rubber pipe, *F*, then led the stream to the brake, *G*, from which the water flowed through the automatic valve, *H*, and the bent rubber pipe, *K*, to the vertical stand-pipe, *L*, carrying the thermometer for measuring the temperature of discharge. Then passing through the condenser, *M*, and the rising pipe, *N*, the two-way switch, *P*, directed the water either to waste or into the tank, *R*, standing on the platform of the weighing machine, *S*. At *T* is shown the tin vessel forming the artificial atmosphere. A small Bourdon gauge was fitted on to the top of the brake because the mercury gauge, indicating the pressure in the air-vessel, was not visible to the observer when taking readings of the thermometers, and it was important that this pressure should be kept constant.

The Hand Brake and Speed Indicator.—(Part I., par. 30.)

16. In addition to the separate parts of the apparatus already mentioned there was a hand brake by which a moment of about 50 ft.-lbs. could be gradually applied to the engine shaft, and by this means a delicate adjustment of the speed of revolution was obtained.

To make this speed evident a small speed gauge was driven by a gut band from the engine shaft. It consisted of a paddle rotating about a vertical spindle in a cylindrical case. The case contained coloured water, and the pressure generated forced a column of the water up a glass tube, to a height which varied with the speed of revolution.

In Part I. Professor Reynolds has referred in one or two instances to the excellent manner in which various parts of the apparatus were constructed by Mr Foster, to whom my thanks are also due for the valuable assistance he often rendered at critical moments in the research, and further for the advice and help he was always willing to give in the construction of apparatus for which I was mainly responsible.

The Method of conducting the Experiments finally adopted—using the Completed Apparatus.

17. During the progress of the experiments I had at my disposal the services of two men and a boy. Of the men, the first, Mr J. Hall, was fully engaged in attending generally to the needs of the engine and boiler, and had besides to maintain the boiler pressure at a point which ensured the steady running of the engine. I am bound to state that very much of the success met with must be attributed to the very admirable manner in which Mr Hall's part of the work was performed.

The duties of the second assistant Mr J. W. Matthews consisted in regulating the engine speed by means of the hand brake, more particularly at the commencement and end of each trial, and also in keeping a constant pressure of 9 inches of mercury in the artificial atmosphere.

The boy's time was occupied in breaking up the ice and feeding it as required into the ice cooler.

In the last series of experiments three similar trials of 62 minutes duration each were made per day, and the engine having been once started was not stopped till the three trials were completed. Consequently what I say below as to the starting of the engine does not refer to every trial, for after emptying the tank at the close of any one all the necessary adjustments were ready made for the next.

I. The pump and engine were started simultaneously, the brake being therefore supplied with a stream of cold water through the ice cooler. The brake then automatically adjusted the weight of contained water till the load floated clear of the engine floor. The speed was then adjusted till the speed indicator gave the required reading, viz., in all recorded trials 300 revolutions per minute.

II. Since all the work done was expended on the stream of water passing through the brake, its final temperature rose more or less quickly, and by adjusting the regulating valve on the supply pipe the temperature of discharge finally remained steady at 212° Fahr. nearly. In the meantime the supply of water to the stuffing-box was regulated till the temperature of the cover was at the required level.

These adjustments took from a quarter to half an hour, and when made, the engine was allowed to run for some half-hour longer to ensure a steady condition being attained.

The water supply to the condenser had also been regulated till the stream of water issuing from the rising pipe and flowing to waste had the requisite temperature.

III. Readings were then taken of—

(*a*) The revolution counter.

(*b*) The weight of the empty tank and its cover.

IV. When a steady condition was reached, the revolution counter at a given signal was pushed into gear with the small spindle previously mentioned, making connection through the cap with the engine shaft, and simultaneously the two-way tipping switch, which had hitherto been directing all the water to waste, was pulled over and diverted the whole stream into the tank. In the later trials all leakage that did sometimes take place from the stuffing-box, and a slight leakage that always occurred at the automatic cock below the brake, were collected in two bottles kept for that purpose. These were put under the drain pipes in each case as soon as possible after the signal.

The speed of the engine as indicated by the gauge was read when the signal was given, and as soon as possible afterwards a reading was taken of the temperature in the discharge pipe.

V. At intervals of two minutes thirty observations were then taken of the temperatures of supply and discharge of the water to and from the brake, and also at each of these intervals a note was made of the reading of the speed gauge.

At intervals of four minutes fifteen observations were made of a thermometer registering the temperature of the room. Also at intervals of eight minutes readings were taken of the two thermometers in the stuffing-box and on the main bearing.

VI. When sixty-two minutes had elapsed the counter was freed from the shaft, at the same time the water being again diverted to waste.

The drain pipes from the stuffing-box and cock were removed from their respective bottles.

Readings were taken of the speed indicator and of the temperature of discharge.

VII. Fresh observations were made of—

(*a*) The reading of the revolution counter.

(*b*) The weight of the tank and water received during the trial, to which had been added the water caught from the regulating cock.

A record was also made of—

(*c*) The weight of water which had been caught from the stuffing-box.

18. These observations were afterwards reduced as follows :—

Let T_1 = mean temperature of water supplied to the brake.

T_2 = „ „ discharged by the brake.

W_1 = weight of tank and contents before the trial.

W_2 = „ „ after the trial.

w = weight of water caught from the stuffing-box.

t = rise of reading of the thermometer in the discharge pipe during the trial.

T_S = mean temperature of the stuffing-box cover.

T_B = „ „ lower brass of the main bearing.

T_A = „ „ air.

N_1 = reading of revolution counter before the trial.

N_2 = „ „ after the trial.

M = moment in ft.-lbs. carried by the brake.

Therefore we have for the total heat generated

$$H = (W_2 - W_1)(T_2 - T_1) + w(T_S - T_1) + t \,.\, X \,.+ (T_S - T_B)\, C + (T_2 - T_A) R.$$

The determination of the quantity X and of the constants C and R, representing the losses by conduction and radiation, will be dealt with later (pars. 30, 43 and 45).

Also the total work done

$$U = 2\pi (N_2 - N_1)(M + m),$$

where m = error in balance of the brake. This error will be dealt with subsequently (par. 29).

If the capitals H and U refer to trials with a large turning moment on the brake, and the small letters h and u refer to trials with a small turning moment, then for our value of the mean specific heat of water in mechanical units we have

$$K = \frac{U - u}{H - h}.$$

This quantity K is not strictly the same as the mechanical equivalent of heat, of which other determinations have been made, since we are here dealing with the mean specific heat of water between freezing and boiling-points.

For this reason it has been decided not to use the usual symbol J, at any rate at this stage of the research.

19. As an illustration of the method of tabulating and reducing the observations, I append all that were taken in trials 69 and 72 made on the 7th and 8th July, 1896, respectively.

It will be seen that all the observations of temperature, together with the readings of the speed indicator, which were made during the actual progress of each trial, are given on pages 679 and 681 respectively.

With the exception of the two readings of the speed indicator taken at the moments of starting and finishing each trial, and shown in brackets at the top and bottom of column No. 8, I was personally responsible for all observations recorded. These two observations were made by the assistant in charge of the hand brake and artificial atmosphere.

In the tables of temperature and speed observations

Col. 1 gives the times at which observations became due, the whole period of 62 minutes being divided into 31 two-minute intervals.

Col. 2 gives the temperatures of supply of the water to the brake.

Col. 3 „ „ discharge of the water from the brake.

Col. 4 „ „ the air in the engine room.

Col. 5 „ „ the stuffing-box cover.

Col. 6 „ „ the lower brass of the main bearing.

Col. 7 „ fall of temperature between the stuffing-box and bearing, being the difference of Cols. 5 and 6.

Col. 8 gives the readings of the speed indicator.

Observations of the revolution counter and of the weight of the tank before and after each trial, are given on pages 678 and 680 respectively.

As I had to take all the observations myself, it was, of course, impossible to make them simultaneously at the times indicated in Col. 1. They were, however, always taken in the same order, as follows.

When the time for the next ensuing series of observations had arrived as given by a watch lying on the table at my side, I immediately read the temperatures of supply and discharge and the speed gauge in the order named, and after reading the three I entered them in the note-book. This generally took about a quarter of a minute. If then a reading of the atmospheric temperature was due, it was next taken and entered. After that the temperatures of the stuffing-box cap and of the bearing were noted in their turn, the whole series of observations being made in 1 or 1¼ minutes.

The interval which then elapsed before the next series of observations became due was often fully occupied in making adjustments of the regulating valve controlling the main water supply to the brake; of the cock regulating the supply to the stuffing-box; and of the speed of the turbine driving the pump, small alterations at all these points being frequently necessary.

At the head and foot of Cols. 3 and 8 will be seen observations in brackets. These observations were taken at the moments of starting and ending the trials, and were required in the calculation of a terminal correction to be referred to later.

At the close of each trial a mean of the observations occurring in Cols. 2, 3, 4, 5 and 7 was made, the two observations in brackets in Col. 3 being omitted in calculating these means.

On pages 678 and 680 additive corrections to the weights and to the mean temperatures of supply and discharge are given. These will be referred to later.

It will be noticed that in neither of the trials chosen was there any leakage of water from the stuffing-box.

The observations are given again in the partially reduced form which has been adopted for the final tabulation of the results on p. 682.

Cols. 1 to 8 should be self-explanatory.

Col. 9 gives the first approximation to the heat generated, obtained by multiplying the weight of water by its mean rise in temperature.

Col. 11 gives the difference of the temperature of the stuffing-box (supposed to be a measure of that of the water leaking from it), and the temperature of supply.

Col. 12 gives the loss of heat due to this leakage, and represents the product of Cols. 10 and 11.

Col. 13 gives the rise of temperature of the brake during the trial and is assumed to be equal to the difference of the two temperatures given in brackets in the table of temperature observations (Col. 3).

Col. 14 gives the terminal correction to the heat required on account of the increase of heat in the brake itself during the trial.

Col. 15 gives the difference between the mean temperature of the stuffing-box and of the shaft bearing. As already explained the loss of heat by conduction has been assumed proportional to this difference, and a determination of its amount will be given later. At present it is sufficient to say that a loss of 12 thermal units occurred per trial per unit fall of temperature along the shaft.

Col. 16 gives, therefore, the product of this difference × 12, which represents the total loss by conduction.

Col. 17. The difference of temperature between the brake and the surrounding air was taken as being equal to the difference of the mean discharge temperature of the water and that of the air. The determination of the constant representing the loss of heat per unit difference of temperature is given later, and consequently,

Col. 18 gives the product of this constant × the difference of temperature in Col. 17.

Col. 19 gives the sum of the heat in Col. 9 added to all the corrections afterwards given.

A further Table (p. 682) gives the work done, and the corrected values of the heat generated in these two trials, and the differences between them.

The value of K in the last column is then found by dividing the difference of work in Col. 4 by the difference of heat in Col. 6.

A slight inaccuracy has been pointed out to me by Professor Reynolds in the method of finding the mean temperatures of supply to and discharge from the brake. It was originally intended that the trials should be of exactly one hour's duration, and that the first series of readings should be taken one minute after the start. It was found impossible to do this, on account of the number of points requiring attention in the first few minutes, and consequently I made all trials 62 minutes long, and took the first reading two minutes after starting. The mean used has not therefore been obtained strictly in accordance with the middle breadth rule. Any error introduced would be of the occasional type, and should be eliminated in the mean of a number of trials.

July 7, 1896.

Trial No. 69 (A).

Moment on the brake . . . . 600 ft.-lbs.

Trial began at 11.17 A.M., and ended at 12.19 P.M.

Reading of revolution counter after trial	92,948
" " " before trial	75,400
Number of revolutions during trial	17,548

Weight of tank and water after trial	811·94 − ·5 lb.
" " " before trial	342·16 + ·4 "
Weight of water discharged by brake during trial, including leakage from bottom cock	468·88 lbs.

Mean temperature of water in the discharge pipe .	212·007° F. + ·04
" " " supply pipe .	33·595° − ·52
Mean rise of temperature of the water	178·972° F.

Weight of water caught from stuffing-box . . . . . = 0 lb.

Temperature of water entering the tank . . . . . = 100° F.

1	2	3	4	5	6	7	8
Times	Temperatures					Fall of temperature between stuffing-box and bearing	Readings of speed-gauge (revolutions per minute)
	Water supplied to brake	Water discharged from brake	Air	Stuffing-box cover	Lower brass of bearing		
	°	°	°	°	°	°	
Began 11.17	...	(212)	...	...	...	...	(302)
19	33·57	211·9	74·4	...	...	...	300
21	33·5	212·0	...	...	...	...	302
23	33·57	212·3	75·7	107	107	...	302
25	33·58	211·3	...	...	...	...	303
27	33·58	211·5	76·0	...	...	...	302
29	33·58	212·2	...	...	...	...	304
31	33·57	211·1	76·4	109	110	−1	302
33	33·6	211·0	...	...	...	...	299
35	33·6	211·0	76·5	...	...	...	299
37	33·6	214·9	...	...	...	...	303
39	33·6	213·7	77·5	109	111	−2	301
41	33·62	213·3	...	...	...	...	301
43	33·6	213·2	76·8	...	...	...	299
45	33·59	212·2	...	...	...	...	301
47	33·64	211·5	77·0	110	111	−1	301
49	33·62	211·8	...	...	...	...	303
51	33·64	212·0	78·1	...	...	...	304
53	33·59	212·3	...	...	...	...	299
55	33·59	212·1	76·5	110	111	−1	301
57	33·58	212·2	...	...	...	...	301
59	33·6	211·8	77·8	...	...	...	301
12.01	33·62	211·9	...	...	...	...	302
3	33·61	212·0	78·3	115	113	2	301
5	33·62	211·5	...	...	...	...	300
7	33·6	212·0	79·0	...	...	...	300
9	33·57	211·6	...	...	...	...	300
11	33·59	211·6	76·8	112	113	−1	297
13	33·57	211·5	...	...	...	...	300
15	33·6	211·3	77·1	...	...	...	301
17	33·66	211·5	...	...	...	...	301
Ended 19	...	(212)	...	...	...	...	(302)
Means......	33·595	212·007	76·9	110·3	...	−·57	...

July 8, 1896.

Trial No. 72 (A).

Moment on the brake . . . . 1200 ft.-lbs.

Trial began 11.11 A.M., and ended 12.13 P.M.

Reading of revolution counter after trial	146,311
„ „ „ before trial	129,000
Number of revolutions during trial	17,311

Weight of tank and water after trial . . .	1283·50 − 1·31 lbs.
„ „ „ before trial. . .	347·21 + ·4 lb.
Weight of water discharged by brake during trial, including leakage from bottom cock . . .	934·58 lbs.

Mean temperature of water in the discharge pipe .	212·46° F. + ·04
„ „ „ supply pipe .	34·706° − ·55
Mean rise of temperature of the water . . .	178·344° F.

Weight of water caught from stuffing-box . . . . = 0 lb.

Temperature of water entering tank . . . . . = 101° F.

1	2	3	4	5	6	7	8
	Temperatures					Fall of temperature between stuffing-box and bearing	Readings of speed-gauge (revolutions per minute)
Times	Water supplied to brake	Water discharged from brake	Air	Stuffing-box cover	Lower brass of bearing		
	°	°	°	°	°	°	
Began 11.11	...	(212·4)	...	...	...	...	(300)
13	34·74	212·3	72·0	...	...	...	302
15	34·8	211·5	...	...	...	...	300
17	34·71	212·8	73·7	97	99	−2	304
19	34·7	212·9	...	...	...	...	303
21	34·69	211·7	74·0	...	...	...	299
23	34·72	212·0	...	...	...	...	302
25	34·7	212·6	73·3	101	101	...	303
27	34·77	212·8	...	...	...	...	307
29	34·78	213·5	74·4	...	...	...	302
31	34·77	214·0	...	...	...	...	300
33	34·69	213·2	74·7	101	102	−1	301
35	35·0	213·2	...	...	...	...	299
37	34·6	214·0	75·6	...	...	...	303
39	34·7	214·4	...	...	...	...	307
41	34·76	214·0	74·7	104	103	1	302
43	34·79	212·8	...	...	...	...	304
45	34·66	213·0	74·8	...	...	...	301
47	34·75	212·3	...	...	...	...	300
49	34·66	211·6	75·7	105	104	1	297
51	34·68	211·2	...	...	...	...	302
53	34·68	212·0	75·4	...	...	...	302
55	34·66	211·6	...	...	...	...	299
57	34·66	211·0	75·3	104	105	−1	297
59	34·58	211·3	...	...	...	...	302
12.01	34·6	212·3	76·0	...	...	...	305
3	34·59	212·9	...	...	...	...	299
5	34·67	211·8	76·0	107	106	1	301
7	34·7	211·4	...	...	...	...	302
9	34·69	211·9	75·8	...	...	...	304
11	34·68	211·8	...	...	...	...	302
Ended 13	...	(211·6)	...	...	...	...	(300)
Means......	34·706	212·46	74·8	102·7	...	−0·14	...

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
Date	Trial No.	Time of start	Moment (ft.-lbs.)	No. of revolutions of engine shaft	Work done (ft.-lbs.)	Weight of water discharged by brake (lbs.)	Rise of temperature in the brake (° F.)	Heat generated, less losses due to radiation, &c. (B.T.U.)	Weight of water caught from stuffing-box (lbs.)	Rise of temperature in the brake (° F.)	Loss of heat by leakage (B.T.U.)	Rise of temperature of brake during trial (° F.)	Terminal correction to heat (B.T.U.)	Fall of temperature along shaft between stuffing-box and bearing (° F.)	Loss of heat by conduction (B.T.U.)	Difference of temperature between brake and air (° F.)	Loss of heat by radiation (B.T.U.)	Corrected heat (B.T.U.)
7 July, '96	69	11. 7 A.M.	600	17,548	66,154,556	468·88	178·972	83,916	...	...	...	...	...	−0·57	−7	135·1	1078	84,987
8 July, '96	72	11.11 A.M.	1200	17,311	130,522,170	934·58	178·344	166,677	...	...	...	−0·8	−46	−0·14	−2	137·7	1099	167,728

1	2	3	4	5	6	7
Determination No.	Trial No.	Works	Diff. of works	Heats	Diff. of heats	K
	72	130,522,170	...	167,728	...	...
	69	66,154,556	64,367,614	84,987	82,741	777·95

The Barometer.—(Part I., par. 47.)

20. Before dealing with the thermometers and their corrections, it becomes necessary to describe a combined barometer and manometer which was constructed to measure the pressures of steam employed in the determination of the boiling-points on the thermometer used to measure the discharge temperature.

The structural details of this instrument are given in Professor Reynolds' paper. At present it is sufficient to say that it consisted of a cast-iron, bottle-shaped reservoir, through the neck of which the glass tube holding the mercury column was carried in a stuffing-box, which made a perfectly air-tight joint between the glass and the reservoir. The pressure to be measured was introduced through a small iron pipe, which penetrated horizontally the cast-iron wall of the reservoir, and then turned vertically upwards till its open mouth stood above the level of the mercury inside. Two circular plate-glass windows in the reservoir walls provided a means of ascertaining the level of the mercury surface. In order to measure the height of the mercury column supported by any external pressure, a brass sleeve was made, which fitted outside the glass tube and the upper part of the reservoir. This sleeve consisted of a piece of $\frac{3}{4}$-inch diameter brass pipe fixed into a conical brass casting, which carried a truly-turned bevelled edge at its lower extremity. This conical casting engaged by an internal screw of twenty threads to 1 inch with the neck of the cast-iron reservoir. The upper part of the sleeve carried an internal thread of the same pitch, and into this was screwed a second piece of pipe through which two long narrow slits were cut at opposite extremities of a diameter. A third piece of brass pipe engaged with the upper end of the piece just mentioned, and was provided at its lower end with a truly-turned bevelled edge.

In use the bevelled edge on the conical brass casting was first adjusted to the surface of the mercury in the reservoir, and then the upper bevelled edge was adjusted to the surface at the top of the mercury column. Suitable horizontal and vertical scales were provided to enable me to measure the vertical distance between these two bevelled edges to $\frac{1}{1000}$ of an inch.

It was necessary to standardise this scale (Part I., par. 44). There is a Whitworth measuring machine in the laboratory, which is provided amongst others with standard end gauges of 9 inches and 3 inches long respectively.

Two new steel standards were made by Mr Foster as nearly as possible of the same length as the 9-inch Whitworth, and by means of the measuring machine I determined their exact lengths as follows, three comparisons being made of the two new gauges with the standard. The table shows the readings obtained.

	Whitworth standard 9-inch gauge	Laboratory standard gauge, No. 1	Laboratory standard gauge, No. 2
Readings on divided wheel of machine	0·0011 0·00112 0·00114	0·00105 0·0010 0·00097	0·00095 0·0009 0·00098
Mean readings ...	0·00112	0·001007	0·000943
True lengths......	9 inches	9 inches $-$ 0·000113	9 inches $-$ 0·000177

These three 9-inch standards, together with the 3-inch Whitworth, therefore gave a length when placed end to end of

$$30 \text{ inches} - 0{\cdot}00029 \text{ inch.}$$

The next operation was to construct a single steel standard with a length of approximately 30 inches. This bar being made, and the measuring machine not being long enough to accommodate 30 inches, the measurements were made between the centres of a large lathe in the laboratory. Two centres were made with polished flat ends. The one was put in the fixed headstock, while the second was carried by the movable sleeve of the loose headstock which had previously been securely bolted to the lathe bed in a convenient position. A temporary wooden trough was made to carry our four short standards, and correctly line them between the two centres. The reciprocating centre in the loose headstock was then gradually screwed up till the gravity piece of the measuring machine just floated between the end of the adjacent standard and the centre. A mark on the hand-wheel actuating the centre was next fixed by means of a pointer. The four standards were then removed, and the 30-inch bar substituted for them, and the operation of bringing up the centre repeated. The circumferential distance separating the pointer from the mark on the hand-wheel was then carefully measured.

A series of five of these observations were made, and the following readings taken, viz.:—

(1) $-0{\cdot}1$ inch, (3) $+0{\cdot}09$ inch, (5) $+0{\cdot}03$ inch.

(2) $-0{\cdot}05$ inch, (4) $+0{\cdot}02$ inch,

$$\text{Mean} = -0{\cdot}002 \text{ inch.}$$

The hand-wheel had a diameter of $9\frac{1}{4}$ inches, and was fixed to a screw of $\frac{1}{8}$-inch pitch.

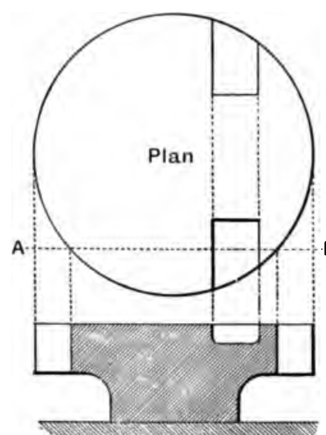
The 30-inch bar was therefore short of the length of the four steel standards by 0·0000138 inch.

Its correct length was, therefore,

$$30 \text{ inches} - 0{\cdot}0003 \text{ inch.}$$

As the barometer was only graduated to 0·001 inch, no error was introduced in assuming the bar to be exactly 30 inches long.

(Part I, par. 48.)—For the purpose of transferring this standard 30 inches to the brass sleeve forming the scale of the barometer, a circular cast-iron surface plate was made. This plate had two pieces cut out of it, as shown in the sketch. The plate was fixed with its surface level, and then the brass sleeve was placed centrally upon it, standing upright on its lower bevelled edge. In this position the portion of the surface between the two grooves cut in the plate corresponded exactly to the surface of the mercury in the barometer between the two windows previously mentioned. As it was probable that in actual use the lower bevelled edge would be slightly above the mercury surface, the sleeve was packed up by means of some very fine sheets of tissue paper till a line of light could be seen under it. Four sheets were necessary to effect this; one of these was removed, and then the standard 30-inch bar was placed inside the brass tube, standing with one end on the surface plate. The upper bevelled edge was then adjusted till the line of light between it and the top of the steel standard was obscured, and the scale was made to read 30 inches in that position.



Section A B

Together with Mr Foster I made this adjustment a number of times, but after once fixing the 30-inch mark, the reading of the length of the steel standard never varied by as much as 0·0003 inch from 30 inches.

Unfortunately, the comparison was made at a temperature of 67° Fahr., while the standard temperature of the Whitworth gauges was 60° Fahr. A formula of reduction of the readings of the barometer therefore became necessary at all temperatures.

Taking for the coefficient of linear expansion of brass per ° Fahr. 0·000012
" " " " steel " 0·0000066
" " " " the mercury column of the barometer.. 0·0001.

Then at 67° Fahr. the true length of the brass barometer scale

$$= 30\,\frac{1 + 35 \times 0{\cdot}0000066}{1 + 28 \times 0{\cdot}0000066}$$

$$= 30{\cdot}000138 \text{ inches.}$$

To find T, the temperature at which the scale gives correct readings, we have, if $T = t + 32°$,

$$\frac{1 + t \times 0{\cdot}000012}{1 + 35 \times 0{\cdot}000012} = \frac{30}{30{\cdot}000138},$$

which gives $t = 31°$ and $T = 63°$ Fahr.

The coefficient of expansion of the mercury column relative to the brass scale is 0·000088.

Now if H_T = reading of barometer in inches at $T°$ Fahr., and as before

$$t = T - 32,$$

then the corresponding corrected height of the column at a temperature of 63° Fahr.

$$= H_{63} = \frac{1 + 31 \times 0{\cdot}000088}{1 + t \times 0{\cdot}000088}\,H_T$$

$$= \frac{1{\cdot}002728}{1 + t \times 0{\cdot}000088}\,H_T,$$

and if H_0 = the corresponding pressure reduced to inches at the freezing-point, then

$$H_{63} = H_0\,(1 + 0{\cdot}0031).$$

Therefore for any required pressure H_0 inches at a temperature of 32° Fahr., the corresponding reading at $T°$ Fahr. is

$$H_T = \frac{1 + 0{\cdot}000088t}{1{\cdot}002728}\,H_0 \times 1{\cdot}0031,$$

or, allowing for the capillarity depression in a half-inch tube, this becomes

$$H_T = (1{\cdot}00037 + 0{\cdot}000088t)\,H_0 - 0{\cdot}009.$$

This formula has been used throughout to determine the steam pressures required for the verification of boiling-points to be discussed later (pars. 23 and 24).

The Thermometers.

21. The thermometers used for the measurement of the temperatures of supply and discharge of the stream of water passing through the brake were supplied by Mr J. Casartelli of Manchester.

Their indications were read through the glass walls of their respective chambers by eye simply, parallax being avoided by the use of a small mirror placed behind the thermometer in each case.

Freezing-point Thermometers.

22. Two similar thermometers were obtained, one only of which was ever used during the experiments. This was a chemical thermometer, bearing the laboratory mark 2Q, with a ¼-inch diameter stem having its scale very plainly etched in black lines on the glass. The length was 11½ inches over all, the bulb being 1½ inches long, and then at a distance of 2½ inches from the top of the bulb the graduations began. The scale extended from 30° to 45° Fahr., 6⅝ inches of the stem being occupied by the 15° mentioned. Each degree was divided into tenths, and it was easy to estimate to the hundredth of a degree.

The index error of this thermometer was repeatedly checked during the whole period occupied by the research by being immersed in a mixture of pounded ice and water.

The table appended gives the corrections and the dates on which tests were made:—

Date	Reading	Correction
5th December, 1895......	31·7	+0·3
20th December, 1895......	31·71	+0·29
9th January, 1896	31·67	+0·33
17th January, 1896	31·67	+0·33
31st January, 1896	31·57	+0·43
5th February, 1896	32·48	−0·48
20th February, 1896	32·46	−0·46
16th March, 1896	32·46	−0·46
21st April, 1896...........	32·47	−0·47
25th June, 1896...........	32·47	−0·47
7th July, 1896	32·52	−0·52

Before making the test on January 31st the hot water from the brake backed up round this thermometer, so that the sudden alteration in the reading is accounted for to some extent.

Also up to this time part of the mercury had remained stuck in the upper bulb, but Dr Harker, of the Physical Department, now succeeded in bringing the separated mercury down into contact with the column below.

By permission of Dr Schuster the scale of this thermometer was compared by Dr Harker on the 27th April, 1896, with a standardised thermometer (Baudin, No. 12,771) in his possession between the points 32° and 35° Fahr.

This comparison showed that the correction of $-0{\cdot}47$ as obtained on April 21st was correct between 33° and 34°, which was the part of the scale used in most of the experiments up to that date.

At 35°, however, the correction increased to $-0{\cdot}5$, and consequently in the later experiments, when the temperature of supply in the heavy trials approached this point, a suitable correction was made to that already obtained by immersion in the mixture of pounded ice and water.

Boiling-point Thermometers.

23. In the first instance two similar thermometers were made to order to be ready for use in the discharge tube, but on one of these being broken, two additional ones were obtained. Only one of the four was, however, used in the research, viz., P1.

This was a chemical thermometer with a $\frac{1}{4}$-inch stem, having the scale engraved as already described. The length was $16\frac{1}{2}$ inches over all, the bulb being $1\frac{1}{2}$ inches long, and a blank space of $5\frac{1}{4}$ inches separating the top of the bulb from the first graduation. The scale extended from 200° to 220° Fahr., the 20° occupying $8\frac{3}{8}$ inches of the stem.

During the course of an experiment the reading of this thermometer was continually altering slightly. This fluctuation made it almost impossible to read the temperatures to $\frac{1}{100}$th of a degree. So that only the nearest $\frac{1}{10}$th of a degree has been recorded throughout.

The English standard boiling point, viz., 212° Fahr., is defined to be the temperature of saturated steam under a pressure which would sustain a column of mercury 29·905 inches long at the temperature of melting ice at the sea level in the latitude of Greenwich.

This corresponds exactly, on being corrected for the variation in the value of gravity, to the modern definition of the boiling point on the Centigrade scale, the pressure in this case being equivalent to a column of mercury 7600 millims. long in latitude 45°, the other conditions being as before.

It was consequently possible to use Regnault's steam table in the neighbourhood of the atmospheric boiling point as a standard of comparison for the scale of this thermometer.

In order to conduct the comparison in Manchester, a knowledge of the relative value of gravity was necessary.

This was deduced from a formula given in 'Mémoires sur le Pendule' (Société Française de Physique), which is given below,

$$\frac{g\phi}{g_{45}} = (1 - 0{\cdot}00259 \cos 2\phi),$$

where $\frac{g\phi}{g_{45}}$ is the ratio of the value of gravity in latitude ϕ to its value in latitude 45°.

The latitude of Manchester being 53° 29′, this gives

$$\frac{g\phi}{g_{45}} = 1{\cdot}000756.$$

The altitude of the Owens College, Manchester, has no appreciable effect on the value given by the above formula.

I give below the table of steam pressures used in the calibration of the scale of the thermometer P1.

Temperature on Centigrade scale	Temperature on Fahrenheit scale	Pressure of steam in millims. of mercury reduced to 0° C. and sea level in lat. 45°	Pressure of steam in inches of mercury reduced to 0° C. and sea level in latitude of Manchester
99°	210°·2	733·305	28·849
100	212·0	760·000	29·899
101	213·8	787·590	30·984
102	215·6	816·010	32·102

24. The general arrangement of the apparatus used to check the scale of the thermometer P1 will be gathered from the annexed sketch (Fig. 8).

A is an ordinary copper boiling-point apparatus, the steam from the boiling water passing up an inner tube in which the thermometer to be tested is hung, and then flowing down again so as to jacket this tube, finally escaping into the atmosphere through the cock shown. The top of the inner tube is closed by a cork having two holes, in one of which is fitted a half-inch brass tube for connection with the manometer, the other carrying the thermometer.

B is a glass flask containing an artificial atmosphere, of which the pressure is under control.

C is the combined barometer and manometer used to measure the pressure in *A* and *B*.

D is the tin receiver previously described, the pressure in which is kept at about 18 inches of mercury, as measured on a U-gauge. This receiver is in free communication through a capillary glass tube with the tube connecting the flask *B* and the manometer *C*.

The bore of the capillary tube just mentioned is just sufficient to admit a very small stream of air from the receiver through the flask *B*, and so out into the atmosphere by way of the cock on the boiler. The object of this stream of air was to counteract the tendency of the steam in the boiler to diffuse down the connecting rubber tube into the flask, where condensation would occur, and possibly some water might get into the barometer, it having been found quite impossible to keep a steady pressure in the apparatus whenever the steam made its way as far as the glass flask, *B*.

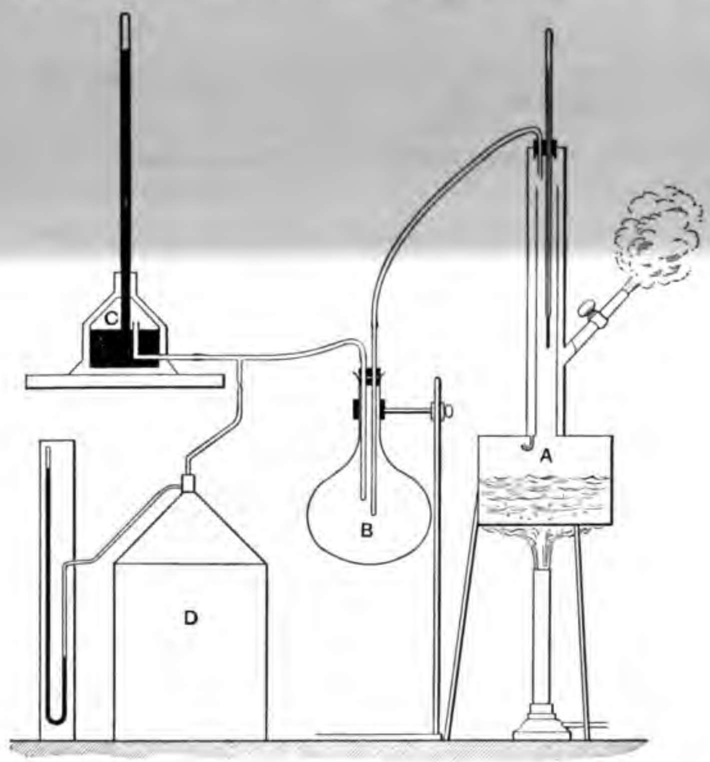


Fig. 8. Apparatus for checking boiling-points.

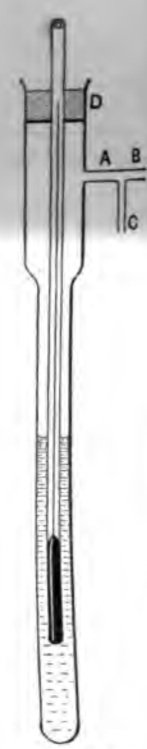


Fig. 9.

The boiler was well lagged and protected as far as practicable from draughts. A thermometer was hung alongside the brass scale tube of the barometer, and its reading was assumed to be the temperature of the barometer. Allowance having been made for this temperature, the steam escape cock was adjusted till the pressure inside the apparatus, as measured in the barometer, was at the required level. A reading was then taken of the thermometer under examination. The stem was pushed as far as possible into the boiler, the reading standing about a quarter inch above the top of the cork. Since there was always some escape of steam which blew up the hole in which the thermometer was inserted, it was not thought necessary to attempt to make any correction for the exposed part of the stem.

The annexed table gives the readings taken from this thermometer when immersed in steam of various known temperatures and the dates on which the tests were made:—

Date	Readings obtained from thermometer P1 when immersed in steam at temperature			Correction used in experiments
	212°	213°·8	215°·6	
28 Nov., 1895	211·43	213·26	215·01	+0·57
4 Dec., 1895	211·44	213·28	215·03	+0·56
5 Dec., 1895	211·5	213·33	215·07	+0·5
6 Dec., 1895	211·51 (rising)	...	...	+0·48
	211·53 (falling)	...	...	
12 Dec., 1895	At temperature 210°·46 reading was 210°·05			
9 Jan., 1896	...	213·38 (rising)	...	+0·44
		213·40 (falling)	...	
17 Jan., 1896	...	213·49	...	+0·34
23 Jan., 1896	...	213·49	...	+0·34
31 Jan., 1896	...	213·49	...	+0·34
8 Feb., 1896	211·76	213·57	215·3	+0·24
20 Feb., 1896	211·78	213·6	215·34	+0·22
	At 211°·34 reading was 211°·1			
16 Mar., 1896	211·86	213·66	215·4	+0·14
	At 211°·07 reading was 210°·87			
18 April, 1896	...	213·7	215·45	+0·11
15 June, 1896	211·94	213·74	215·5	+0·06
6 July, 1896	211·96	213·75	215·52	+0·04

25. In the case of each of these thermometers, viz., Q2 and P1, the water surrounding them was under a very considerable pressure, and it was therefore necessary to determine the effect of pressure on the reading given by each.

A piece of strong glass tube, Fig. 9, about 1 foot in length and $\frac{3}{8}$ inch inside diameter, having one end fused up, was provided with a slightly wider mouth, in which was inserted a small branch pipe, *A*. This branch again split up into two arms, one of which, *B*, was connected through a rubber tube with an air receiver in which the pressure was indicated by a U-gauge, while the other, *C*, communicated directly with the atmosphere. Each of the branches B and C could be closed at will by means of a screw clip on the rubber tubing.

The pressure tube having been about half filled with water, the thermometer under consideration was fixed inside it by means of a cork, D.

In the case of the freezing-point thermometer, Q2, the pressure tube was then surrounded with pounded ice. After the contained water had cooled sufficiently for the thermometer inside to remain steady, the communication with the atmosphere was closed, and the full pressure of the air receiver put on the thermometer bulb by opening the clip on the tube, B. The rise in the reading due to the known rise of pressure was then noted. A number of these observations were made, using different additional pressure in each case. The result obtained was that for a rise in pressure on the bulb due to 1 inch of mercury, the rise in the reading was 0·0072°.

In the case of the boiling-point thermometer, P1, the pressure tube was immersed in the steam generated in the copper boiler previously alluded to. Similar procedure gave in this case a mean rise of 0·0066° per inch rise of pressure.

After applying corrections (to be dealt with later—par. 62), rectifying the thermometric indications on this account, I think that no error of greater magnitude than 0·01° can have existed in the calculated mean rise of temperature in any trial.

On 180° this gives accuracy of 1 part in 18,000.

26. In addition to the thermometers just dealt with, three others were used, on the readings of which depended the additive corrections to the heat already referred to. One of these indicated the atmospheric temperature, while two others were placed one on the stuffing-box and the other on the shaft bearing.

On the differences of heat which were used as the divisors in the determination of the equivalent from each pair of trials, these corrections all became extremely small quantities, and therefore it was of no importance that small errors should exist in these thermometers. Their scales were therefore never calibrated. Still another thermometer was used to determine the temperature of the stream of water entering the tank. As it was only necessary to keep this temperature in each pair of trials at the same level, errors in this thermometer were negligible.

Weighing Machine and 25-lb. Weights used on the Brake.—(Part I., par. 40.)

27. The absolute value of the unit used in the graduation of the lever of the weighing machine was a matter of indifference, but it was of vital importance that the same unit should be used for the weighing machine and for the 25-lb. weights used on the brake.

A set of iron weights were, however, sent down to the Manchester Town Hall, and there compared with the Board of Trade standards.

The comparison of the 25-lb. weights with our standard 25 lbs. was one of the first things undertaken in the course of the investigation. This was done by first balancing the standard placed on the platform of a small weighing machine in the laboratory by adjustment of the rider weights on the lever of the machine. The standard was then removed, and one of the 25-lb. weights substituted, a balance being made by adding to or drilling out some of the lead inserted in the weight.

This adjustment was accepted as perfectly satisfactory till towards the close of the experiments, when a small difference in the value of the equivalent as derived from trials in which different numbers of the weights were used, seemed to suggest an error in the weights themselves.

Accordingly, on the 9th June, 1896, I again compared the weights with the standard on a temporary balance, consisting of a simple lever with three knife-edges in a straight line, with the following result:—

Weight number	True weight
1	25·00
2	25·02
3	25·03
4	25·02
5	25·01
6	24·99
7	25·02
8	25·02
9	25·03
10	25·00
11	25·04
Hanger	24·99

And a lead balance weight to be referred to later, which weighed 13·98 lbs. instead of 13·97 lbs. as assumed.

On the 17th of January, 1896, a set of four of these 25-lb. weights, at that time all supposed accurate, were used as a standard 100 lbs., by which a series of corrections to the 100-lb. scale of the weighing machine were obtained. These corrections have been used throughout the investigation, and are given below :—

Reading......	300	400	500	600	700	800	900	1000	1100	1200	1300
Correction ...	0·4	...	...	− 0·12	− 0·42	− 0·5	− 0·65	− 1·12	− 1·22	− 1·31	− 1·78

Rider weights Numbers 2 and 3 were at the same time made correct on their whole range.

In June another comparison was made, and the set of four weights, Numbers 2, 8, 9, and 10 were found to give substantially the same list of corrections as previously obtained.

The complete set of weights were then again weighed on the weighing machine, using the list of corrections given, together with the true value of the standard 100 lbs. The result was a verification of the list of their values already given.

The maximum error that might possibly be produced by using the weights on the brake in specially arranged groups was found to be—

In a pair of trials carrying moments of 1200 and 600 ft.-lbs. respectively, − 0·037 per cent. or + 0·043 per cent., and in a pair of trials run with moments of 1200 and 400 ft.-lbs. respectively, − 0·025 per cent. or + 0·03 per cent.

The value of the equivalent obtained from a set of six trials in which the weights had been specially arranged to eliminate the above possible error entirely, gave a result which did not differ at all from that previously obtained, and it may therefore be safely assumed that in the first series of trials this error did not occur to any sensible extent.

I think that, especially with the above result in view, the loading of the brake may be taken as absolutely accurate.

As to the limit of accuracy of the weighings in the 600 ft.-lb. trials, the weight of water dealt with was approximately 470 lbs. On this quantity the maximum probable error was 0·02 lb. in any trial. This gives greater accuracy than 1 part in 20,000.

The Adjustments of the Brake.

(1) *Length of the Lever.*—(Part I., par. 45.)

28. This length was required between the centre line of the engine shaft traversing the brake and the V-groove carried by the lever.

It had been previously observed that both the shaft and the brake shifted a little horizontally when the engine was started, from the positions occupied with the engine stationary. It was therefore necessary to make the comparison between the length of the lever and our standard 4-feet with the engine running. Also, since the length of the lever varied with the temperature of the brake, this temperature was maintained, as in all the trials, at 212° Fahr.

Between the brake and the adjacent bearing the shaft is 4 inches diameter within $\frac{1}{1000}$ of an inch.

At a distance of 3 feet 10 inches from one of its square ends a fine line was scribed on a steel straight edge. This straight edge was then held with the square end aforesaid butting against the shaft, the length being horizontal and perpendicular to the line of shafting, and the distance between the straight edge and the lever being 10 inches. At a distance of 11 feet from the other side of the lever a theodolite was set up and adjusted so that the vertical plane of collimation of the instrument was parallel with the shaft and contained the line scribed on the face of the straight edge.

A steel scale, graduated to $\frac{1}{50}$ of an inch, was fixed firmly on to the lever, and a reading of this scale was taken through the telescope without altering the adjustments mentioned. This reading, of course, referred to the point on the scale just 4 feet distant from the centre line of the shaft. By a slight rotation about the vertical axis the line of collimation was then made to cut the centre line of the groove, and then a vertical rotation enabled a second reading of the scale to be taken.

A number of these observations were made while the brake was subjected to moments of 1200, 600, and 400 ft.-lbs., and they all indicated that the length of the lever in the trials made was $4' + 0{\cdot}02''$.

A correction to the value of the equivalent derived directly from the trials is therefore necessary on this account. It amounts to $+\,0{\cdot}0417$ per cent.

With this correction added, I think that the length of the lever can be assumed accurate to $\frac{1}{200}$ inch, or 1 part in 10,000 nearly.

(2) *The Balance of the Brake.*—(Part I., par. 9.)

29. If a pair of trials are run, the one with a heavy indicated load, M_1, and the other with a lighter one, M_2, and if m be the moment carried by the brake on account of its initial want of balance, then the works done in the two trials are

$$U_1 = 2\pi N_1 (M_1 + m),$$
$$U_2 = 2\pi N_2 (M_2 + m),$$

where N_1 and N_2 are the revolutions in the two cases.

The difference of the work done

$$= 2\pi [N_1 M_1 - N_2 M_2 + m (N_1 - N_2)]$$

and the relative error involved in writing for this

$$2\pi (N_1 M_1 - N_2 M_2),$$

which has been done in these experiments, is

$$\frac{m (N_1 - N_2)}{N_1 M_1 - N_2 M_2}, \text{ very nearly.}$$

This error is 0 when $N_1 = N_2$.

The speed of the engine was therefore always regulated to the end that the number of revolutions in each of a pair of trials which were afterwards to be compared together should be approximately the same. As a general rule, this object was very nearly attained.

The maximum value of $N_1 - N_2$ was about 300, the values of N_1 and N_2 being approximately 18,000.

Under these circumstances, in trials carrying loads of 1200 and 600 ft.-lbs. respectively, the above error amounts to

$$\frac{300}{18000 \times 600} = \frac{1}{36000} < 0{\cdot}003 \text{ per cent. per ft.-lb. of error}$$

in the balance of the brake.

The method pursued to determine the want of balance was as follows:—

The lever was freed from all extraneous loads.

The brake and its pipe connections were then all filled with water, so as to be in the same condition as during the progress of a trial.

The lever was then lifted till its end was in its mean position opposite a pointer at a fixed height from the ground. A load was then gradually added to the front side of the brake till the friction of the bearings was overcome, and the lever fell. An observation of the moment required to cause the motion was then made. A series of twenty of these observations were made for the front and then a second series of twenty for the back of the brake, in which case the load on the back had to lift the lever from its mean position.

On taking the difference of the means of these two series of observations, the friction is eliminated and the resulting moment represents the error of balance of the brake.

Since in the course of a trial the lever oscillates a little from its mean position, the brake will, when in motion, be working against the resistance offered by the linkage connected with the regulating cock. When at rest, however, this resistance will not affect the load at all. In view of this fact, two determinations of the error in balance were made, the first with the brake working free of the linkage, by allowing the small motion to take place in the slack of the pin-joints, the second with the brake working against the resistance of the regulating apparatus. The results obtained were

In the first case, error in balance $= 45{\cdot}5$ ft.-lbs.

In the second case, error in balance $= 41{\cdot}73$ „

A mean of these two quantities would probably be approximately correct viz., 43·615 ft.-lbs.

The lead balance weight previously mentioned, and weighing 13·97 lbs. was substituted for one of the 25-lb. weights, on the removal from the lever of the brake of a rider weight and a balance weight whose combined moment (par. 40) was calculated at $-44{\cdot}12$ ft.-lbs.

The actual uncompensated error in the balance appears therefore to be practically $\frac{1}{2}$ ft.-lb. This is so small, and the balancing of the brake such a very difficult operation to perform with any approach to accuracy, that any error there may be has been ignored, and the balance assumed perfect in all the calculations.

The end of the lever has always been kept at the level of the pointer indicated before, and by this means all error due to the varying horizontal position of the centre of gravity of the brake has been avoided.

Terminal Corrections to the Apparent Heat Generated.—(Part I., par. 31.)

30. In order that the work done in any trial should be exactly equivalent to the heat generated in the water used, it was necessary that the total heat contained in the brake itself should be the same at the beginning and end of the trial.

This condition was rarely fulfilled, since it required that the weight of water in the brake, together with its temperature, should be unaltered at the close of the trial.

A determination was made of the amount of water contained by the brake at various speeds by suddenly stopping the engine when running at any given speed, simultaneously shutting off the water supply to the brake, and afterwards draining off and weighing the water shut in.

The results are shown in the annexed curves.

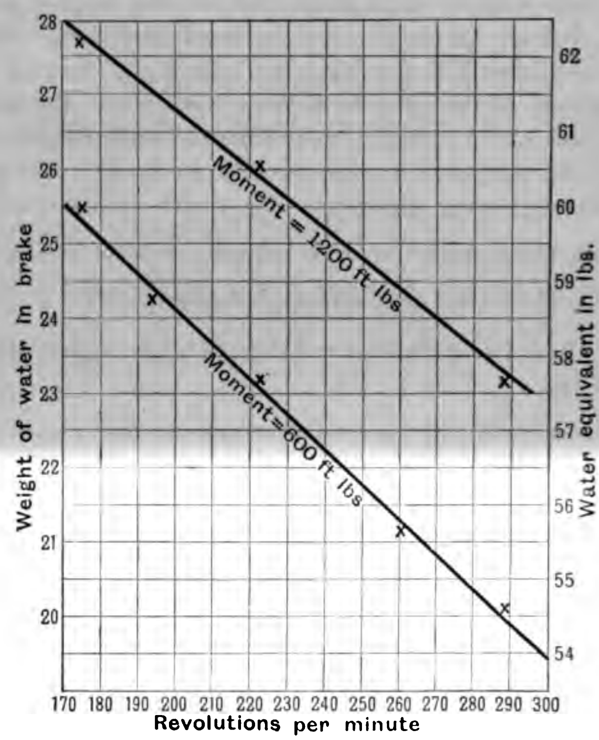


Fig. 10. Curves showing water contained by and water equivalents of brake and contents at varying speeds.

The weight of brass in the brake is 368 lbs. Taking 0·094 for its specific heat, the water equivalent is 34·6 lbs.

To obtain a scale of weights representing the water equivalents of the brake at different speeds, we have to add 34·6 to the weights of water contained at the different speeds.

This scale is given at the right of the curves just alluded to (see above).

A correction to the heat obtained is now very easily deduced.

Let w_1 = water equivalent of brake at commencement of trial.

w_2 = " " end "

t_1 = temperature of water in discharge pipe at commencement of trial.

t_2 = " " " " end "

Therefore, additional heat generated in the brake $= w_2t_2 - w_1t_1$, and this quantity is added to the heat already calculated as generated in the water.

The speed indicator, which was used in the determination of the number of revolutions per minute required as the ordinate in the curve of water equivalents, was not reliable to one or two revolutions, and, therefore, unless a large difference of speed was indicated between the commencement and end of a trial, this difference was altogether ignored, and the rise in temperature was multiplied by the constant corresponding to any particular load at 300 revolutions to obtain the terminal correction.

The speed gauge required a negative correction of 11 at 300 revolutions, and, consequently, the curves give 57·6 and 54·6 as the water equivalent of the brake when loaded with 1200 and 600 ft.-lbs. respectively.

By interpolation from the above values 53·6 was obtained and used as the water equivalent in trials carrying a moment of 400 ft.-lbs.

Loss of Water by Evaporation and Leakage from the Discharge Pipe and Tank.—(Part I., par. 37.)

31. In order to test the general efficiency of the discharge pipe as a conveyer of the water used, it was disconnected in June, 1896, from the brake, and the circulating pump was arranged to pump the water out of the tank and through the discharge pipe, which emptied itself again into the tank by means of the tipping switch.

The stream of water was regulated so as to correspond exactly with the quantities passed in trials carrying loads of 400, 600, and 1200 ft.-lbs. In a period of 62 minutes it was found that in each of these cases the loss approximated very closely to a quarter of a pound of water when its temperature was between 90° and 100°. Since this loss was the same in all the trials it has not been thought necessary to make a correction rectifying the heats on this account, for it would be completely eliminated in the differences of heat used in the calculation of the values of K given in the tables, if the interval of temperature through which the water was raised in the brake was the same in corresponding light and heavy trials.

When, however, I examined the results after the final reduction had been made, I found that the mean temperature of supply in the light trials was 0·7° lower than that in the heavy trials.

Consequently the mean difference of heat would require a slight correction, which, however, is less than −0·000002 relatively to the whole. This, being quite outside our limits of accuracy, has been ignored.

The Main Experiments.

32. In December, 1895, the apparatus, though not yet quite complete, was in a sufficiently advanced state to make it possible to commence the main K experiments.

The observations were taken and reduced in every experiment in substantially the same manner that I have described (paras. 17, 18, and 19). Some of the particulars mentioned were, however, omitted in the earlier trials, and were only recorded subsequently after their importance had come to be recognised.

In all, 80 trials were made on which any reliance has been placed, and these will be dealt with in different series, between any consecutive two of which some slight alteration had been made in the apparatus, the method of taking the observations, or of reducing the same; all these alterations leading up to the finally adopted methods which have been described.

33. I must first mention two sets of trials which do not appear in the tables. They were commenced in December, 1895, and were made mainly with the object of gaining experience in the behaviour of the apparatus, and of determining the most favourable conditions under which the experiments could be conducted.

The moments carried by the heavy and light trials in each set were 1200 and 600 ft.-lbs. respectively.

The speed was in the first set 230 revolutions per minute, and in the second set 180 revolutions per minute.

With the following exceptions the apparatus and methods were the same as described.

I. Omissions and faults in apparatus.

(1) There were no thermometers on either the stuffing-box cover or on the main bearing, and consequently no effectual attempt could be made to keep these parts of the shaft at the same temperature in a pair of trials.

(2) There was no means of catching the leakage from the stuffing-box, or from the bottom regulating cock.

(3) The rising pipe at this time only maintained a head of about 5 feet of water over the thermometer in the discharge pipe.

(4) The hand brake had not been fitted to the shaft.

II. Omissions and faults in the methods employed.

(1) No corrections were added to the heat as given by the formula $(W_2 - W_1) \times (T_2 - T_1)$.

(2) The heavy trials were of only half-an-hour's duration, in order that the second reading taken of the weight of the tank should lie on the same part of the scale of the weighing machine, which had not up to this time been corrected, in both heavy and light trials.

The results obtained were not very consistent, but, perhaps largely on that account, the trials admirably fulfilled the purpose for which they were made.

The importance of the terminal corrections were clearly indicated when the results were considered, and consequently means were at once taken to apply these corrections to the preliminary reduction of all subsequent trials. These included the provision of the hand brake, by means of which the engine speed on starting and finishing the trials could be easily controlled, and the observations of the speed of the engine and the temperature of the brake which were taken at the moments of starting and ending the trials.

Again, the terminal corrections and other incidental errors had very unequal weights when acting on the quantities obtained in the hour light trials and in the half-hour heavy trials—which latter quantities required doubling before the subtraction requisite to eliminate losses of heat could be effected.

It was therefore decided that in future all trials should be of equal duration (viz. 62 minutes), and this necessitated the immediate careful checking of the scale of the weighing machine, which was thereupon proceeded with. Furthermore, it was probable that many of the discrepancies which occurred were due to the small quantities of water it was possible to deal with at the low speeds hitherto used, and to remedy this defect a larger amount of work was done and heat generated by increasing the speed in all the recorded trials to 300 revolutions per minute. Incidentally this increase of speed was conducive to the steadier running of the engine.

I was much troubled with bubbles of steam in the discharge pipe, and to prevent their formation the rising pipe was lengthened till it gave a head of 11·3 feet over the thermometer bulb.

These trials also furnished information which led to the adoption of a pressure of 9 inches of mercury in the artificial atmosphere. It was found that with higher pressures than this the air by some means found its way into the discharge pipe, even with the lengthened rising pipe in position.

During the first few trials the only regulation of the water supplied to the bearings of the brake consisted of screw clips on the rubber pipes carrying the water. These were found to be very inefficient, and two cocks were substituted, each of which carried a scale which showed the amount to which it was open at any time.

34. Before dealing with the tables showing the final reduction of the experiments made, it is necessary to mention a preliminary reduction of trials Nos. 1 to 42 shown in Table A (p. 722), from which the constants used in the determination of the losses of heat by conduction along the shaft, and also by radiation, were deduced.

In this table the actual observations are as far as possible omitted, since they will appear later in the completely reduced tables.

It will be seen that the table consists of three similar parts, referring respectively to the heavy trials, the light trials, and the differences.

In each part

Col. 1 gives the number of the trial.

Col 2 gives the work done, calculated in the ordinary way.

Col. 3 gives the heat generated, as calculated from the formula $(W_2 - W_1)(T_2 - T_1)$, all corrections being omitted.

Col. 4 gives the terminal corrections, for which, as I have said, the necessary observations were always taken.

Cols. 5 and 6 give respectively the mean differences of temperature observed between the stuffing-box and the top and bottom brasses of the main shaft bearing.

The quantities in brackets are not actually observed differences, but were deduced in the manner to be hereafter explained (par. 43).

These differences are + or − according as the stuffing-box was hotter or colder than the adjacent bearing.

Col. 7 gives the mean difference of temperature observed between the brake and the surrounding air. These differences are, of course, all positive.

The quantities given in the part of the table headed "differences" are in every case the remainders which are left on subtracting the corresponding quantities under the heading "light trials" from those appertaining to the "heavy trials."

In the last column are given the values of K, obtained by dividing the work occurring under the heading differences, by the heat, to which has first been added the terminal correction.

The conditions under which each series of trials given in Table A was run are enumerated below.

In every case the engine speed was 300 revolutions per minute, as read on the speed-gauge.

In all heavy trials the moment was 1200 ft.-lbs., with the exception of Series IV., in which the moment was 1244·12 ft.-lbs.

In all the light trials the load was 600 ft.-lbs.

Series I.

35. This series contains trials Nos. 1 to 11, No. 5 being omitted on account of an accident to the revolution counter.

In all these trials the outer brass skin of the brake was exposed directly to the atmosphere, and consequently the loss of heat by radiation was very large.

No attempt was made to catch the small quantities of leakage occurring at the stuffing-box and the bottom regulating cock.

The water supply to the stuffing-box was only regulated to the end that the bearing should not become unduly hot, and no record was kept of the temperature gradient along the shaft till trial No. 10 was reached.

In order to avoid any bias which might be given to the experiments by always combining a trial of one type with one of another type, trials of both of which types were always made at the same relative part of any day, the relative order of running was changed as indicated by the dates and times given in Table B (Part I., par. 32). This method of combining the trials was adopted because at this time it was not as a rule possible to make more than two trials a day successfully, for breakdowns of a more or less serious nature were of frequent occurrence.

Referring now to the preliminary reduction shown in Table A, Series I.:

The values of K, Nos. I., III., IV., and V. are seen to be in close agreement, notwithstanding the comparatively rough method of reduction used.

Determination No. II., however, stands out as very distinctly higher than the others, and the cause of this was fortunately evident.

In order to prevent the attempted rotation of the small handle shown in the illustrations at the end of the brake lever, one revolution of which altered the load on the brake by 1 ft.-lb., one of my assistants had tied it to the hanger carrying the load. The string making the connection was very tight, and the load was pulled perceptibly out of the perpendicular plane passing through the groove on the lever.

This fault was sufficient to condemn the two trials Nos. 3 and 4, and they do not appear in the final table on that account.

A wooden clip was subsequently added to prevent the rotation of the handle and its attached screw.

Lagging.—(Part I., par. 33.)

36. The results given by the four accepted determinations of Series I. were so consistent that it was decided to proceed at once with the lagging of the brake, which, up to the present time, had been deferred on account of want of confidence in the apparatus generally.

The lagging consisted of a layer of about $1\frac{1}{2}$ inches of loose cotton wadding with which the whole of the exterior of the body of the brake was covered, together with the discharge pipe between the brake and the thermometer chamber. The cotton was all tied firmly in position, and the whole was enclosed in a covering of thick flannel.

As will be seen later, this lagging reduced the radiation by nearly 75 per cent. Its weight, about 2 lbs., was inappreciable, and, being evenly distributed, could not affect the balancing of the brake to any extent which it would be possible to detect.

The lagging was, I believe, of use, more especially in that it protected the bare metal from the strong draughts which often occurred in the engine-room. It required very careful attention, however, to protect it against dampness, and on this account I am not certain that better results would not have been obtained without it.

Series II.

37. With the exception of the addition of the lagging, no alteration was made in either apparatus or method between trials 11 and 12.

Sufficient experience and confidence in the apparatus had now been gained to enable me to make three trials per day, as a rule two being made in the morning and one in the afternoon, a stop of about one hour being made after the second trial. The brake was not allowed to cool down during this interval; the hot water contained on finishing the morning's run being shut in.

In Table A, the value 787·4 is given as the result of the combination of trials 12 and 14. There was evidently something amiss with this result, and as the combination of trials Nos. 13 and 14 gave the result 779·4, which agrees fairly closely with those given in Series I., the explanation which at once suggested itself was that the new lagging was damp when the day's running began and had dried before the commencement of trial 13. On this account trial No. 12 has been expunged from the final Table B, and takes no further part in the investigation.

Series III.

38. As it had by this time been found possible to run three satisfactory trials per day, the most obvious way of combining them was to make three trials, all carrying the same load, on the first day; while the trials required to complete the three determinations were run on the next convenient day.

This method was pursued during the whole of the subsequent course of the investigation.

From this series onward I made an attempt to keep the temperature gradient along the shaft, between the brake and the adjacent bearing, the same in each pair of trials. In trial No. 21 I took observations for the first time of the temperature of the lower brass in the main bearing. In these trials also the possible importance of the small leakage of water, occurring along the spindle of the lower regulating cock, for the first time became apparent. The weight of water actually leaking away had not, I think, any appreciable effect, but owing to its high temperature it was nearly all evaporated, and, consequently, may have had a sensible effect in the lowering of the temperature of the water discharged from the brake. No successful means were yet devised for catching this water. So, in this series, it still remains as a possible source of error.

Series IV.

39. For use in the regular engine trials the brake is provided with a rider weighing 48 lbs., which can be traversed along a graduated scale on the lever by means of a leading screw. In order to maintain the balance of the brake it carries at the back a second fixed load of 74·6 lbs.

These two large masses of iron had hitherto been left on the brake, but it seemed probable that they would very much affect the flow of heat away from it between any pair of consecutive trials (Part I., par. 33), for they continued to rise in temperature during the whole of any day on which experiments were made, and evidently they would absorb heat more rapidly when cold in the early part of the day than when hot later. It was therefore decided to remove them. Their combined moment about the engine shaft was

$$-44{\cdot}12 \text{ ft.-lbs.}$$

No allowance was made for this alteration in the loading of the brake, and, consequently, the moment in these trials was 1244·12 ft.-lbs., this figure having been used in the calculations given.

In order to bring the trials under some general denomination, this series has not been further reduced, nor combined with a corresponding set of light trials.

With the intention of stopping the leakage at the bottom cock, I had had some more packing placed in the gland surrounding the cock spindle. This did, to some extent, reduce the leakage, but it also had another effect which will be referred to under Series V.

Series V.

40. For the purpose of keeping the loads on the brake at the values carried by trials preceding the removal of the rider and balance weights, one of the 25-lb. hanger weights was removed, and for it were substituted some lead sheets weighing 13·97 lbs.

This lead weight then corresponded with the initial want of balance to a moment of 100 ft.-lbs., made up as follows:—

Want of balance..................	44·12	ft.-lbs.
Moment of lead weight.........	55·88	„
	100	„

After these trials had been made, I determined, with Professor Reynolds, by means of a spring balance, the force necessary to move the bottom cock.

This was found to amount to a moment of 30 ft.-lbs. on the brake, and on this account this series of trials, though appearing in the final tables, have not been allowed any weight in the calculation of the final mean value of K. The preliminary reduction of Table A gave what were apparently very good values of K, but this only shows the small effect on the mean moment produced by variations in the resistance offered to the brake's motion, and this although its period of oscillation was very long.

Series VI.

41. These trials differ from those of Series V. only in the fact that the extra packing had been removed from the gland on the cock spindle, while a means of catching the whole of the leakage, and at the same time preventing its evaporation, had been provided (par. 14). The whole of the leakage was credited with the temperature of the water in the discharge pipe, and was weighed with the main stream of water which had been caught in the tank.

Series VII.

42. These trials were made under similar conditions to those in Series IV. In the two last trials, however, viz., Nos. 39 and 42, some leakage was observed and caught from the stuffing-box.

An approximate estimation of the loss of heat due to this leakage is given in Table B, and has been included in the heats given in Table A.

Determination of the Loss of Heat by Conduction along the Shaft.

43. In the trials enumerated in Table A, the varying values of the temperature gradient, existing in the shaft leaving the brake, might evidently be a cause of comparatively large losses of heat which were not eliminated in the differences of heat, so far assumed to be equal to the corresponding differences of work.

It therefore became important to determine, at least approximately, what was the loss of heat by conduction along the shaft in each trial.

I have already said that the temperature of the shaft in the main bearing was assumed to be the same as that of the lower brass, while the temperature on leaving the brake was similarly taken as that of the stuffing-box cover.

Unfortunately, before trial No. 21, I had made no record of the temperature of the lower brass.

It was, however, found that in trials Nos. 21 to 41 the mean temperature of the lower brass exceeded that of the upper brass by about 7° Fahr.

Consequently, in Column 6, in the parts of Table A, where no observations had been taken, an estimation of the difference of temperature between the stuffing-box and the lower brass was made by subtracting seven from the difference occurring in Column 5. In this manner the differences entered in brackets were obtained for trials Nos. 10 to 20.

It appears that we have, therefore, ten determinations, viz., V., VI., VII., VIII., IX., X., XI., XII., XIII., and XVIII., in which the differences of heat generated require a positive correction on account of the unbalanced conduction along the shaft, and four determinations, viz., Nos. XIV., XV., XVI., and XVII., in which those differences require a negative correction.

Assuming, as is very nearly the case, that the losses of heat by radiation are eliminated in the differences of the heats, it follows that by taking C = loss of heat per trial, by conduction along the shaft, per unit difference of temperature between the stuffing-box and lower brass,

C is given by the equation

$$\frac{675844869}{867995 + 75{\cdot}6C} = \frac{271143956}{348866 - 22{\cdot}5C} = K,$$

where the numerators represent the sums of the differences of work in the sets enumerated above, while the first terms of the denominators represent the sums of the differences of heat in the same sets, to which the terminal corrections have been added. The second term in each denominator represents the correction to be applied to the differences of heat for unbalanced conduction along the shaft.

On solving the equation we get

$$C = 12, \text{ very nearly.}$$

This agrees very closely with the value $C = 13{\cdot}61$, which may be calculated from the dimensions of the conducting shaft, viz., 4 inches diameter and $2\frac{3}{4}$ inches long, and Forbes' value of the conduction coefficient for iron, viz.:

(0·1429 in C.G.S. unit).

Since nothing was known as to the internal thermal condition of the shaft, the figure 12 has been used throughout as a sufficiently close approximation to the constant required.

The corrections to the heat for conduction along the shaft in each trial were then obtained by multiplying the fall of temperature between the brake and bearing by 12.

The sign of the correction varies, of course, with the sign of the temperature gradient along the shaft.

Determination of the Loss of Heat by Radiation.

44. Under this heading are included all losses of heat not already dealt with under the headings "terminal corrections," "loss by conduction," and "loss by leakage of water."

Radiation in the Unjacketed Trials.—Series I.

45. Determination No. II., consisting of a combination of trials 3 and 4, is omitted, for the reasons given. A constant R, representing the loss of heat by radiation per trial per unit difference of temperature between the brake and surrounding air is required.

In Tables B and C the corrections to the heat are given for terminal errors and conduction along the shaft, the calculation of which has been explained.

The quantities given in the annexed table are sums obtained by adding together the corresponding quantities in Series I. of Tables B and C.

In trials 1, 6, and 9 the loss by conduction has been assumed the same as in trial 10; while in trials 2, 7, and 8 this loss has been given the same value as calculated for trial No. 11.

SERIES I.—Unjacketed Trials.

	Work done	Heat	Terminals	Conduction	Diff. of temperature between brake and air
Heavy trials ...	542,876,020	677,309	+ 19	+116	556·4
Light trials ...	272,418,189	330,280	−131	−496	558·4

We have, therefore, the same value of K given by

$$K = \frac{542{,}876{,}020}{677{,}444 + 556{\cdot}4\,R} = \frac{272{,}418{,}189}{329{,}653 + 558{\cdot}4\,R},$$

and, solving for R, we get

$$R = 36{\cdot}86,$$

or, using this value of R and solving for K,

$$K = 777{\cdot}81,$$

which is the mean value deduced from this series of eight unjacketed trials.

Radiation Coefficient for Jacketed Trials, Nos. 12 *to* 42.

46. As in Series I., we get the sums of work, heat, &c., shown in the annexed table:—

	Work done	Heat	Terminals	Conduction	Diff. of temperature between brake and air
Heavy trials ...	1,752,718,746	2,236,681	− 64	− 886	1862·6
Light trials ...	874,319,846	1,108,013	− 183	− 1369	1872·5

In this table the sums are given of the respective quantities in the trials used in Determinations VI. to XVIII. inclusive, Series No. V. being included, because no error was apparent in the quantities obtained; Series No. IV. being omitted, since the moment given could not be guaranteed correct with any certainty.

We thus get the following equation for R:—

$$\frac{874,319,846}{1,106,461 + 1872{\cdot}5\,R} = \frac{1,752,718,746}{2,235,731 + 1862{\cdot}6\,R},$$

which, on solution, gives

$$R = 9{\cdot}33,$$

and, substituting for R,

$$K = 777{\cdot}91.$$

47. The loss of heat by radiation from the brake, as given in the Tables B, C, &c., was determined by multiplying the difference of temperature between the brake and the air by the radiation constants, calculated as just described.

The Tables B, C, and D, giving the results of trials 1 to 42 inclusive, should now be self-explanatory.

The mean value of K given by the eight unjacketed trials I have mentioned was 777·81.

48. The best way of stating the values of K obtained throughout seemed to be as follows:—

The sums of the differences of the works and of the corrected heats were taken for each series of trials, and then a mean value of K for the series was found by dividing the first of these quantities by the second.

The values of K given as the mean for each series in Table D have been calculated in this way.

49. A mean value of K can be obtained from the jacketed trials contained in Series II., III., VI., and VII. (Series V. being kept out of the determination on account of the possible error already noticed), by finding the sums of the respective differences of work and heat given with each of these series in Table D, and then dividing the work by the heat so obtained.

The sum of the differences of work in Series II., III., VI., and VII.

$$= 676{,}259{,}560,$$

and the sum of the corresponding differences of heat

$$= 869{,}396;$$

therefore the mean value of K given by the accepted jacketed trials so far considered is

$$K = \frac{676{,}259{,}560}{869{,}396} = 777{\cdot}85.$$

From this mean none of the values obtained from any one of the above series differs by as much as 0·03 per cent.

Closer agreement than this could not possibly be expected, and it was consequently decided to vary the trials somewhat, in order to determine if any errors had been overlooked. For this purpose I made two fresh series of six trials each, the light trials carrying a moment of 400 ft.-lbs. only, none of the other conditions being altered in any way.

50. The full reduction of these Series (Nos. VIII. and IX.) is shown in the two Tables E and F.

As before, three trials were run on each day, but the last trial, on April 1, was not finished on account of an accident preventing me getting the correct weight of the water discharged by the brake. There are, consequently, only eleven trials in the tables. The radiation constant for these trials worked out to 8·16.

The mean value of K, given by the whole eleven trials, was 778·14, which is lower than the two means for the separate series in Table F, on account of the inclusion of the light trial No. 45, which does not appear in Table F.

This new value of K, viz. 778·14, did not agree so closely with the former one of 777·85 as we had hoped, and, after reducing the last two series of trials, I devoted all my time to the checking of the whole of the apparatus anew.

It was a consequence of this stringent supervision of every separate part that the small errors in the 25-lb. weights, already noticed, were discovered (par. 27).

51. Calculation showed that this error might account for the discrepancy observed, and so it was decided to run a fresh series of trials with the weights so arranged that no error could appear on their account.

In order to have no known outstanding errors whatever, I made a small rectangular trough, fitted with a drain-pipe, by means of which all leakage from the stuffing-box was caught.

52. A series of fifteen trials, numbered 54 to 68 inclusive, was accordingly made, beginning on June 29, 1896. Owing, no doubt, to the long rest which the apparatus had had since Easter, a number of accidents were met with which completely spoiled the whole series.

The lagging of the brake was very damp when the series was begun, and, on account of the bursting of the various rubber-pipe connections, it did not thoroughly dry during the whole course of this series of trials.

For these reasons the results are not tabulated.

53. After remedying all the defects which had developed in the previous week's running I made two fresh series of six trials each between July 7 and 10 inclusive.

No further accidents occurred and the results were in every way satisfactory.

These are shown in Tables G and H.

The radiation constant worked out at $R = 7{\cdot}98$.

The mean value of K, given by the two series, was

$$K = 777{\cdot}85,$$

which happens to be exactly the same as obtained previously from Series II., III., VI., and VII.

54. This last lot of trials afforded no explanation of the small difference $(778{\cdot}14 - 777{\cdot}85)$

$$= 0{\cdot}3 \text{ ft.-lb. nearly,}$$

which occurred between the results given by the 1200—600 ft.-lb. determinations and the 1200—400 ft.-lb. determinations respectively.

The difference, of course, may be due to terminal errors, which, I think, have been mainly responsible throughout for the small discrepancies found to occur between individual determinations. It is more likely, however, that the small quantity of water dealt with in the 400 ft.-lb. trials, and the consequent greater effect of the oscillations of the brake on the mean moment, may have introduced some error into these lightly-loaded trials. Further, some slight bias may have been given to the Series, Nos. VIII. and IX., by the long rest caused by the Easter Vacation, between trials 47 and 48.

55. In the annexed table I give the mean value of the work done and of the heat generated in the heavy and light jacketed trials respectively, against which no known sensible error can be placed.

Trials Numbers	Mean work per trial	Mean heat per trial
Heavy trials: (13, 17, 18, 19, 20, 35, 36, 37, 38, 39, 46, 47, 48, 49, 50, 72, 73, 74, 75, 76 and 77)	134,337,403	172,685
Light trials: (14, 15, 16, 21, 22, 23, 33, 34, 40, 41, 42, 43, 44, 45, 51, 52, 53, 69, 70, 71, 78, 79 and 80)	61,355,503	78,867
Differences	72,981,900	93,818

and dividing the mean difference of work by the mean difference of heat we have

$$K = 777 \cdot 91.$$

This mean value of K deduced from the experiments requires correcting on a few counts, which are due to the method of working. These will be dealt with later.

56. The table given on page 714 illustrates the almost perfect manner in which losses of heat were eliminated on the mean result, by the method adopted throughout the investigation of always working on the differences of the quantities of work done and heat generated in a pair of trials.

A value of K can be obtained by dividing the difference of work in Column 3 by the uncorrected difference of heat in Column 4. This operation gives

$$K = 778 \cdot 06.$$

The various corrections which this number requires are as follows:—

I. Correction due to difference in number of revolutions of shaft between light and heavy trials.

Since the difference in the number of revolutions is only 15, this correction, as previously indicated, when dealing with the balance of the brake, will be zero (par. 29).

	No. of revolutions of shaft	Work done	Heat generated, less losses due to terminals, conduction, &c.	Loss of heat by leakage of water	Terminal corrections	Difference of temperature between stuffing-box and bearing	Difference of temperature between brake and air
Means for 21 accepted heavy trials	17,817	134,337,403	171,510	4	−1	−3·9	140·5
Means for 23 accepted light trials	17,832	61,355,503	77,710	1	−7	−5·4	141·5
Differences ...	−15	72,981,900	93,800	3	6	1·5	−1·0
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

II. Correction due to loss by leakage of water from the brake.

$$\text{This correction amounts to } -\frac{3}{93{,}800} = -0{\cdot}000032.$$

III. Correction due to terminal differences of temperature of the brake.

$$\text{This correction amounts to } -\frac{6}{93{,}800} = -0{\cdot}000064.$$

IV. Correction due to loss of heat by conduction along the shaft.

$$\text{This correction amounts to } -\frac{1{\cdot}5 \times 12}{93{,}800} = -0{\cdot}000192.$$

V. Correction due to loss of heat by radiation.

Assuming 9 for the value of the radiation constant, this becomes

$$= +\frac{9}{93{,}800} = +0{\cdot}000096.$$

The total correction factor is therefore (1 − 0·000192), which gives as before

$$K = 777{\cdot}91.$$

Corrections to the Mean Value of K given by the Experiments.

I. Length of Brake Lever.

57. In dealing with the calibration of the measurements of the brake (par. 28), I have already mentioned that the value of K given by the experiments would require a correction factor of $(1 + 0{\cdot}00042)$.

II. Salts Dissolved in the Manchester Water.

58. Professor Dixon kindly furnished Professor Reynolds with the results of a number of analyses of the town's water made during the College session, 1894—95. The dissolved salts were

$$\left.\begin{array}{ll}\text{Common Salt,} & 14{\cdot}4 \\ \text{Calcium Carbonate,} & 27{\cdot}7\end{array}\right\} \text{ milligrammes per litre;}$$

therefore the proportion of salts by weight is 0·0000421. Taking their specific heat at 0·2, we get for the correction factor required, due to the lowering of the specific heat of the water,

$$1 + (1 - 0{\cdot}2) \times 0{\cdot}0000421 = (1 + 0{\cdot}00003).$$

III. Air Dissolved in the Water Used.—(Part I., par. 43.)

59. Being rain water it probably contained about $2\frac{1}{2}$ per cent. by volume of dissolved air. As affecting the specific heat of the water, this air would not have of itself any sensible influence.

It did, however, influence the resulting final temperature, as it was most probably all boiled out of the water, and the bubbles of expelled air would all be saturated with water vapour at a temperature of 212°, which vapour could not be formed without extracting its latent heat from the surrounding water.

I made some experiments in December, 1896, with the object of determining the actual volume occupied by the bubbles of mixed air and water vapour under the conditions obtaining in the trials. The pressure on the water in the discharge-pipe was 10 inches of mercury very nearly.

The method adopted was as follows:—

I put a depth of about two inches of mercury into the bottom of a strong bolt-head flask, and above the mercury I poured in $1\frac{1}{4}$ lbs. of water. This filled the flask nearly to the brim. A rubber stopper, through which passed a glass tube, was then pressed into the neck of the flask, the glass tube being of such a length that the insertion of the stopper displaced mercury only up the tube, care being taken that no bubbles of air were included under the stopper. The stopper was then firmly tied into the neck, and the flask was hung inside a large glass beaker, which was then filled with water to a depth which covered the top of the rubber stopper.

One end of a piece of strong rubber tube was then fastened on the glass tube protruding from the flask, while its other end was fixed to the vessel shown at A, which was open to the atmosphere.

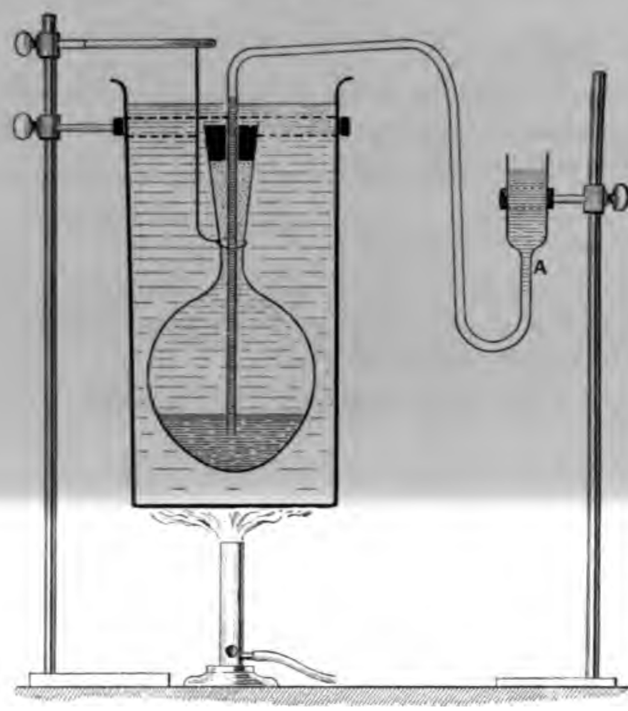


Fig. 11.

Mercury was poured into the glass funnel at A, and it was raised till there was a solid column of mercury from the bottom of the flask to the surface in A. The water in the beaker was then heated by a Bunsen flame till it boiled. This boiling was continued during a whole day, the water in the beaker being replenished as required. By adjusting the level of the free surface of the mercury at A, any required pressure could be put on the vapour column which formed over the water in the flask neck and displaced some of the mercury from the bottom. Also, by suddenly raising the pressure, the vapour was compressed and cold mercury flowed down into the flask, condensing the vapour in the neck as it descended. By this means the water in the flask could be made to boil briskly for a few moments now and then, so as to facilitate the escape of the air. At the close of the day the levels of mercury and water were adjusted so as to give the requisite pressure on the vapour column. The length of this column was then measured, and knowing the diameters of the flask neck and tube, it was easy to calculate the volume of vapour.

This was 2·2 cubic inches.

If this be reduced to a temperature of 32° and atmospheric pressure, the proportion of air by volume appears to be 1·6 per cent.

This number is considerably less than the 2·5 per cent. already mentioned, but as it was determined under conditions which approximated closely to those which held in the main trials, it was used in the calculation of the correction given below.

The weight of water vapour at a temperature of 212° per cubic foot

$$= 0{\cdot}03797 \text{ lb.}$$

Therefore the correction due to the loss of the latent heat necessary to evaporate this weight of water, is, relatively to the 180 thermal units generated per lb. of water discharged by the brake,

$$\frac{4}{5} \times \frac{2{\cdot}2}{1728} \times \frac{0{\cdot}03797 \times 966}{180} = 0{\cdot}00021.$$

The correction factor is therefore $(1 - 0{\cdot}00021)$.

IV. Reduction of the Weighings to Vacuo.—(Part I., par. 41.)

60. Taking the density of water

$$= 62{\cdot}425,$$

and of air at 32° Fahr.

$$= 0{\cdot}08073,$$

and also assuming 70° Fahr. as the mean temperature of the engine-room during the trials, the correction factor becomes

$$1 - 0{\cdot}08073 \times \frac{493}{531} \times \frac{1}{62{\cdot}425} = 1 - 0{\cdot}00120.$$

In the calculation of this factor it must be borne in mind that the density of the air causes errors of equal magnitude in the measurement of both work and heat on account of the alteration of apparent density of the cast-iron weights used on the brake and on the lever of the weighing machine.

V. Varying Specific Heat of the Water.—(Part I., par. 51.)

61. According to Regnault the mean specific heat of water between freezing and boiling points is 1·005, assuming the specific heat unity at the

lower temperature. If his formula for the specific heat be correct, then a correction factor of (1 − 0·00006) is necessary to make the value of K derived from the trials represent this mean specific heat. This factor is introduced because it was not strictly the whole range of temperature between freezing and boiling points which was dealt with in the trials, for the cold water supplied to the brake had various temperatures ranging from 32·7° to 34·3°. This correction would only just affect the second decimal place, and in consideration of the uncertainty that exists as to the exact value of the specific heat of water at any temperature, I do not propose to use a correction factor on this account.

VI. Corrections due to the Fall in Pressure between the Supply and Discharge Pipes.

62. From observations taken on October 1st, 1896, I determined the pressure on the thermometer in the supply pipe to be :—

In the 1200 ft.-lb. trials	....	15 inches of mercury.
„ 600 „ „	....	11 „ „
„ 400 „ „	....	9·7 „ „

I have already stated that the pressure on the thermometer in the discharge pipe was 11·3 feet of water in all trials.

From these varying pressures two corrections are obtained as follows:—

(*a*) Elevation of Temperature Readings by the Pressure on the Thermometers.

	1200 ft.-lbs.	600 ft.-lbs.	400 ft.-lbs.
Pressure on thermometer bulb in supply pipe in inches of mercury	15·0	11·0	9·7
Consequent elevation in readings of temperature (0°·0072 per inch)	0°·108	0°·0792	0°·0698
Pressure in discharge pipe in feet of water	11·3	11·3	11·3
Consequent elevation in readings of discharge temperature (0°·0066 per inch of mercury)	0°·066	0°·066	0°·066
Percentage correction to heat obtained	$\frac{0·042}{1·8}$ = 0·0233	$\frac{0·013}{1·8}$ = 0·0072	$\frac{0·004}{1·8}$ = 0·0022

If we now confine our attention to the combination of 1200 and 600 ft.-lb. trials, the relative correction to the difference of heat is

$$\frac{0\cdot000233 - \frac{1}{2} \times 0\cdot000072}{\frac{1}{2}} = 0\cdot000394,$$

i.e., the correction factor to K on this account is

$$(1 - 0\cdot000394).$$

Considering next the 1200—400 ft.-lb. determinations, the relative correction to the difference of heat is

$$\frac{0\cdot000233 - \frac{1}{3} \times 0\cdot000022}{\frac{2}{3}} = 0\cdot000339,$$

which makes the correction factor

$$(1 - 0\cdot000339).$$

On the mean value of K deduced from the trials, I propose to make this factor

$$(1 - 0\cdot00037).$$

63. (*b*) GENERATION of Heat in the Water on account of the Loss of available Head between the Supply and Discharge Pipes. (Part I., par. 53.)

	1200 ft.-lbs.	600 ft.-lbs.	400 ft.-lbs.
Head in supply pipe in feet of water	17·0	12·45	10·98
Loss of head before reaching the discharge pipe in feet	5·7	1·15	−0·32
Correction required by the work given in the tables per cent.	$\frac{5\cdot7}{1\cdot8\times777}$	$\frac{1\cdot15}{1\cdot8\times777}$	$\frac{-0\cdot32}{1\cdot8\times777}$
	=0·0041	=0·0008	= −0·0002

Therefore the correction factors required are—

(*α*) For the 1200—600 ft.-lb. determinations

$$1 + \frac{0\cdot000041 - \frac{1}{2} \times 0\cdot000008}{\frac{1}{2}} = (1 + 0\cdot000074).$$

(*β*) For the 1200—400 ft.-lb. determinations

$$1 + \frac{0\cdot000041 - \frac{1}{3} \times 0\cdot000002}{\frac{2}{3}} = (1 + 0\cdot000063).$$

This factor also I propose to give the value

$$(1 + 0\cdot00007),$$

when applied to the mean value of K deduced from all the trials.

VII. Correction due to the manner of Engagement of the Revolution Counter with the Engine Shaft.—(Part I., par. 34.)

64. The spindle of the counter carried a wire pin parallel with the axis of revolution, which pin was driven by another carried by, and passing at right angles through, the axis of the spindle making connection with the engine shaft.

The mean chance was therefore that at every engagement of the counter with the shaft one-fourth of a revolution would be lost by the instrument, while on disengaging the counter stopped the instant it was withdrawn.

The work in every trial should therefore be increased to compensate for this loss.

The number of revolutions was approximately 18,000.

The correction factor is therefore

$$1 + \frac{1}{72,000} = (1 + 0{\cdot}00001).$$

65. A summary of these corrections is appended.

Cause of correction	Magnitude and sign	
	+	−
I. Length of lever	0·00042	...
II. Dissolved salts	0·00003	...
III. Dissolved air	...	0·00021
IV. Weight of atmosphere	...	0·00120
V. Varying specific heat of water	Neglected	...
VI. (a) Effect of pressure on thermometers	...	0·00037
(b) Loss of head in the water	0·00007	...
VII. Engagement of revolution counter	0·00001	...
Totals	0·00053	0·00178

Therefore the final correction factor is

$$(1 - 0{\cdot}00125).$$

66. Applying this correction factor to the value obtained from the experiments, we get for the value of the mean specific heat of water between freezing and boiling points, expressed in mechanical units, at Manchester,

$$777{\cdot}91\,(1 - 0{\cdot}00125),$$

$$\mathbf{776{\cdot}94}.$$

APPENDIX.

Although no part of this research, it may be interesting to notice that reduced to the latitude of Greenwich this becomes

$$777{\cdot}07,$$

and reduced to latitude 45° at sea-level

$$777{\cdot}53.$$

Expressed in metre-grammes and the centigrade unit of heat this last value becomes

$$426{\cdot}58.$$

The value of g being

$$980{\cdot}63,$$

we have for the mean value of the specific heat of water between 0° and 100° C., expressed in absolute C.G.S. units,

41,832,000 ergs.

Making use of Regnault's formula for the specific heat of water at different temperatures, this would give the mechanical equivalent of the heat required to raise 1 lb. of water at 60°·5 Fahr. through 1° Fahr. at Manchester as

773·74 ft.-lbs.,

and taking water at 32° Fahr., this gives

773·07 ft.-lbs.

Similarly expressing the result in absolute C.G.S. units, we have for the mechanical equivalent of the heat necessary to raise 1 gramme of water through 1° C. in latitude 45° and at sea-level

(*a*) From a temperature 15°·8 C. .... 41,660,000 ergs.

(*b*) „ „ 0° C. .... 41,624,000 ergs.

TABLE A.—SHOWING THE PRELIM

K Determination number	Heavy trials. Moment, 1200 ft.-lbs.							Light	
	Trial number	Work done	Heat generated	Terminal correction	Difference of temperature between stuffing-box and upper brass	Difference of temperature between stuffing-box and lower brass	Difference of temperature between brake and air	Trial number	Work done
Series Number I.									
I.	1	134,201,602	167,191	+ 11	...	...	139·3	2	68,310,950
II.	4	138,446,542	172,957	− 63	...	...	140·5	3	68,182,773
III.	6	135,935,775	169,686	+ 31	...	...	137·6	7	67,926,419
IV.	9	136,063,953	169,859	− 10	...	...	138·9	8	68,096,065
V.	10	136,674,680	170,573	− 13	+ 9·4	(+ 2·4)	140·6	11	68,084,755
Series Number II.									
	12	133,628,584	169,519	− 40	+10·1	...	144·4		Combined with
VI.	13	135,392,907	172,591	+ 12	+ 9·3	(+ 2·3)	143·8	14	67,933,958
VII.	17	135,098,853	172,408	+ 6	− 3·9	(−10·9)	140·2	16	67,677,604
								15	67,658,754
Series Number III.									
VIII.	18	133,734,142	170,604	− 69	+ 6·3	(− 0·7)	141·0	21	66,580,557
IX.	19	133,892,479	170,867	+ 63	+ 1·1	(− 5·9)	140·9	22	67,142,275
X.	20	135,332,588	172,666	− 29	+ 0·3	(− 6·7)	140·8	23	66,765,283
Series Number IV.									
	24	139,870,565	178,183	+104	+ 2·1	− 6	141·7		
	25	139,448,444	177,847	+ 40	− 1·4	− 7·4	139·1		
	26	140,073,809	178,984	− 40	− 3·1	−10·2	139·3		
Series Number V.									
XI.	30	134,073,435	171,054	− 12	+ 7·9	− 1·3	145·8	27	67,353,391
XII.	31	134,623,843	171,793	− 12	− 6·3	−11·7	145·4	28	67,146,045
XIII.	32	135,257,190	172,618	...	− 4·0	−10·3	141·4	29	67,315,692
Series Number VI.									
XIV.	35	134,744,481	171,995	+ 6	− 2·9	− 9	144·9	33	67,692,684
XV.	36	135,702,040	173,226	− 6	− 3·1	− 9	143·4	34	66,765,283
Series Number VII.									
XVI.	37	134,819,879	172,059	− 6	+ 8·9	− 0·3	145·9	40	67,703,993
XVII.	38	135,151,632	172,550	− 17	+ 1·1	− 4·4	144·3	41	67,112,116
XVIII.	39	134,895,277	172,250	...	− 0·3	− 5·9	144·8	42	67,130,965

'CTION OF TRIALS, 1 TO 42 INCLUSIVE.

nt, 600 ft.-lbs.			Differences							
Difference of tempera-ture between stuffing-box and upper brass	Difference of tempera-ture between stuffing-box and lower brass	Difference of tempera-ture between brake and air	Work done	Heat gene-rated	Terminal correction	Difference of tempera-ture between stuffing-box and upper brass	Difference of tempera-ture between stuffing-box and lower brass	Difference of tempera-ture between brake and air	Preliminary value of K obtained
...	...	137·4	65,890,662	84,565	+ 11	...	...	...	779·1
...	...	140·7	70,263,769	89,867	− 57	...	...	...	782·4
...	...	140·8	68,009,356	87,254	+122	...	...	...	778·3
...	...	139·1	67,967,888	87,134	+ 19	...	...	...	779·9
− 3·3	(−10·3)	141·1	68,589,925	88,076	− 2	...	(+12·7)	...	778·8
...	...	...	...	...	...	...	...	...	787·4
−11·2	(−18·2)	142	67,458,949	86,537	+ 17	...	(+20·5)	...	779·4
−10·9	(−17·9)	140	67,421,249	86,589	− 49	...	(+ 7)	...	779·1
− 1·9	(− 8·9)	140·8							
+ 3	− 2·6	145·3	67,153,585	86,431	− 64	...	(+ 1·9)	...	777·5
− 1·9	− 7·9	144·3	66,750,204	85,855	+101	...	(+ 2)	...	776·6
− 2	− 7·7	144·4	68,567,305	87,963	+ 26	...	(+ 1)	...	779·3
− 2·6	−11·1	149·4	66,720,044	85,710	− 7	...	+ 9·8	...	778·5
−11·8	−18·3	144·2	67,477,798	86,646	− 17	...	+ 6·6	...	778·9
− 6·2	−13·7	140·4	67,941,498	87,212	+ 16	...	+ 3·4	...	778·9
+ 2·1	− 5·1	145·7	67,051,797	86,271	+ 61	...	− 3·9	...	776·7
+ 0·9	− 5·0	144·2	68,936,757	88,601	− 6	...	− 4·0	...	778·1
+24·9	+11·6	146·5	67,115,886	86,504	+ 21·0	...	−11·9	...	775·7
+ 4·0	− 1·7	143·0	68,039,516	87,415	− 0·9	...	− 2·7	...	778·4
...	−16·6	143·1	67,761,312	86,934	+ 21·0	...	+10·7	...	779·3

TABLE B.

Date	Trial No.	Time of start	Moment (ft.-lbs.)	No. of revolutions of engine-shaft	Work done (ft.-lbs.)	Weight of water discharged by the brake (lbs.)	Rise of temperature in the brake (° F.)	Heat generated, less losses by radiation, &c. (B.T.U.)	Weight of water caught at stuffing-box (lbs.)	Rise of temperature in the brake (° F.)	Loss of heat by leakage (B.T.U.)	Rise of temperature of brake during trial (° F.)	Terminal correction to heat (B.T.U.)	Fall of temperature along shaft between stuffing-box and bearing (° F.)	Loss of heat by conduction (B.T.U.)	Difference of temperature between brake and air (° F.)	Loss of heat by radiation (B.T.U.)	Corrected heat (B.T.U.)
							Series	*No. I.*										
Feb. 5, '96	1	11.5	1200	17,799	134,201,612	935·53	178·713	167,191	...	...	...	0·2	11	(2·4)	29	139·3	5135	172,366
Feb. 12, '96	6	1.57	1200	18,029	135,935,775	948·39	178·92	169,686	...	...	...	−0·2	31	(2·4)	29	137·6	5072	174,818
Feb. 13, '96	9	2.8	1200	18,046	136,063,953	952·09	178·406	169,859	...	...	...	0·2	−10	(2·4)	29	138·9	5120	174,998
Feb. 19, '96	10	11.21	1200	18,127	136,674,680	954·46	178·711	170,573	...	...	...	−0·6	−13	(2·4)	29	140·6	5183	175,772
							Series	*No. II.*										
Feb. 28, '96	13	2.14	1200	17,957	135,392,907	965·37	178·782	172,591	...	...	...	0·2	12	(2·3)	28	143·8	1342	173,973
Mar. 5, '96	17	2.52	1200	17,918	135,098,853	964·71	178·715	172,408	...	...	...	0·1	6	(−10·9)	−131	140·2	1308	173,591
							Series	*No. III.*										
Mar. 6, '96	18	10.22	1200	17,737	133,734,142	952·88	179·04	170,604	...	...	...	−1·2	−69	(−0·7)	−8	141·0	1316	171,843
Mar. 6, '96	19	11.53	1200	17,758	133,892,479	955·85	178·759	170,867	...	...	...	1·1	63	(−5·9)	−71	140·9	1315	172,174
Mar. 6, '96	20	2.26	1200	17,949	135,332,588	969·38	178·12	172,666	...	...	...	−0·5	−29	(−6·7)	−80	140·8	1314	173,871
							Series	*No. V.*										
Mar. 20, '96	30	10.13	1200	17,782	134,073,435	959·88	178·204	171,054	...	...	...	−0·2	−12	−1·3	−16	145·8	1360	172,386
Mar. 20, '96	31	11.39	1200	17,855	134,623,843	959·29	179·083	171,793	...	...	...	−0·2	−12	−11·7	−140	145·4	1357	172,998
Mar. 20, '96	32	2.15	1200	17,939	135,257,190	968·3	178·269	172,618	...	...	...	...	...	−10·3	−124	141·4	1319	173,813
							Series	*No. VI.*										
Mar. 25, '96	35	11.1	1200	17,871	134,744,481	962·18	178·756	171,995	...	...	...	0·1	6	−9·0	−108	144·9	1352	173,245
Mar. 25, '96	36	2.7	1200	17,998	135,702,040	967·85	178·98	173,226	...	...	...	−0·1	−6	−9·0	−108	143·4	1338	174,450
							Series	*No. VII.*										
Mar. 27, '96	37	10.27	1200	17,881	134,819,879	963·21	178·631	172,059	...	...	...	−0·1	−6	−0·3	−4	145·9	1361	173,410
Mar. 27, '96	38	11.44	1200	17,925	135,151,632	965·05	178·799	172,550	...	...	...	−0·3	−17	−4·4	−53	144·3	1346	173,826
Mar. 27, '96	39	2.28	1200	17,891	134,805,277	961·57	179·073	172,190	...	...	60	...	...	−5·9	−71	144·8	1351	173,530

Date	Trial No.	Time of start	Moment (ft.-lbs.)	No. of revolutions of engine-shaft	Work done (ft.-lbs.)	Weight of water discharged by the brake (lbs.)	Rise of temperature in the brake (° F.)	Heat generated, less losses by radiation, &c. (B.T.U.)	Weight of water caught at stuffing-box (lbs.)	Rise of temperature in the brake (° F.)	Loss of heat by leakage (B.T.U.)	Rise of temperature of brake during trial (° F.)	Terminal correction to heat (B.T.U.)	Fall of temperature along shaft between stuffing-box and bearing (° F.)	Loss of heat by conduction (B.T.U.)	Difference of temperature between brake and air (° F.)	Loss of heat by radiation (B.T.U.)	Corrected heat (B.T.U.)
								Series No. I.										
Feb. 5, '96	2	3.4	600	18,120	68,310,950	426·15	178·787	82,626	...	...	...	...	...	(−10·3)	−124	137·4	5065	87,567
Feb. 13, '96	7	10.12	600	18,018	67,926,419	459·03	179·578	82,432	...	...	...	−0·9	−91	(−10·3)	−124	140·8	5190	87,407
Feb. 13, '96	8	11.30	600	18,063	68,096,065	461·58	179·221	82,725	...	...	...	−0·5	−29	(−10·3)	−124	139·1	5127	87,699
Feb. 19, '96	11	2.30	600	18,060	68,084,755	459·66	179·475	82,497	...	...	...	−0·2	−11	(−10·3)	−124	141·1	5201	87,563
								Series No. II.										
Feb. 28, '96	14	3.36	600	18,020	67,933,958	480·87	178·954	86,054	...	...	...	−0·1	− 5	(−18·2)	−218	142	1325	87,156
Mar. 5, '96	15	10.34	600	17,947	67,658,754	479·29	178·883	85,737	...	...	...	−0·4	−22	(− 8·9)	−107	140·8	1314	86,922
Mar. 5, '96	16	11.50	600	17,952	67,677,604	479·3	179·05	85,819	...	...	...	1·0	55	(−17·9)	−215	140	1306	86,965
								Series No. III.										
Mar. 12, '96	21	10.33	600	17,661	66,580,557	469·17	179·408	84,173	...	...	...	−0·1	− 5	− 2·6	− 31	145·3	1356	85,493
Mar. 12, '96	22	11.48	600	17,810	67,142,275	474·84	179·034	85,012	...	...	...	−0·7	−38	− 7·9	− 95	144·3	1346	86,225
Mar. 12, '96	23	2.38	600	17,710	66,765,283	473·33	178·951	84,703	...	...	...	−1·0	−55	− 7·7	− 92	144·4	1347	85,903
								Series No. V.										
Mar. 19, '96	27	10.22	600	17,866	67,353,391	476·05	179·275	85,344	...	...	...	−0·1	− 5	−11·1	−133	149·4	1394	86,600
Mar. 19, '96	28	11.36	600	17,811	67,146,045	474·95	179·276	85,147	...	...	...	0·1	5	−18·3	−220	144·2	1345	86,277
Mar. 19, '96	29	2.19	600	17,856	67,315,692	477·32	178·928	85,406	...	...	...	−0·3	−16	−13·7	−164	140·4	1310	86,536
								Series No. VI.										
Mar. 23, '96	33	11.38	600	17,956	67,692,684	478·3	179·226	85,724	...	...	...	−1	−55	− 5·1	− 61	145·7	1359	86,967
Mar. 23, '96	34	2.27	600	17,710	66,765,283	472·18	179·221	84,625	...	...	...	...	...	− 5	− 60	144·2	1345	85,910
								Series No. VII.										
Mar. 30, '96	40	10.49	600	17,959	67,703,993	478·23	178·899	85,555	...	...	...	−0·5	−27	11·6	139	146·5	1367	87,034
Mar. 30, '96	41	11.59	600	17,802	67,112,116	474·96	179·246	85,135	...	...	...	−0·3	−16	− 1·7	− 20	143	1334	86,433
Mar. 30, '96	42	3.29	600	17,807	67,130,965	477·11	178·772	85,294	...	...	22	−0·4	−21	−16·6	−199	143·1	1335	86,431

TABLE D.

Determination No.	Trial No.	Work	Difference of Work	Heat (corrected)	Difference of Heat	K
			Series No. I.			
I.	1	134,201,612	...	172,366		
	2	68,310,950	65,890,662	87,567	84,799	777·02
III.	6	135,935,775	...	174,818		
	7	67,926,419	68,009,356	87,407	87,411	778·04
IV.	9	136,063,953	...	174,998		
	8	68,096,065	67,967,888	87,699	87,299	778·56
V.	10	136,674,680	...	175,772		
	11	68,084,755	68,589,925	87,563	88,209	777·58
			Mean value = 777·81.			
			Series No. II.			
VI.	13	135,392,907	...	173,973		
	14	67,933,958	67,458,949	87,156	86,817	777·02
VII.	17	135,098,853	...	173,591		
	16	67,677,604	67,421,249	86,965	86,626	778·3
			Mean value = 777·66.			
			Series No. III.			
VIII.	18	133,734,142	...	171,843		
	21	66,580,557	67,153,585	85,493	86,350	777·69
IX.	19	133,892,479	...	172,174		
	22	67,142,275	66,750,204	86,225	85,949	776·63
X.	20	135,332,588	...	173,871		
	23	66,765,283	68,567,305	85,903	87,968	779·46
			Mean value = 777·94.			
			Series No. V.			
XI.	30	134,073,435	...	172,386		
	27	67,353,391	66,720,044	86,600	85,786	777·75
XII.	31	134,623,843	...	172,998		
	28	67,146,045	67,477,798	86,277	86,721	778·1
XIII.	32	135,257,190	...	173,813		
	29	67,315,692	67,941,498	86,536	87,277	778·46
			Mean value = 778·1.			
			Series No. VI.			
XIV.	35	134,744,481	...	173,245		
	33	67,692,684	67,051,797	86,967	86,278	777·16
XV.	36	135,702,040	...	174,450		
	34	66,765,283	68,936,757	85,910	88,540	778·59
			Mean value = 777·89.			
			Series No. VII.			
XVI.	37	134,819,879	...	173,410		
	40	67,703,993	67,115,886	87,034	86,376	777·02
XVII.	38	135,151,632	...	173,826		
	41	67,112,116	68,039,516	86,433	87,393	778·55
XVIII.	39	134,895,277	...	173,530		
	42	67,130,965	67,764,312	86,431	87,099	778·01
			Mean value = 777·86.			

TABLE E.

Date	Trial No.	Time of start	Moment (ft.-lbs.)	No. of revolutions of engine-shaft	Work done (ft.-lbs.)	Weight of water discharged by the brake (lbs.)	Rise of temperature in the brake (° F.)	Heat generated, less losses by radiation, &c. (B.T.U.)	Weight of water caught at stuffing-box (lbs.)	Rise of temperature in the brake (° F.)	Loss of heat by leakage (B.T.U.)	Rise of temperature of brake during trial (° F.)	Terminal correction to heat (B.T.U.)	Fall of temperature along shaft between stuffing-box and bearing (° F.)	Loss of heat by conduction (B.T.U.)	Difference of temperature between brake and air (° F.)	Loss of heat by radiation (B.T.U.)	Corrected heat (B.T.U.)
							Series No. VIII.											
Apr. 1, '96	46	10.23	1200	17,997	135,688,500	967·92	179·102	173,356	...	...	...	−0·5	−29	−3·4	−41	146	1191	174,477
Apr. 1, '96	47	11.39	1200	17,990	135,641,722	969·39	178·73	173,259	...	...	...	−0·3	−17	−5·9	−71	143·8	1173	174,344
							Series No. IX.											
Apr. 17, '96	48	10.43	1200	17,735	133,719,062	955·6	178·505	170,579	...	...	...	...	...	−5·3	−64	143·5	1171	171,686
Apr. 17, '96	49	12.00	1200	18,033	135,965,935	974·92	178·162	173,694	...	...	...	...	...	−7·0	−84	142·5	1163	174,773
Apr. 17, '96	50	2.17	1200	17,601	132,708,724	950·31	178·352	169,490	...	...	...	...	...	−9·6	−115	141·8	1157	170,532
							Series No. VIII.											
Mar. 31, '96	43	10.30	400	17,958	45,133,482	317·85	179·188	56,955	...	...	...	−0·3	−16	−2·9	−35	145·7	1189	58,093
Mar. 31, '96	44	11.45	400	18,005	45,251,606	318·84	179·158	57,123	...	...	...	+0·1	5	−5·6	−67	143·4	1170	58,231
Mar. 31, '96	45	2.30	400	17,704	44,495,109	311·81	179·495	55,968	...	...	...	0·8	43	−7·4	−89	145·4	1186	57,108
							Series No. IX.											
Apr. 20, '96	51	11.12	400	18,009	45,261,660	318·46	178·777	56,933	...	...	...	−0·1	−5	−6·1	−73	145	1183	58,038
Apr. 20, '96	52	12.20	400	17,819	44,784,136	316·04	178·686	56,472	...	...	...	0·3	16	−9·1	−109	142·1	1160	57,539
Apr. 20, '96	53	2.39	400	17,919	45,035,464	317·84	178·926	56,870	...	...	...	−0·3	−16	−8·1	−97	142·4	1162	57,919

TABLE F.

Determination No.	Trial No.	Work	Difference of Work	Heat (corrected)	Difference of Heat	K
			Series No. VIII.			
XIX.	46	135,688,500	...	174,477		
	43	45,133,482	90,555,018	58,093	116,384	778·07
XX.	47	135,641,722	...	174,344		
	44	45,251,606	90,390,116	58,231	116,113	778·47
			Mean value = 778·27.			
			Series No. IX.			
XXI.	48	133,719,062	...	171,686		
	51	45,261,660	88,457,402	58,038	113,648	778·35
XXII.	49	135,965,935	...	174,773		
	52	44,784,136	91,181,799	57,539	117,234	777·78
XXIII.	50	132,708,724	...	170,532		
	53	45,035,464	87,673,260	57,919	112,613	778·54
			Mean value = 778·22.			

TABLE G.

Date	Trial No.	Time of start	Moment (ft.-lbs.)	No. of revolutions of engine-shaft	Work done (ft.-lbs.)	Weight of water discharged by the brake (lbs.)	Rise of temperature in the brake (° F.)	Heat generated, less losses by radiation, &c. (B.T.U.)	Weight of water caught at stuffing-box (lbs.)	Rise of temperature in the brake (° F.)	Loss of heat by leakage (B.T.U.)	Rise of temperature of brake during trial (° F.)	Terminal correction to heat (B.T.U.)	Fall of temperature along shaft between stuffing-box and bearing (° F.)	Loss of heat by conduction (B.T.U.)	Difference of temperature between brake and air (° F.)	Loss of heat by radiation (B.T.U.)	Corrected heat (B.T.U.)
							Series No. X.											
July 7, '96	69	11.17	600	17,548	66,154,556	468·88	178·972	83,916	...	...	...	...	...	−0·57	−7	135·1	1078	84,987
July 7, '96	70	12.24	600	17,807	67,130,965	474·78	179·474	85,211	...	...	...	...	...	−0·86	−10	135·0	1077	86,278
July 7, '96	71	1.41	600	18,095	68,216,702	482·62	179·608	86,682	...	...	...	...	...	0·57	7	135·6	1082	87,757
							Series No. XI.											
July 10, '96	78	10.45	600	17,436	65,732,325	464·59	179·393	83,344	...	...	...	...	...	−0·71	−9	136·5	1089	84,424
July 10, '96	79	11.55	600	17,602	66,358,132	470·46	179·269	84,339	...	...	...	−0·4	−22	−4·43	−53	134·3	1072	85,336
July 10, '96	80	1.11	600	17,894	67,458,948	477·08	179·597	85,682	...	...	...	0·3	16	1·14	14	135·1	1078	86,790
							Series No. X.											
July 8, '96	72	11.11	1200	17,311	130,522,170	934·58	178·344	166,677	...	...	...	−0·8	−46	−0·14	−2	137·7	1099	167,728
July 8, '96	73	12.29	1200	17,528	132,158,316	950·07	177·719	168,845	...	...	...	1·1	63	−0·43	−5	135·0	1077	169,980
July 8, '96	74	1.43	1200	17,737	133,734,142	956·46	178·559	170,785	0·31	82·3	26	0·3	17	1·0	12	135·5	1081	171,921
							Series No. XI.											
July 9, '96	75	10.35	1200	17,529	132,165,855	945·95	178·39	168,748	...	...	...	0·6	35	0·86	10	134·1	1070	169,863
July 9, '96	76	11.50	1200	17,858	134,646,463	969·82	177·47	172,114	...	...	...	−0·6	−35	−1·71	−20	131·2	1047	173,106
July 9, '96	77	1.6	1200	17,954	135,370,287	974·37	177·566	173,015	...	...	...	0·4	23	−0·57	−7	130·3	1040	174,071

TABLE H.

Determination No.	Trial No.	Work	Difference of Work	Heat (corrected)	Difference of Heat	K
			Series No. X.			
XXIV.	72	130,522,170	...	167,728		
	69	66,154,556	64,367,614	84,987	82,741	777·95
XXV.	73	132,158,316	...	169,980		
	70	67,130,965	65,027,351	86,278	83,702	776·89
XXVI.	74	133,734,142	...	171,921		
	71	68,216,702	65,517,440	87,757	84,164	778·44
			Mean value = 777·74.			
			Series No. XI.			
XXVII.	75	132,165,855	...	169,863		
	78	65,732,325	66,433,530	84,424	85,439	777·56
XXVIII.	76	134,646,463	...	173,106		
	79	66,358,132	68,288,331	85,336	87,770	778·03
XXIX.	77	135,370,287	...	174,071		
	80	67,458,948	67,911,339	86,790	87,281	778·07
			Mean value = 777·88.			

DESCRIPTION OF THE PLATES.

(*See end of Volume.*)

PLATE 1.

From a photograph in 1888. Is a front view of the triple expansion engines (100 H.-P.) and brakes, as they existed in the engineering laboratory, Owens College, before any modifications for the determination of the equivalent. The engine-shafts are disconnected from each other, and are working on three separate brakes. In the trials the three large pulleys (5 feet in diameter) were removed with the brakes on the high-pressure and intermediate engines, and the engine-shafts coupled by intermediate shafts, the work being all absorbed by the brake on the low-pressure engine—seen, on the right hand of the plate, overhanging the last bearing of the brake-shaft. On this shaft are two heavy 3-feet pulleys, which served as fly-wheels during the trials.

It was the facilities afforded by this brake and its appurtenances (§ 11) that suggested the research and rendered it possible: and, although the method of admitting the water and air to the brake was necessarily modified in the experiments, the brake remained essentially the same. Part of the trials was made with the brake uncovered, as seen in this plate; and it was after the brake was covered that the subsequent photographs were taken.

The vertical pipe supplying the town's water from the service tank to the brake, with the hand-cock and the automatic inlet-cock above, leading through the bowed pipe and flexible indiarubber tube to the inlet passage over the bush of the brake, are seen on the immediate right. Immediately on the left and a little behind and lower, is another bowed pipe leading from the top of the brake, with a gap in it; this is the air passage leading through the vanes to the centres of the vortex chambers, to secure atmospheric pressure there. The suspended and riding loads on the lever, the dash-pot, the front stop on which the lever rests (not being at work), are also seen. The hand wheel for adjusting the height of the lever when at work, the linkage connecting the automatic inlet and outlet-cocks with each other and with the front stop, together with the outlet-cock, the receptacle for waste, and the drip-can for the water escaping from the front bush, can be traced, though they are obscure in this plate.

Up high on the photograph is seen a shaft with two large pulleys; these are for connecting the separate engine-shafts by belts and rope (seen), and have no place in the trials. But the bright shaft immediately below, seen as driven by a rope pulley from behind the wall of the engine-room, is the line shaft driven by the separate engine, always running, which afforded most important facilities for the research.

PLATE 2.

From a photograph, 1896. Also shows a front view of the engine-room, but taken more to the right; it includes only the low-pressure engine. It shows a general front view of the appliances in the condition in which they were during the final experiments, as well as some of the standing appliances not included in Plate 3.

Low down, immediately on the right, is the front of the weighing-machine, with the tank resting on it; and immediately behind this, against the wall, are seen the mercury balances for the pressures of water in the mains; also the town's main to the service tank (out of sight on the right), in front of which is the 3-inch quadruple turbine which drives the ($1\frac{1}{2}$-inch) quintuple centrifugal pump (out of view, behind the tank) supplying the brake through the ice-cooler (§ 20). On the left of the tank, and passing through its cover, is the water-switch; and over this is the nozzle of a vertical pipe, straight almost to the roof, then horizontal, with an open vertical branch, to form an air-gap, then down again into the lower of the two horizontal pipes; this is the stand-pipe on the outlet from the condenser, for securing pressure in the final thermometer chamber (§ 22). The upper of the two horizontal pipes is the water-jacketed outflow pipe or "condenser," which passes to the end of the room, and returns as the lower horizontal pipe to the stand-pipe. Immediately on the left of the plate, standing on the floor, is the frame for the hand-brake (§ 30). Besides the appliances mentioned, as seen, in this plate, nearly all the appliances are seen in front view; but many are better seen in the following plates, though this plate affords the best view of the general arrangement, and the best idea of the circumstances under which the observations were made. The passage between the brake and the 3-inch pipe supplying condensing water to the engine afforded the only post of observation for the counter, thermometers, speed-gauge, and pressure-gauges. The centrifugal speed-gauge, with its scale, is seen rising vertically from behind the small pressure-gauge on the brake.

PLATE 3.

This is a nearer and simplified front view of the more special appliances shown in Plate 4. Proceeding from the right is the switch and outlet nozzle from the condenser, with the water flowing into the tank over the thermometer. From the switch may be traced the linkage forming the automatic connection of the switch with the counter, immediately in front of the covered bush of the brake. Supported by the original supply pipe to the brake (the hand-cock being shut) is seen the new inlet pipe from the ice-cooler, behind the brake. The pipe, rising on the right from behind the brake, passes a branch to the by-channels leading to the bushes (not seen) and a branch to the large pressure-gauge, then to the regulator; thence the water flows upwards past the bulb of the inlet thermometer, some of it passing up through the glass thermometer chamber, and so to waste through the small pipe at the top, but the main stream passing through the covered horizontal branch, and down the flexible indiarubber pipe into the brake. On the top of the brake is seen the new air-passage, of flexible indiarubber, leading to the vessel in which is the artificial atmosphere, which is connected with the large mercury-gauge on the left, also with the syringe. The automatic outflow cock is clearly seen under the brake, also the curved flexible pipe, covered with cotton-wool, which receives the water from the outflow cock, leading to the fixed pipe behind the regulator, also covered, in which is the bulb of the outflow thermometer, and immediately over this the glass thermometer chamber, with its indiarubber continuation leading back into the main outflow channel which rises up behind the inlet thermometer chamber, till it turns at right angles into the condenser. Behind and on the left of the brake are seen protruding the stems of the thermometers for measuring the difference of temperature in the stuffing-box and the near bearing. Of the two bottles standing on the floor, that on the left is collecting the leakage from the stuffing-box, and the other the leakage caught in the indiarubber bag enclosing the automatic outflow cock.

PLATE 4.

This is a back view. On the left, close in front of the tank on the weighing-machine, over which is the condenser leading to the switch, is seen the $1\frac{1}{2}$-inch quintuple centrifugal pump, with its driving gear and the pipe supplying it from the service tank. On the other side of the 3-inch pipe for condensing water for the engines, and partly behind it, is seen the pipe leading from the pump up and along behind the 3-inch pipe, then down again into the ice-tank (on the extreme right of the plate); through this it passes in a coil, emerging from the cover again as the covered pipe rising obliquely to the regulator and inlet thermometer chambers (not seen), with the branch to the pressure-gauge. The small horizontal branch coming through from beneath the pressure-gauge, continued by the covered indiarubber pipe, passing behind the vortex vessel of the speed-gauge to the stuffing-box, is one of the by-paths taking ice-cold water to the bushes; that on the left is behind the brake. The outlet thermometer chamber, with its indiarubber continuation to the main outflow channel into the condenser, is also clear; as are also the belt and pulley driving the paddle in the ice-tank.

PLATE 5.

This is again a back view, but taken so as to show the appliances up to the end of the engine-room, not seen in the previous plates. In the middle front is seen the 6-inch quadruple centrifugal pump in circuit, with the rising 4-inch main from the lower tank to the tank in the tower (§ 3), together with the belt from the line shaft by which this pump is driven. Immediately on the left of this plate, standing on a bench, is the end of the 3-inch quadruple vortex turbine, driven by water from the tower, and driving by a cord the $1\frac{1}{2}$-inch quintuple centrifugal pump. The standard, the lever, and the large riding weight of the weighing-machine, with the tank behind, are completely in view; and over these again appears the condenser for cooling the effluent water, passing to the end of the room and returning underneath to the stand-pipe and thence to the switch.

PLATE 6.

This is from a photograph of the apparatus for correcting the high temperature thermometer. On the table is the barometer, and to the right is the vapour chamber, in which the thermometer is immersed through the cork on the top as far as to leave the top of the mercury visible. The escape passage and regulator are seen on the right. The pipe leading from the top is the connection of the vapour chamber with the lower mercury chamber in the barometer. This, after passing through the flask, receives by the branch (seen) a slight current of air from the pressure reservoir, with the top of which it is connected by a restricted pipe, so that the current is so slow that the resistance is negligible, though sufficient to prevent the vapour passing to the barometer; the pressure of air in the reservoir is shown by the large mercury-gauge, and is maintained by occasional pumping with the syringe seen in connection. The nozzle on the barometer, to which the air-passage is connected, leads into the cast-iron bottle which forms the mercury chamber, above the surface of the mercury. The level of this surface is observed through the circular windows, of which that which is in front is shown to the left of the axis of the barometer, above the nozzle. Immediately above this window is seen the cylindrical brass curtain, which screws on to the neck of the bottle, by which the light through the windows over the mercury can be eclipsed. Attached to this curtain, and co-axial with it, is the outer brass tube extending up to the gap, with a vertical scale attached reaching past the gap. Behind the vertical scale, and screwed into the tube on the lower curtain, is a tube screwed throughout its length, and having two parallel slots, as windows, some 5 inches long, through which the upper limb of the mercury may be observed. From the top of this windowed tube downward is screwed the cap, the lower limb of which forms a cylindrical curtain for eclipsing the light over the upper limb of the mercury (§ 48).

67.

ON THE SLIPPERINESS OF ICE.

[*From the Forty-third Volume of the* "Memoirs and Proceedings of the Manchester Literary and Philosophical Society." *Session* 1898—9.]

(*Received and read* February 7th, 1899.)

THE slipperiness of ice is, and has been, one of the most noticeable, interesting, and important circumstances under which we live, as well as one of the commonest. Ice is not the only slippery thing in the world, but it is the only one of all the solid substances which, in the condition nature has left them on the surface of the earth, possesses the property of perfect slipperiness. This being so, and being commonly known to be so, it is certainly remarkable that, whatever may be the reason, there appears to have been little or no curiosity as to the physical significance of the unique property which ice possesses. Speaking for myself this is simply explained: ice was slippery when I was born, I never knew it otherwise, and, to put it shortly, it was slippery because it was ice, whereas it now seems to me that, of all the secrets nature has concealed by her method of deadening curiosity by leaving them exposed, in this her method has been the most successful.

The cause of my ultimately discovering the secret, unsought by me, was an accident, though brought about by another line of research. The other sources of perfect slipperiness are complex; a smooth solid surface covered by a viscous fluid, as a well-greased board, is perfectly slippery just as ice is, which fact had been taken for granted much in the same way as the slipperiness of ice, neither more nor less.

That surfaces of machines would not slip over each other without grease was well known and followed out, but the physical significance of the action was apparently not questioned until, in 1884, Mr Beauchamp Tower[1],

[1] *Proc. Inst. M. E.*, Nov. 1883 and Jan. 1884.

while making experiments as to the resistance of a railway journal, accidentally came across a fact of very striking significance.

In this experiment, instead of using an axle, Mr Tower used an overhanging shaft driven by a steam-engine, the shaft being supported on bearings in the usual manner. The overhanging portion of the shaft was turned to the same shape as one of the journals of a railway wheel, four inches in diameter and six inches long. On this journal the ordinary axle-box was suspended, the load to correspond with the proportion of the weight of a loaded truck being suspended from the axle-box underneath the shaft. The axle-box had the usual brass wearing-piece, and the provision for lubrication was, as usual, an oil or grease cup communicating through a vertical oil-hole, so that the oil might descend by gravitation through the brass on to the surface of the journal, and thence escape, after being used, to the ground. This was in the first instance, but, after experimenting in this way, Mr Tower proceeded to find what would be the effect on the resistance if, instead of allowing the oil or grease to escape freely from underneath the journal, the whole under side of the journal was encased in a vessel, so as to form a bath of oil in which the journal would be completely covered.

In commencing these experiments with the bath, Mr Tower noticed with surprise that, although the oil in the bath did not cover the top of the brass when the journal was at rest, when in motion the oil escaped upward against gravity through the oil-hole, and as this was inconvenient, tending to empty the bath, he drove a plug of wood into the hole and tried again, when to his still greater surprise he found that the oil forced out the wooden plug. This led him to fit a pressure gauge to the hole; this immediately rose to the top of its scale, 200 lbs. per square inch. Then, realising that he had before him evidence of an action in lubrication until then unsuspected, Mr Tower turned his attention to its experimental investigation, finding that when the journal was run at 400 revolutions a minute, the pressure on the square inch indicated on the gauge was somewhere about 3/2 of the pressure necessary, if distributed over the whole horizontal area of the section of the bearing, to sustain the load. The pressure in the oil-hole would be 600 lbs. per square inch when the total load was 9,600 lbs., whence, as the area of the horizontal section was 24 square inches, the mean intensity of pressure would be 400 lbs. This, however, was only when the speed of the journal was greater than a certain limit depending on the load; when the speed diminished below this limit, the pressure on the gauge fell to any degree below that necessary to sustain the load. But this was not all. When the speed was such as to sustain the load, the friction was 1 in 400, but when running slow the friction reached 1 in 3, or the journal seized the brass.

Taking these two things together, it made clear the fact which had never been surmised before, that the *action of lubrication consisted in the actual separation of the solid surfaces* by a film of fluid of finite thickness.

These discoveries of Mr Tower excited great interest at the time, and, being myself occupied in the study of fluid motion, I was induced to undertake the theoretical analysis of Mr Tower's experimental results, from which, after two years' work, I was able to publish a complete theory of lubrication[1], showing that not only in the case of the oil-bath, when the thickness of the separating film of oil was about 2/1,000th of an inch, but in cases of ordinary lubrication where the thickness of the film is less than ·0001 of an inch, the surfaces are separated by a complete film.

This is very strikingly indicated by a rarely shown but simple experiment. Two cylindrical hard steel gauges, male and female, one inch in diameter, made to gauge to within 1/20,000th of an inch will not pass one into the other, if wiped as clean as possible of all oil, without the use of great pressure or of a mallet. If oiled and kept moving they can be easily passed one into the other. But should the motion be arrested for a second, they seize and can only be separated by the mallet, which shows that a film of oil less than the 1/20,000th of an inch is sufficient to sustain perfect slipperiness, while the least contact destroys this property.

My research also led to the recognition that the property on which the lubricating action depends is the viscosity of the fluid, and that all fluids are lubricants, provided they are not corrosive. Air lubricates, as is shown by the floating of one true surface plate on another with perfect slipperiness. Now water had, at the time, not been recognised as a lubricant; its viscosity is from 200 to 400 times less than oil, but from my research it appeared that it is a lubricant in proportion to its viscosity.

All this is now matter of history, and its bearing on the slipperiness of ice may not as yet be clear. But it has a fundamental bearing nevertheless.

It was about 1886, while I had this subject of lubrication very fresh in my mind, that I was, for some reason, using a common soldering-iron, and was in the act of testing the copper point of the hot iron to see if it was hot enough to melt the solder, when, from some cause or another, instead of merely touching the block gently with the point of the copper, I must have pushed the sloping edge obliquely and somewhat roughly on to the flat top of the block, for, to my surprise, instead of melting a little pock in the surface, the square-edged side of the copper slipped without friction right along the face of the solder. It was a perfectly casual accident, but, under the circumstances, it caused me a sense of mental shock, as I instantly recognised the analogy to the *skate*.

The barely hot enough, parallel sharp edge of the copper, pressed and pushed forward on the block, was just able to melt the immediate surface, which completely lubricated the iron on the solder beneath. The then well-known property of the lowering of the melting point of ice under pressure at

[1] *Phil. Trans.* 1886, Part I., pp. 157—234, p. 228 in this volume.

once presented itself; the shock was the result of the instantaneous reflection that I had never before thought of considering why ice was slippery.

On trying to remember whether I had ever heard of any attempt to explain the slipperiness of ice in any way—for I felt at the moment as though everyone was laughing at me—I found that I could not recall any mention of the subject. And then, in self-extenuation, I reflected that water was not recognised as a lubricant, so that even James Thomson himself, or his brother, Lord Kelvin, might have failed to realize that the melting of the ice under the pressure of the skate would lubricate the moving skate, and rendered the ice slippery to any hard body pressed against it. I also reflected, that had not my mind been full of the circumstances of lubrication, including the lubricating properties of all fluids, I should not have recognised in the slipping of the hot iron the action of the lubricant, and that, even if I had, I should not have attributed like properties to melted ice.

Of course, this evidence as to the cause of the slipperiness was altogether one-sided, and it was still open for ice to have other properties which would account for the slipping besides the property of melting under pressure, and it was at once plain that to render the evidence complete it was necessary to show that, under circumstances of temperature and pressure such that the pressure was nowhere sufficient to melt the ice, the property of perfect slipperiness of ice did not exist.

Looking carefully into the matter from the theoretical side, with Lord Kelvin's determination of the laws of the melting point, 0·014° F. for each additional atmosphere, it appeared that taking a weight of 140 lbs., and an area of 1·4/10 (= 1/7) square inch, a man skating would melt ice at 31° F. with a skate-bearing of 1·4/10 square inch, while to melt ice at a temperature of 22° F. the bearing must be reduced 1·4/100 (= 1/70) square inch. That is, the ice at 22° F. would have to be able to sustain a pressure up to 10,000 lbs. on the square inch. That ice should stand such pressure at first sight seems unlikely, but then our general impression as to the hardness of ice is derived from ice at or near its melting point.

That this theory admits of experimental verification is certain, but such experiments only become possible when the general surroundings are at a temperature of 22° F.

It was this consideration which caused me, in the first instance, to delay any publication of the facts I observed until there came a frost sufficient for my purpose. There have been frosts of sufficient extent when my preparations were not ready, and my preparations have been ready when there were no frosts; until, at last, my patience has given way and I have determined to wait no longer. In taking this decision, however, I have been greatly influenced by my general observations on the effect of the temperature on the ease of skating, and on the liability to slip. I notice that without great care you cannot walk on ice at $31\frac{1}{2}°$ in leather boots without nails, whereas you can walk safely with boots and somewhat blunt nails under the same circumstances; with a temperature of 27° you can walk with leather boots almost as safely as on any polished floor, while with somewhat blunt nails it is very unsafe to walk on uneven ice.

On ice near 32° skaters find no resistance however slowly they may move, while on hard ice it is necessary to move quickly, or the skates seize, showing that the ice melts under the edge, but owing to the small area of the lubricating surface, the lubricant is squeezed out rapidly, thus destroying the lubrication below certain speeds, as in Mr Tower's experiment.

But the circumstance that has most confirmed me in the view that the slipperiness of ice is due to the lubrication afforded by the melting under pressure is a casual but emphatic statement made by Dr Nansen, in his book on Greenland, that at the low temperatures he there encountered the ice completely lost its slipperiness.

INDEX.

Analyzers, harmonic, 519

Balancing of machines, 17
Boiling of water at ordinary temperatures, 578
Boundary conditions of fluid motion, 132
Brake, hydraulic, 353

Colour bands, use of, in fluid motion, 524
Criterion of steady motion, Theory of, 561
Critical velocity of water, 51, 535
Currents, action on beds of rivers and estuaries, 326, 380, 410, 482

Diagram, indicator, 163, 368
Dilatancy of media, 203, 217
Dissipation function, 544
Dryness of steam, 591
Dynamic similarity, 321, 380, 410
Dynamics of oscillations, 8, 25, 35, 41
Dynamo, stresses in, 28, 44

Eddies in water, 51, 153
Energy, directed and undirected, 138
„ storage of, 39
„ transmission of, 106
Engine trials, 336
Equations of motion of viscous fluids, 132, 258, 544
Errors of steam-engine indicator, 163
Estuaries, *régime* of, 326, 380, 410, 482

Flow of gases, 311
Fluid motion, dynamical theory of, 535
„ use of colour bands in, 51, 158, 524

Friction of lubricated bearings, 228
„ in reciprocatory motion, 41

Gases, flow of, 311
Gravitation, possible explanation of, 203, 217

Harmonic analyzers, 519
„ motion, 25
Heat, mechanical equivalent of, 601
Hydrodynamics, equations of, 132, 258
Hydraulic brake, 353

Ice, slipperiness of, 734
Indicator, errors of steam-engine, 163
„ diagrams, to combine, 368
Inertia, forces due to, 1, 28
Isochronous vibration, 25

Lifeboats, qualities of, 321
Limits to speed, 1
Lubrication, theory of, 228

Mechanical equivalent of heat, 601
Media, dilatancy of, 203, 217
Model estuaries, 326, 380, 410, 482
Motion of water, two manners of, 51, 153, 524

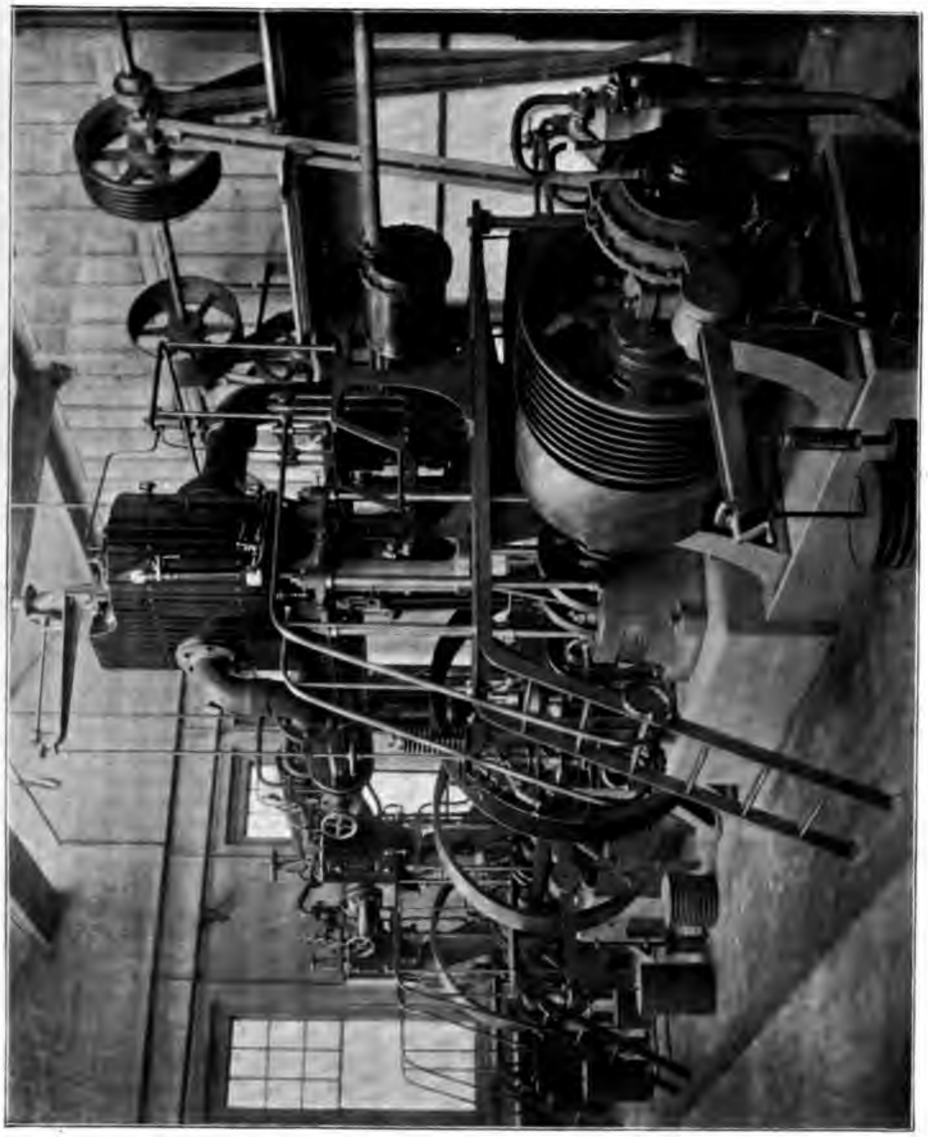
Reservoirs of energy, 39
Resistance of water, law of, 51
Rivers and estuaries, *régime* of, 326, 380, 410, 482

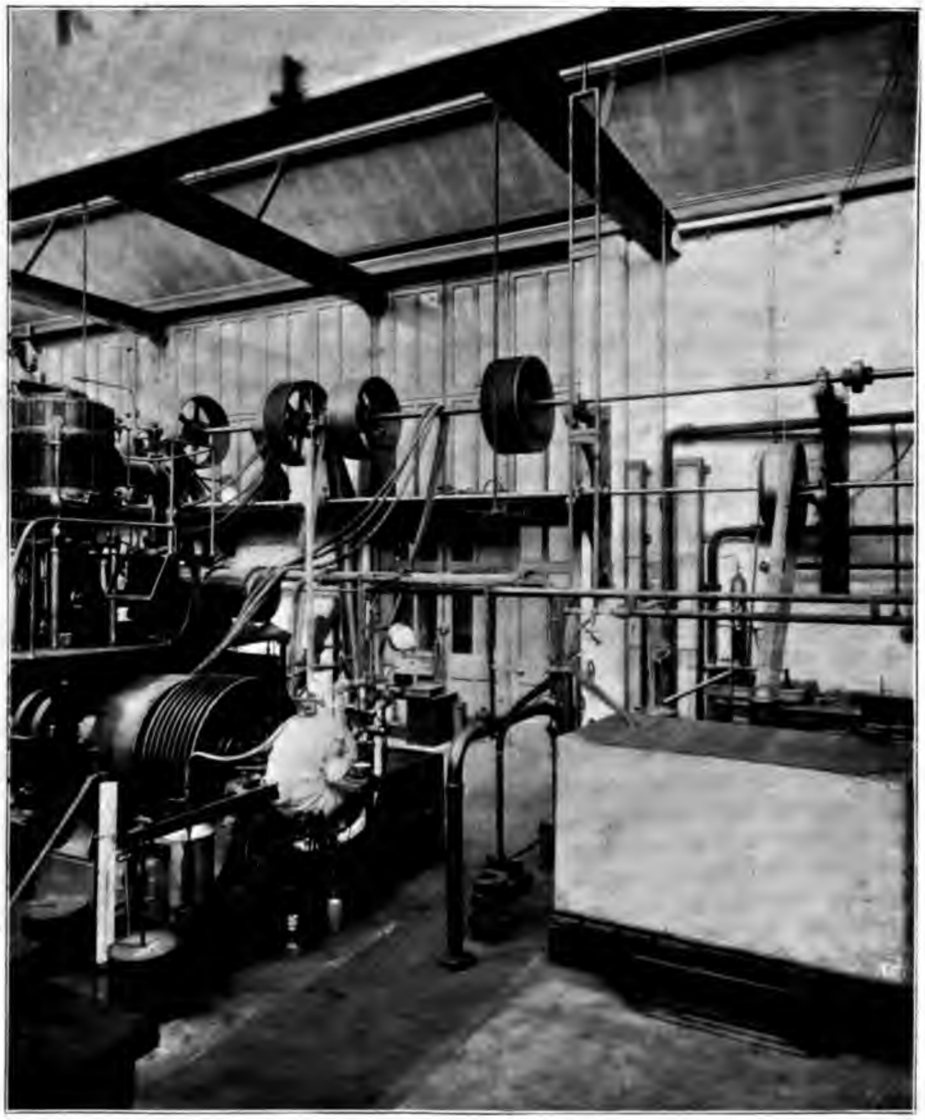
Saturated steam, dryness of, 591
Speed, limits to, 1
Stability of motion in water, 51

Tides, action on beds of rivers and
estuaries, 326, 380, 410, 482
Transmission of energy, 106
Waves, action on beds of r
aries, 326, 380, 410, 482

END OF VOLUME II.

Plate 1





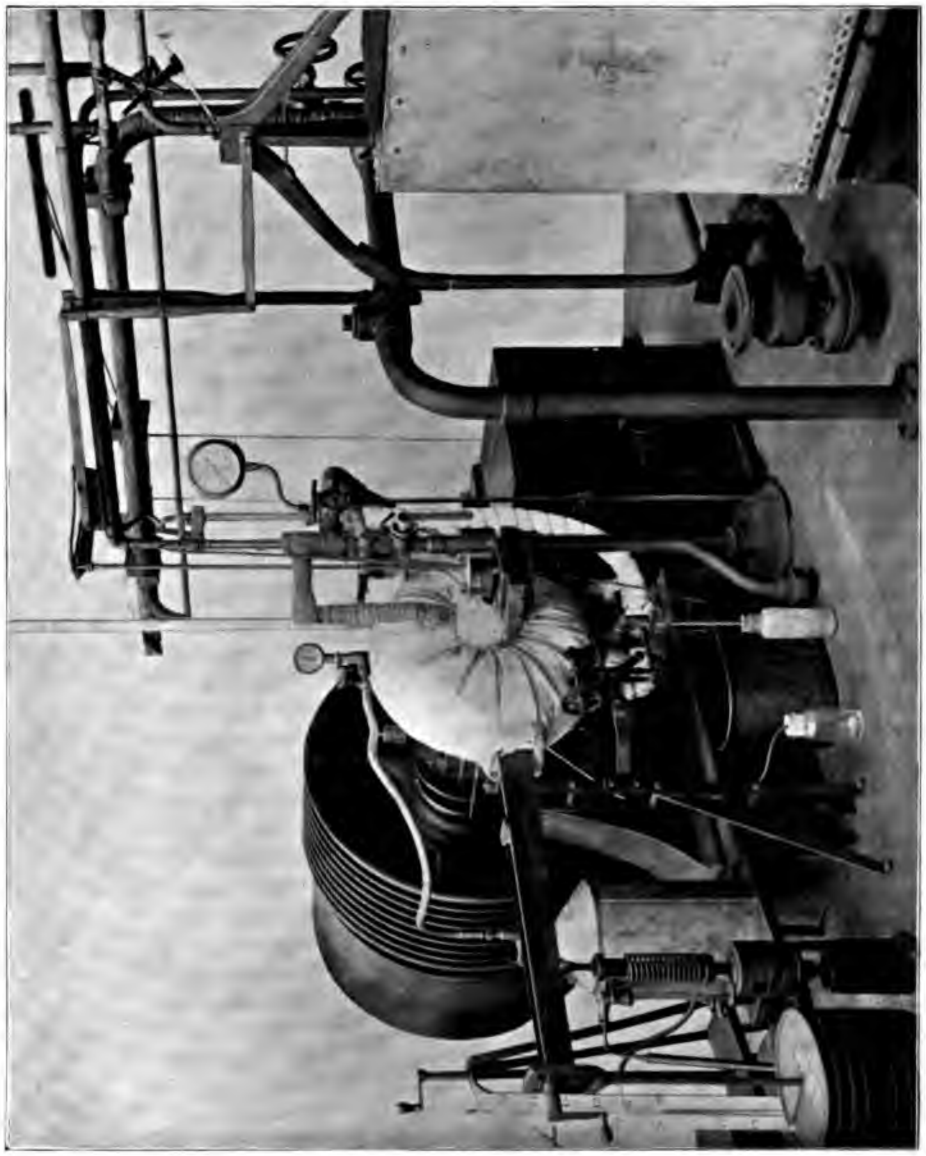


Plate 4

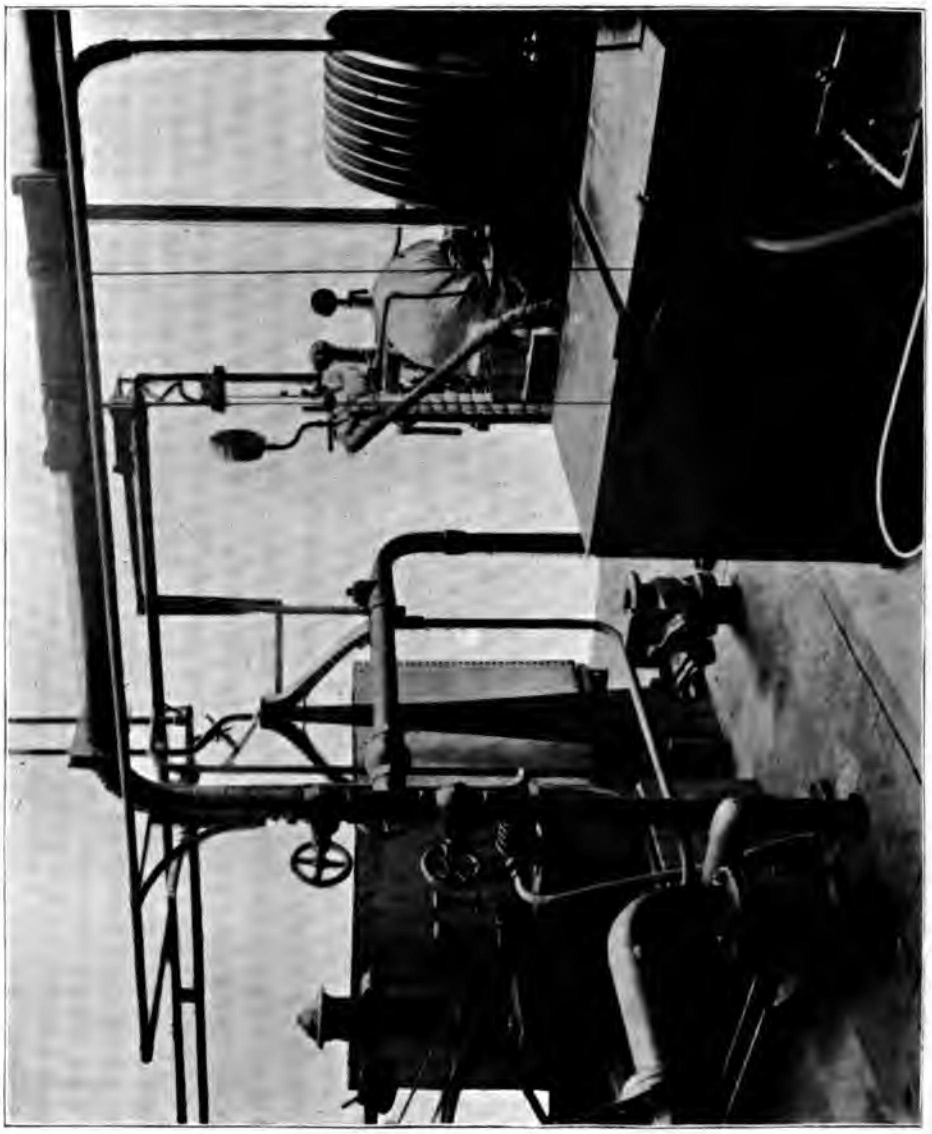


Plate 5

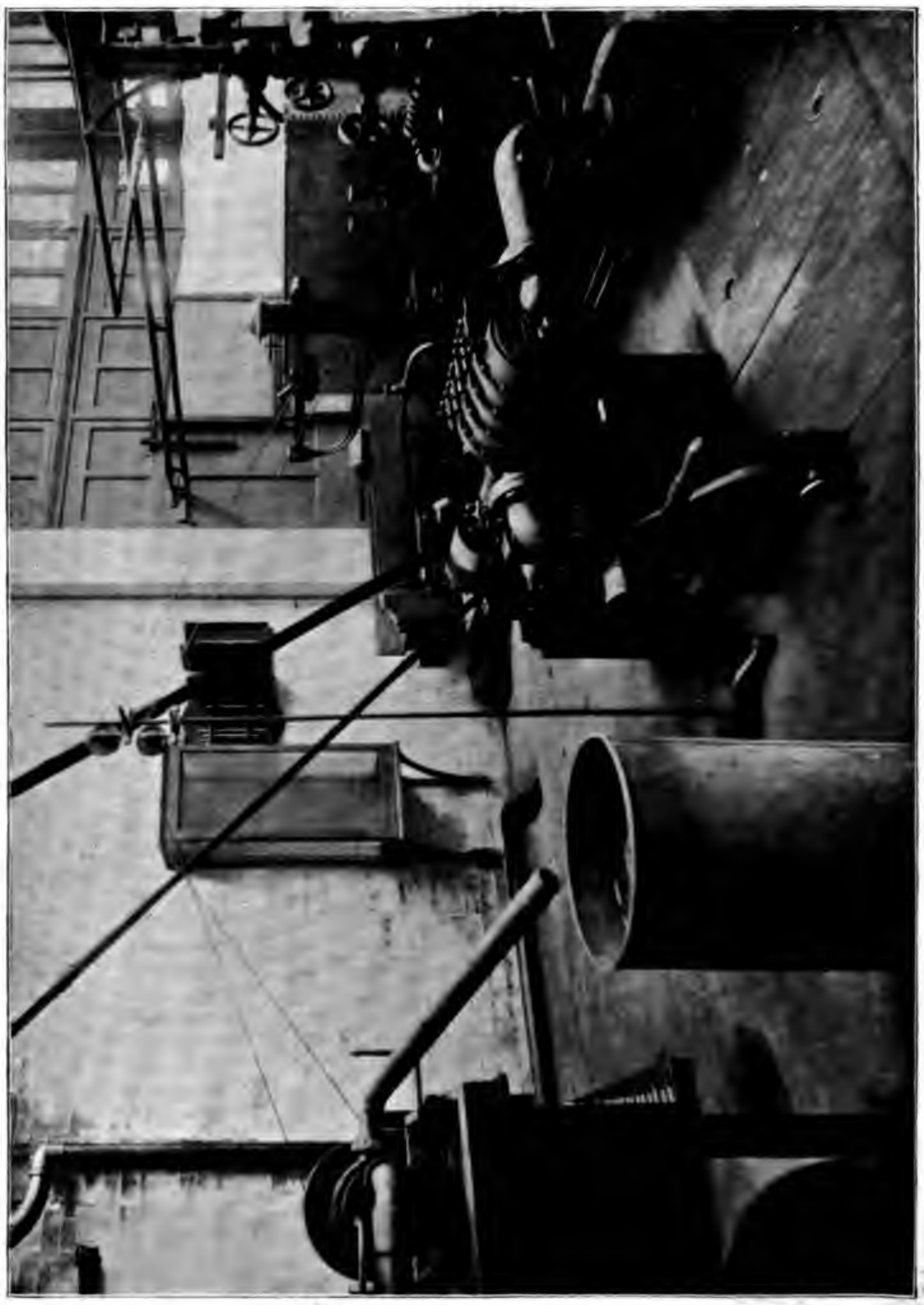


Plate 6